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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Statistics of Chromato-Pseudopsis or Colour-Blindness.* By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.

No formal statistical inquiry embracing a considerable number of individuals has hitherto been instituted into the extent of colour-blindness among the community of any country, and the few limited inquiries which have been made have referred almost entirely to persons of the male sex. The results, however, which have been reached are sufficiently important to demand discussion here.

The first who made any attempt to ascertain the number of colour-blind in the population was Dalton. Besides tracing out among his acquaintances some twenty cases, he observes, "it is remarkable that, out of 25 pupils I once had, to whom I explained this subject, 2 were found to agree with me; and on another similar occasion one."[1] Adding Dalton to his pupils, we shall have in the first case 3 in 26, or nearly 12 per cent. colour-blind, and in the second 2 in 26, or nearly 8 per cent. These are very high proportions, and the parties, according to Dalton's statement, presented colour-blindness in its most marked form.

Professor Pierre Prevost is referred to by Wartmann[2] as having estimated the colour-blind as amounting to 1 in 20, or 5 per cent., but it is not stated how many persons he examined, or what their kind of colour-blindness was. Professor Seebeck "found 5 out of 40 youths who composed the two upper classes in a gymnasium at

[1] Manchester Mem., 1798, p. 39. [2] Taylor's Scientific Mem., 1846, p. 171.

Berlin" the subjects of colour-blindness.[1] The proportion here given is higher than Dalton's highest number, exceeding 12 per cent., but the degree of colour-blindness is not stated, and from Seebeck's classification, as given by Wartmann, it is not probable that all the cases were so marked as those included in the English philosopher's list.

Professor Kelland, of the University of Edinburgh, informs me, that among 150 students attending his mathematical classes during the winter session of 1852–53, he found 3 who could not distinguish red from green. In his own words, they were "thorough Daltonians," and there were several less marked cases which were not included in the numeration.

In the same winter I found among about 20 of my students 2 cases (Mr P. and Mr D.), which will be seen from the record of them, to have been of the most marked kind. On the other hand, among 47 students of the Edinburgh Veterinary College, only one unimportant case occurred, of a party who confounded green with blue.

Among the Edinburgh police, 158 of whom were examined last winter by my friend, Mr Finlay Dun,[2] Lecturer on Materia Medica to the Edinburgh Veterinary College, 5 cases were encountered. One of these confounded full red and green, 2 confounded brown and dark green, and 1 blue and green.

Dr Rowe, of the Royal Asylum for the Insane, Morningside, kindly tested the colour-vision of the male attendants in that institution, and found among 42, 5 colour-blind. Of these, 2 were very decided examples of the confusion of red with green; 1 mistook brown for green, and 2 blue for green.[3]

Anxious to extend the inquiry to a larger number of persons, I obtained permission from the commanding officers of the cavalry, infantry, and artillery detachments in Edinburgh and Leith, to examine the vision of their men, and the results obtained were, in several respects, important. Every facility was afforded by the military authorities, to whom I have here to express my great obligations for the service rendered me.

The investigation was made by daylight, in the latter part of October 1853; and as the scanty leisure accorded to a soldier in a short winter day could not be largely encroached upon, it was impossible, though some hours were devoted daily by Mr Dun and myself, for nearly a fortnight, to the inquiry, to make other than a rapid examination of each individual. Accordingly, as I was mainly anxious to ascertain how far the prevalence of colour-blindness in the community rendered the red and green signals in use on our

[1] Op. et loc. cit.

[2] To this gentleman I am under very great obligations for his willing and efficient assistance in collecting the statistics given in this section.

[3] It was not thought advisable to include in the examination the patients of the Asylum, as the amount of reliance to be placed on their answers could not be determined.

railways unsafe, I confined my observations almost entirely to the difficulties attending the discrimination of red from green, and brown from green; mistakes between other colours being noticed only when they prominently presented themselves; though even then they were not always recorded.

The men were examined one by one; in the first place by asking them to name coloured papers, or the diagrams in Mr D. R. Hay's Nomenclature of Colours. Their catalogue of names, unless they had previously followed trades conversant with colours, was in all cases exceedingly scanty, including in general only the terms red, blue, yellow, green, and brown (which last, however, was often wanting); purple being scarcely ever named, and orange, I may say, never. By requesting them, however, to refer the colours shown them to red, blue, or yellow, all necessity for precise terms, in the case of compound tints, was dispensed with, and the risk was avoided, of confounding those who could not name colours, with those who could not distinguish them.

The majority answered promptly in reference to red, blue, and yellow; purple was generally called either blue or brown, but was nevertheless distinguished from these colours, when shown side by side with them; orange was almost invariably called red, and there was great uncertainty as to the difference between green and blue. Those who gave reasonably intelligent answers in reference to the names of the primary and secondary colours, were dismissed without further inquiry; but if any marked hesitation was shown in distinguishing red from green or brown, they were asked without naming them to assort coloured papers, wools, and pieces of glass, and to place those of the same hue together.

Proceeding in this way, 31 colour-blind persons were found among 437 soldiers of the 4th or King's Own Infantry. Of these, 5 confounded full red with full green, and 1 pink with light green; 13 confounded brown with green, and 12 blue with green.

Among 177 soldiers of the 7th Hussars, 14 were colour-blind; 4 mistaking full red for full green, and 1 pink for light green; 2 brown for green, 6 blue for green, and 1 yellow for pink.

Of the detachment of artillery at Leith Fort, 123 were examined, 5 of whom were distinct cases of colour-blindness, and 2 were doubtful. Of these, 2 mistook full red and green, 1 brown and green, 2 blue and green, and 2 whose cases have already been referred to (*ante*, vol. xviii. p. 41), appeared to mistake purple and yellow.

The list thus given errs by defect, not excess, especially in reference to the confusion of red with green. In so rapid an inquiry as I was under the necessity of making, only the more prominent cases could be detected, and timorous, sulky, obstinate, or unwilling witnesses had frequently to be dismissed, though their cases were suspicious, rather than prolong the detention of a company or troop of their fellow-soldiers, who were waiting to be examined man by man. Those, however, who distinctly betrayed a tendency to confound

red or brown with green, were very carefully examined by Mr Dun and myself together, and the attention of the officers present was directed to the mistakes made, so as to secure us against any attempt at deceit on the part of the men.[1]

The results obtained in Edinburgh in 1852–53 are given in the following table, the two doubtful cases of confusion of yellow and purple occurring among the artillerymen being omitted, as well as the one of yellow and pink among the Hussars; whilst the case of pink and light green among the latter, and the similar case among the infantry, are classified along with red and green.

Each person is put down under a single head, but it is to be observed that those who confounded red with green, also confounded brown with green, and were uncertain as to the difference between blue and green. I have no doubt, also, that several of those who appear only as hesitating between brown and green, were liable to mistake red for the latter colour. Two of the Hussars classified under "red and green" were brothers. None of the other parties, so far as I know, were relatives.

It is further of importance to notice, that although the observations were made at Edinburgh, exceedingly few of the persons examined were born in that city, and very few are settled in it. The soldiers, who form more than half the number referred to in the table, were chiefly from England and Ireland, a few being from Scotland and Wales. The students of the mathematical classes of the University, my own pupils, and those of the Veterinary College, are mainly gathered from the various districts of Scotland, and to a smaller extent from England, Ireland, and Wales. The police are recruited from all quarters, and I believe the same may be said of the attendants in the Morningside Asylum; at all events, the most marked cases of colour-blindness among them, are presented by persons who have come from a distance to Edinburgh. Altogether, then, the parties whose cases are reported represent fairly, though unequally, all the sections of our population.

If to the numbers given in the accompanying table, those obtained by Dalton, Prevost, and Seebeck, be added, the extent to which colour-blindness prevails will appear more markedly; but I omit them, because Dalton's numbers apply to 1793–94,[2] and the cases enumerated by the other observers are not classified. How far any of the results are applicable to the entire community, I shall not attempt to decide; but the number of cases of colour-blindness which have been discovered by every one who

[1] In one case only, I was cautioned, in his hearing, by a sergeant of his company, against crediting the statements of a soldier, as alike unwilling and unable to speak the truth; and although a subsequent private examination led me to believe that he was a *bona fide* case of colour-blindness, the man was so ignorant and eccentric, that I thought it best to exclude him from my list.

[2] Dalton himself lived till 1844, continuing to the end of his days as decidedly colour-blind as he was when he first detected his peculiarity of vision.

EXTENT OF COLOUR-BLINDNESS, AS ASCERTAINED BY THE EXAMINATION OF 1154 PERSONS AT EDINBURGH, IN 1852-53.

Profession.	Number of Persons Examined.	Confounded Red with Green.	Confounded Brown with Green.	Confounded Blue with Green.	No. of Persons Colour-Blind.	Per Centage of Persons Colour-Blind.	Proportion of Persons Colour-Blind as referred to Unity.
4th Infantry, . . .	437	6	13	12	31	7·1	1 in 14·1
7th Hussars, . . .	177	5	2	6	13	7·3	1 in 13·6
Artillery Soldiers, Leith Fort,	123	2	1	2	5	4·0	1 in 24·6
Professor Kelland's Pupils, Edinburgh University, .	150	3	0	0	3	2	1 in 50
Edinburgh Police, . .	158	1	2	2	5	3·1	1 in 31·6
Students of the Edinburgh Veterinary College, .	47	0	0	1	1	2·1	1 in 47
Medical and other attendants, Royal Asylum, Morningside, . . .	42	2	1	2	5	11·9	1 in 8·4
Dr G. Wilson's Pupils, .	20	2	0	0	2	10·0	1 in 10
	1154	21	19	25	65	Average 5·6	Average 1 in 17·7

RATIO OF SPECIAL COLOUR-BLINDNESS, AS DEDUCED FROM PREVIOUS TABLE.

Confound Red with Green, 1·8 per cent., or 1 in 55 ; confound Brown with Green, 1·6 per cent., or 1 in 60 ; confound Blue with Green, 2·2 per cent., or 1 in 46.

has sought for them, shows that the general per-centage cannot be very low. Only two of the cases (Mr P. and Mr D.) enumerated in the table, are described by me in this paper: I have left many known to me undescribed, whilst fresh examples are continually presenting themselves. The experience of Professor Kelland, and Professor Allen Thomson, as to the comparative frequency of colour-blindness, has been similar; M. D'Hombre-Firmas has met with corresponding results in France;[1] and I learn from a gentleman recently returned from Germany, that Professor Dove, in some lectures lately delivered in Berlin on colour-blindness, referred to the unexpectedly large number of persons he had found subject to this peculiarity of vision, although experienced oculists in large practice had never heard of a case.

Further: no fact is better ascertained than that colour-blindness clings to certain families, and is hereditary. With few exceptions, every one of the parties whose cases I have specially recorded in this paper, has near relatives as colour-blind as himself. Thus, Dalton had a brother, and so have Professor L. (whose case I have not described), Dr S., Mr N., and Dr Y., circumstanced like themselves in the matter of vision. Mr X. (whose case, however, is not described), introduced to me by Professor Bennett, of Edinburgh, has five brothers as defective in vision as himself. Mr D.'s father, brother, sister, and nephew, are colour-blind; the Countess of D.'s father, a son, and two nephews, see colours as she does; and Mr S. of London has, or had, five near relatives sharing with him in marked colour-blindness. It seems, indeed, a safe estimate, that every decided case of colour-blindness implies the existence of another case of equal or similar severity in the person of a relative, so that the numbers I have given as representing the proportion of colour-blindness in the community, may fairly be doubled.

For my own part, accordingly, I think it probable that the number of persons in this country as markedly colour-blind as Dalton was, *i.e.*, given to mistake red for green, brown for green, purple for blue, and occasionally even red for black, is not less than 1 in 50; and including all kinds and degrees of colour-blindness, 1 in 20. At all events, the prevalence of this peculiarity of vision is certainly such as to make it an object of interest and importance to the entire community.

In the table above, the number of colour-blind is estimated on the assumption that they are uniformly distributed throughout the community. Such an assumption must be made before any per-centage table of statistics can be constructed, and for many purposes it is a useful fiction. But after, in the case under consideration, it has served the important purpose of showing how prevalent colour-blindness is, it must be set aside, whilst we ask the question, not less important, "In what proportion are the colour-blind divided amongst the population?"

[1] Liebig and Kopp's Annual Report, 1853, p. 131.

Statisticians have made very few attempts to answer such a question, in reference to the idiosyncracies or diseases which they have discussed and tabulated; doubtless because, however important they may deem such queries, they have found it impossible to give replies of almost any value to them. The law regulating the mode in which the totally blind, the deaf, or the like, are distributed throughout the community, must be one very difficult to discover; and it may be questioned whether the discovery would yield a result worth the trouble of attaining it. But in the matter of colour-blindness, considered in connection with the use of coloured signals on railways and at sea, it is scarcely more important to know what the percentage of cases is, than it is to learn to what extent these are concentrated in particular localities, and of more sparing occurrence in others. The hereditary character of colour-blindness, must of necessity keep it running in certain lines of descent, and preponderating in certain families. An instance of this, pertinent to the point, is presented by the 7th Hussars, two of the most marked cases among whom are brothers.

Apart, however, from relationship, as affecting the distribution throughout the community of colour-blindness, it will be seen from the table that the allotment of the colour-blind among the population is, as might have been anticipated, *apparently* most capricious. And hopeless as it is, in our present state of knowledge, to resolve this seeming caprice into the rigid law which doubtless underlies it, one conclusion of very great importance is plainly deducible from the cases recorded, namely, that whilst the average proportion of examples of colour-blindness is high, the extent to which these may become concentrated in single spots is also very remarkable.

This will appear from the following table, in which the results already given are stated more fully, along with certain additional particulars referring, however, solely to the confusion of red and brown with green.[1]

In the following table the same parties are not counted twice, except of course, Dalton, along with his two sets of pupils. Among the cavalry and infantry soldiers, one or two officers' servants, and musicians of the band, are included; and the artillery were taken as they presented themselves on the two days devoted to their examination. Upon the whole, however, each section of persons represents a set of individuals acting for weeks, months, or even years together, as the staff of officials on a line of railway, or the seamen of a ship, who are constantly using coloured signals, are called to do. The evils which might arise in the employment or observation of

[1] The wording of Dalton's statement regarding his second set of pupils included in the table is rather obscure. I have interpreted it as signifying that on both the occasions he alludes to, his pupils were 25, or thereabouts, in number: if his words do not bear this interpretation, it is as probable that they refer to a smaller as to a larger number, for he was not over-burdened with students in his early days.

such signals, by bodies of men containing so many persons unable to distinguish the colours preferred in signalling, will be referred to in the concluding section of the paper. Here I wish simply to present the statistics of colour-blindness apart from any consideration as to the conclusions they warrant.

DISTRIBUTION OF THE COLOUR-BLIND AMONG 1058 PERSONS.

Profession.	No. of Individuals.	Confound Red and Green.	Confound Brown and Green.
Professor Kelland's Students, meeting daily together for 5½ months, . . .	150	3	0
Edinburgh Police on duty together, ...	158	1	2
Dr G. Wilson's Students, meeting daily together for 5½ months, . . .	20	2	0
4th Infantry, Edinburgh Castle.			
a. *Two Companies*, . .	91	1	1
b. *Two Companies*, . .	88	2	4
c. *Two Companies*, . .	86	1	5
d. *Two Companies*, . .	110	2	3
Artillery, Leith Fort.			
a. *Detachment, first day*, .	64	2	0
b. *Detachment, second day*,	59	0	1
7th Hussars, Piershill.			
a. *One Troop, including 2 Officers*, . . .	47	2	
b. *One Troop*, . . .	81	2	
c. *One Troop*, . . .	49	1	2
a. Dalton and his Pupils on one occasion, . . .	26	3	0
b. Dalton and other Pupils on a different occasion, .	26	2	0
Resident Medical Officers of a Public Institution, acting together for several years,	3	2	0

I have no results, it will be seen, to offer respecting the prevalence of colour-blindness among females. I have already stated my conviction that it is rarer among them than among males, but only an extended inquiry can show the amount of the difference in this respect between the sexes.

Opportunities do not largely offer themselves in Edinburgh for testing the colour-vision of an associated number of females, unless in the Charity Schools, where the girls are too young, and, comparatively speaking, too uneducated, to make their examination satisfactory. I commend the inquiry to the teachers of the Schools of Design, and the drawing-masters of educational institutions for ladies, who have it in their power, without putting formal queries, to ascertain with great precision the power of discriminating colours possessed by their female pupils. The surgeons, also, of the large cotton mills or factories could, without much difficulty, examine the bands of female workers under their charge. Their age would atone for any defect in their education, especially in those cases where their avocations led to their dealing with colours.

Reverting, in conclusion, to the colour-blindness of males, it is worth a moment's consideration how far this peculiarity of vision characterises one race of men more than another. It is, doubtless, more common among the civilised nations, large numbers of whom are doomed, by that division of labour, which is a great source of their strength, to occupations which dwarf one or more of the external senses, than it is among the uncivilised races, each member of whom cares only to do what is "right in his own eyes," and cultivates the powers of those eyes to the fullest.

Among both the civilised and uncivilised nations, however, there are doubtless great differences in original endowment, so far as the sense of colour is concerned; and, as may be reasonably surmised, there are corresponding differences in the extent to which colour-blindness prevails among them. Thus, those eastern and southern nations, who live under bright skies, among plants and animals of vivid and brilliant colours, exhibit—partly as a prerogative of race, partly and largely as an effect of such colours daily impressing them—a delight and skill in arranging, matching, and harmonising tints, such as are incompatible with colour-blindness, and imply its rare occurrence in those whose love of colour and command over it are so great.

The Chinese, the Japanese, many of the tribes of Hindostan, the Venetians, the Italians, the Spaniards, the Flemings, the inhabitants of Southern France, and some of the northern Teutonic and Celtic tribes have, as florists, painters, dyers, weavers, glass and porcelain makers and stainers, excelled for centuries sister-nations in the management of colours. Among untutored races, the Indians of the American continent, the African tribes, the uncivilised races of Central and Southern Asia, and the inhabitants of the islands in the Pacific Ocean, have shown by their war-paint, their crowns of brilliant flowers, and still more brilliant birds' feathers, their brightly stained skins and parti-coloured dresses, their dedication of the most splendid coloured objects to their gods and their chiefs, besides much else; that however different their canons of taste may be, they are as passionate and exclusive lovers of colour, as the over-

civilised ancient nations who allowed none but princes to wear robes dipped in the Tyrian dye, or to write with purple ink.

On the other hand, the civilised nations of temperate climes, where the summers are short and the winters long and gloomy, living under sombre skies, amidst a Fauna and Flora of pallid and inconspicuous, or dark and subdued tints, and surrounded by masses of green which satisfy, but do not excite the eye, care little for brilliant colours in their dress or household adornments, compared with the inhabitants of more sunny regions; and probably are more liable to colour-blindness than they.

A similar observation may probably be made, with the deductions requisite in contrasting the condition of the external senses in civilised and uncivilised nations, in reference to such races as the Esquimaux and Fuegians, and especially to the former, who live in regions bereft of vegetation during the greater portion of the year, and presenting to the eye little but the dazzling monotony of ice and snow. The sense of colour must, to a great extent, lie dormant in those so circumstanced, and become dulled through want of exercise. The tribes in question and others in similar latitudes seem very indifferent to colour, as an addition to their dress or ornaments.

The examination of single individuals of a nation, can, of course, settle nothing concerning the endowments of the whole people; but I may mention here, as at least interesting in itself, that through the kindness of the parties themselves, who took a hearty interest in the subject of colour-blindness, I have had the opportunity of testing the colour-vision of Wong Fun, the Chinese student of medicine; of Tiyo Soga, a Caffre student of the United Presbyterian Church; and of the five Turco-Egyptian gentlemen who are prosecuting medicine and physics in Edinburgh at present. Their appreciation of colour is excellent, and certainly superior to that of the majority of our own students, who have not accidentally or designedly made colour a special object of study. The most expert of them all was the young Caffre, an amiable, gentle, intelligent person, who named the colours shown him with great rapidity and precision, although his English vocabulary of colour-names was necessarily limited, and matched Berlin wools and tinted papers with a readiness and unhesitatingness, such as even practised professional colourists might envy.

ARTICLE II.—*Remarks on Quekett's "Histology;" on Kölliker's "Human Histology*, vol. i.;" *and on the Physiological Importance of the Nucleus of the Cell.* By MARTIN BARRY, M.D., F.R.S., F.R.S.E.—(*Continued from* Vol. xviii., p. 528.)

THE fact that secretion takes place in cells, was first announced by Professor John Goodsir.[1] He originally supposed secretion to

[1] Trans. R.S.E. 1842.

be effected by the cell-membrane; but when subsequently becoming acquainted with what he is pleased to term "that most important observation of Martin Barry, on the function of the nucleus in cellular development," he attributed secretion to the nucleus.[1]

Professor Quekett has truly represented each cell of a plant "as having an independent or individual existence," and shown that "in one situation it may secrete *colouring matter;* in another *starch*, *gum*, *sugar*, *oil*, etc.; and in another, the *material for the reproduction of its species*" (*lc.* p. 113). I would, however, add, that it is the *nucleus* of the cell that does all this; one main purpose for which the cell is formed being, I apprehend, the performance within it of the functions of the nucleus, and the resolution of the nucleus into its many forms.

It is now well known that the elements of some tissues consist of caudate, fusiform, stellate, many-rayed, etc., "cells," as they are called. And it is usually said that their processes split up, so that "what was once the cell-wall becomes a mass of fibres." Quekett, *lc.*, p. 129.

I long since investigated with some care the mode of origin of such candate and fusiform bodies in one tissue—the *connective*, more general in its distribution than any other. And having reason from what I saw of the mode of origin of similar bodies in other tissues to believe it to be essentially the same in all, I will briefly recapitulate the process. In the Phil. Trans. for 1841, Plate XXII., figs. 108 to 116, from the tadpole, rabbit, and fœtal ox, there will be found sketched from the connective tissue about fifty bodies exhibiting a transition from the simple discoid nucleus to the caudate, fusiform, etc., bodies in question by the following changes:—1. The nucleus has no nucleolus. 2. It presents a single nucleolus. 3. There are two or more nucleoli, and in some instances many. 4. Nucleoli initiating division of the nucleus into as many parts as there are nucleoli, the nucleus becomes a compound body consisting of smaller nuclei, which however differ in their size. 5. Processes are sent out, giving the compound bodies in question a caudate, fusiform, and other shapes. 6. These processes near their place of origin contain *a row of the smallest nuclei.* 7. Of the processes farther from their place of origin, nothing more can be said than that they defy the microscope, and in my drawings are represented in outline only.[2]

I have been thus particular, in order to afford observers the opportunity of judging whether the substance splitting up into a mass of fibres is not something more than a "cell-wall." What that substance really is, I have just said the microscope fails to show.

[1] "Anatomical and Pathological Observations," by John Goodsir, F.R.S.E., and H. D. S. Goodsir, M.W.S. 1843.

[2] Several German observers, the first of whom I believe was Gerber, have lately seen the true nature of the fusiform, etc., bodies in question, and proposed to call fibres proceeding from them *nucleus*-fibres. Kölliker, however, is not one of these, for he recommends the disuse of the term.

But if nuclei, however small, can be discerned in the processes at their commencement, it does not seem too much to presume the presence originally of still minuter nuclei in their continuations. Now, if the processes are originally filled with nuclei, however small, there can be little doubt that nucleoli perform the same part in them as nucleoli perform in their progenitors, the larger nuclei—*i.e.* that nucleoli effect their cleavage. This cleavage continuing, would soon carry the nuclei beyond the limits of the visible, and the containing processes into a so-called homogeneous state. The nucleus, as I have shown, gives off fibre. Where a mass or fasciculus of fibres is to arise, I have found the nuclei to arrange themselves in parallel lines, as in the young crystalline lens.[1] So that if from analogy we may extend what is easily seen in the lens to fibres of the connective and other tissues, it would seem to be from *cleavage of a line of nuclei* that we get a mass of fibres, and not from the splitting up of a cell-wall.

I have stated the processes near their place of origin to *contain* a row of nuclei. This implies my belief in the presence of a substance holding them together, or rather in which they are contained, proceeding from the investment of the nucleus.

Changes of the same kind as those now described as producing the caudate, fusiform, etc., bodies out of simple nuclei, I met with in other tissues. In the crystalline lens I saw, in the first place, large nuclei arranged with great regularity in lines.[2] In a later stage, such nuclei had become composed of smaller ones.[3] In a stage still later the compound nuclei had elongated, so that the smaller nuclei of which they consisted were arranging themselves *in lines* to form the fibres of the lens.[4] One of these lines is comparable to one of the processes we have been considering. And what I have just said of the material split up, of the way in which it would seem that the splitting up occurs, and of the investment, I would apply here, as well as to the formation and reproduction of all other organic fibre where a splitting up occurs, including woody fibre.[5]

Certain nerve-"cells" furnish examples of bodies having a fusiform, etc., shape, which I believe to be nuclei and not cells. I am desirous of bringing these under the notice of observers on account of the physiological importance of the nucleus, and especially that of the nucleolus. For if such nerve-cells are still nuclei, it is part of a nucleus and not merely cell-contents and a cell-wall that enter into the formation of *their processes*. And if such nerve-cells are still nuclei, the pellucid body they contain is a vesicular *nucleolus*,

[1] Phil. Trans., 1841, Pl. XXIV., fig. 150.
[2] Phil. Trans., 1841, Pl. XXIV., fig. 145 to 148.
[3] Ditto, fig. 149.
[4] This was in a large tube formed by coalescence of embryonic cells. Phil. Trans., 1841, Pl. XXIV., fig. 150.
[5] See my observations on the mode of reproduction of flax. Phil. Trans., 1842, p. 105, and figs. 75, 76, 113, etc.

no doubt continuing to perform its all-important functions, though in what way we have yet to learn, or perhaps may never know.

In the tadpole, I investigated minutely the mode of origin of the epithelium and pigmentum nigrum in the tail, and that of the pigmentum nigrum in the eye; and refer to drawings I gave[1] as showing stages earlier than those of pigment figured by Quekett. My drawings of pigment, I think, explain the mode of origin of what he has well described in stages more advanced.

The "black granules" of Quekett, in pigment, I found to have their origin *in the smaller nuclei*, which the original nucleus gives off in stages earlier probably than those described by him.[2]

Professor Quekett remarks:—"In some plants, and especially in the *Canna bicolor*, the spiral vessels are remarkable for the occurrence of longitudinal as well as spiral fibres." (*Lc.* p. 98.) And he illustrates this by fig. 86, *c*, *d*, *e*, *f*, *g*, *h*. I have already stated my conviction that the spiral fibre in spiral vessels is given off by the nucleus, and, indeed, that all the forms of vascular tissue are referable to altered nuclei. Now, in plants such as the *Canna bicolor*, presenting *longitudinal* as well as spiral fibres, it appears to me that analogy points out the following as the mode of origin of the former. The longitudinal fibres occupy in the vessel a position internal to the spiral fibres. After the nucleus has given off the spiral fibre, its inner part becomes fusiform and splits up into the longitudinal ones, which thus come to lie upon the inner surface of the first.

"A matrix, lastly," says Kölliker, "is found in cartilages and bones as a solid, homogeneous, granular, or even fibrous substance connecting the cellular elements, and for the most part arising from the blood, independently of them." And in an immediately following remark, it is added:—"The occurrence of a solid blastema, growing independently, in the villi of the chorion and of a solid matrix deposited directly out of the blood, demonstrates that all parts of the body are not, as Schwann was disposed to believe, without exception developed from cells or in dependence upon cells. . . . It is certain that in pathological formations such masses very frequently occur, fibrinous exudations becoming changed in great measure, without previous organisation, *i.e.* cell formation, into permanent tissues." *Lc.* p. 11.

This harmonizes with what I have been endeavouring to demonstrate, viz., that the formation of fibre by no means implies the previous formation of what is usually called a "cell." Fibre may form within a cell, for instance in cartilage; it may form, according to me, independently of cells from the fusiform bodies we have been considering; and as Kölliker states, it may form, independently of cells, as "a solid matrix deposited directly out of the blood." But I

[1] Phil. Trans., 1841, Plate XXI.

[2] For such earlier stages see Phil. Trans., 1841, Plate XXI., figs. 93, 90, 91. See also my drawings of the elements of connective tissue, referred to in a preceding note.

must repeat—when fibre forms within a cell, I find it to be the *nucleus* that forms it; when it forms independently of cells from the fusiform bodies in question, it is still, as I believe, a *nucleus* that forms it; and though a solid matrix of fibre be deposited, as Kölliker remarks, "directly out of the blood," independently therefore of cells, and without proceeding from large fusiform nuclei—yet it cannot be shown that what the blood deposits does not contain, derived from the corpuscles of the blood, immeasurably minute nuclei to form the fibres.

Another of the principal facts recorded by me in the Phil. Trans. for 1842 was, *that the muscular fibril, when fully developed and acting, consists of two spirals.*

On the subject of muscle, Professor Kölliker writes as follows:—"As regards the notion adopted by Bowman, Dobie, and others, that the fibrils are constituted of still more minute particles (sarcous elements), it may perhaps be stated, as the study of development shows, that the fibrils do, in fact, at first appear to consist of separate particles. But the question is, whether in the adult such elementary particles continue to be evident, and this, at present, I am inclined to deny." (*Lc.* p. 239.) "To Bowman's opinion, moreover, is opposed the fact, that his assumed 'elementary particles,' except in macerated muscles, where such a thing readily occurs, can only with difficulty be obtained in an isolated form, whilst, according to his view, such a disintegration, in cases where these particles do not cohere firmly, either in a longitudinal or transverse direction, would necessarily take place with equal facility in either." *Lc.* pp. 236–237.

In fig. 36 *b*, Kölliker gives a drawing of a muscular fibril, and remarks: "The fibrils are generally enlarged at regular intervals, so that they appear to consist of a series of many portions, and give a transversely striated aspect to the muscular fibres." (*Lc.* p. 91). The drawing is reproduced by Kölliker in fig. 89 *b*, and he subsequently says that it is still doubtful "whence the appearance of transverse striation in the fibrils themselves arises; whether from their being *spirally twisted* (Arnold), from *zigzag curvatures* (Will), or from varicosities. All that I have seen" (he adds), "leads me to adopt the latter view, which is also that most generally entertained. . . . . Quite lately, Dr Barry has propounded the view that each muscular fibril is constituted of two spirally convoluted filaments running in the same direction. I have seen nothing of the kind, and do not hesitate to describe the whole of Dr Barry's exposition as nothing but a myth, and his figures as fantastic images." *Lc.* pp. 238-239.

As more than a score of drawings in the Phil. Trans. for 1842 show that I had then seen the transverse striæ in muscular fasciculi to arise from *the structure of their fibrils*, I have now, from the foregoing paragraphs, the satisfaction to find, as his fig. 89 *b* also shows, that Kölliker and I on *this* point are agreed.

Regarding the structure producing the transverse striation in the fibrils, however, it is evident that the worthy professor and I differ widely, as he has most unequivocally shown in the last line of the paragraph last quoted. Nor am I at all surprised at this. My experience during the last twelve years enables me to say that Kölliker is but one of very many who have regarded my views on this vexed question just as he does. Perhaps the day may come when he and they will wish they could expunge what they have written; for facts are stubborn things. I do not expect, however, that this will happen until observers have directed their research patiently and in earnest to muscle, where, as in the tail of the *youngest* tadpole, the fibrils are very large. In older muscle, minuteness alone would constitute a difficulty. But here, what mainly complicates the structure is *continued change*—old fibrils, as contractors, worn out, and young ones succeeding—old fibrils passing into that *apparently* homogeneous, structureless state which I have described as constantly seen to be assumed by other fibre; and young fibrils in every phase of quadrilateral, oblong quadrilateral, and rhomboidal particles—and these undergoing not only division and subdivision, but dislocation also, until at length they reach the twisted, and then the twin spiral form.[1] No wonder if old and young fibrils, mixed up in all these phases, present to those who have begun the investigation at the wrong end a structure more ambiguous than any other.

The drawing, however, of a fibril given by Professor Kölliker, figs. 36 *b* and 89 *b*, was taken, it appears, from one of the perennibranchiate amphibia, where, as he says, the fibrils are large, and perhaps "above all others adapted for this investigation." (*Lc.* p. 239). I have already said that that figure shows transverse striæ in muscle to arise from the fibrils. But that figure shows something more than this, and gives proof how well adapted the perennibranchiate amphibia are for the investigation; for, in a German edition of Kölliker's "Histology," that very figure was pointed out to me by Purkinje himself, with a remark to this effect: "Da sind Ihre zwei Spiralen" ("there are your two spirals"). He saw, and I saw with him, that a structure which, nearly every day for months, I had the opportunity of preparing anew, and demonstrating to Purkinje, had been seen and delineated, but not understood by Kölliker.

This observer errs in supposing my views on the spiral structure of muscle to have been propounded "quite lately" for the first time; a proof that, to continental physiologists, what is published in the Philosophical Transactions often remains unknown. It appears that he became acquainted with my views through a paper in Müller's Archiv. Heft vj. for 1850, without observing that what that paper contained was only the account of a *renewal* of inquiries

[1] See a paper of mine entitled, "*A Main Cause of Discordant Views on the Structure of the Muscular Fibril*," in the Phil. Mag. for Nov. 1853.

on a subject, regarding which my views had been recorded eight years before, viz., in the Phil. Trans. for 1842; and he omits to say that the said paper was translated from the English, and communicated to the Archiv. by—a PURKINJE. I appeal to all who have the honour of knowing, in connection with the microscope, the rigorously investigating, most cautious, and circumspect Purkinje, whether there lives the man less likely to be misled by "a myth" or by "fantastic images" than he.

The fibre forming vegetable membrane, was in 1852 delineated by Agardh as having a twin *spiral form*.[1] This in 1842 I had shown to be the very form of the fully developed and acting muscular fibril, as well as the very form of the axile fibre in nerve. And two hundred drawings of mine, then published,[2] show that I had been unable to see any difference between the markings *originally* presented by organic fibre in general, and those of muscular fibrils of about the same size.

THE SPIRAL FORM.

One of my announcements that met with absolute derision was, the presence everywhere of spirals. This fact, I confess, seemed marvellous enough to me, and it was not until, as it were, compelled by accumulated observations, that I ventured to make it known. Now, the observations of Agardh may be referred to as a confirmation.

Yet, is the universality of the spiral form so very marvellous after all? I think not, if we consider the spiral in its most incipient state. Cells, or rather the elements of cells, I suppose are now acknowledged to be universal. By the elements of cells, I of course mean nucleolated nuclei. Why *this* uniformity, we do not inquire. Yet, if elements with such a form are universal, it surely is not so very marvellous that the *first changes* in their form should be the same. Now let us see what one of these first changes is. I refer to many scores of drawings of mine from nature in the Phil. Trans. for 1841, in proof of the frequency with which the nucleus becomes a *biconcave body, or a ring*. Divide a ring at any part, and on lateral separation of the ends, you have an incipient spiral. It was such ring-like nuclei and the crescentic halves thereof, that I saw passing into spirals. There the spirals were of considerable size. But if such be the mode of origin of spirals that can be discerned, does not analogy forbid the supposition that even the minutest spirals have an essentially different mode of origin? So that it being ordered that every structure shall have its origin in bodies of the same form—the elements of cells, it does not seem more mar-

[1] "*De cellula vegetabili fibrillis tenuissimis contexta.*" Lundæ, 1852.

[2] In the 157 *figures* then engraved—Phil. Trans. 1842. The arrangement of many of those figures is not such as to bring together all those of the same tissue, but simply to show identity in the size and structure of parts composing tissues *of all sorts*. Thus, for instance, β in Plate VIII. figures of spirals of the cornea, of muscle, and of nerve, are mixed up with those of vegetable spirals.

vellous that there should be great uniformity in the primary arrangement of the same, especially when it leads to the formation of one thing—fibre. And be it remembered, this arrangement in animal fibre is in nearly every instance as transitory as those elements themselves.[1]

CLEAVAGE IN THE MAMMIFEROUS OVUM.

The first two cells of which, after the disappearance of the germ vesicle, the new being is composed, correspond to what observers of ova of some of the lower animals have termed the first two "cleavage masses." Judging from drawings given by others, I think there is little doubt that the ova of some animals present true cleavage. The best evidence of this that I have met with, is in Nelson's figures of ova of *Ascaris Mystax*.[2] But, because not undergoing literal *cleavage*, does the mammiferous ovum essentially differ from other ova? Certainly not. The difference seems to lie in this: that where literal cleavage takes place, there is an external solid substance of secondary importance left—a true yelk—to cleave; whereas, in the mammiferous ovum there is none.

I adhere to an opinion expressed in the Phil. Trans. for 1839,[3] that in the mammiferous ovum the "yelk," as it is called, corresponds to no more than the substance termed the *discus vitellinus* in the ovarian ovum of the bird. And perhaps in the ovum of the bird a process in that *discus vitellinus* may be hereafter seen, not differing essentially from the process I described as seen in the ovum of mammalia. For the operation of such a process, and for rotation on its axis of such a ball, it may be that in the true yelk of birds there exists a large central cavity of fluid, between which and the surface there is a canal provided for passage and return. This implies my belief that the said *discus vitellinus* in the bird's ovum undergoes as much literal cleavage as the so-called "yelk" in the ovum of the mammal, and no more; that is to say, none at all.

DIFFERENCE BETWEEN NUCLEI AND CELLS IN REGARD TO CLEAVAGE.

"*Cell*-division" is an unfortunate expression. A *nucleus* undergoes true cleavage, never seen, I believe, in the membrane of a *cell*. I think it possible thus in some instances to distinguish a nucleus from a cell, which by other means is often no very easy matter.

This cleavage of a nucleus sometimes takes place within a cell, and sometimes a nucleus is cleft without having formed what can be called a cell.

1. *Cleavage of the nucleus within a cell.*—The nucleus here is cleft either when quite small, or not until it has enlarged; and

[1] In the muscular fibril the twin spiral form is permanent, until this fibril as a contractor is worn out.

[2] Phil. Trans. 1852.

[3] P. 370.

sometimes not until it fills the cavity of the cell. In the latter state the nucleus is the folding in primordial utricle of Von Mohl.

2. *Cleavage of the nucleus which has not formed a cell.*—The circulating red blood-discs of mammalia are nuclei. These are constantly seen becoming segmented. The segments are set free, and separate from one another. This is an example of cleavage into about half a dozen parts.

Size does not enable us to distinguish nuclei from cells. Nuclei may become very large, and yet continue nuclei. Examples of this are afforded, I think, by the protozoa; my belief being that these *are enlarged nuclei and not cells*, the minute pellucid body they contain being a nucleolus which takes part in, and no doubt (like other nucleoli) initiates that cleavage. Cilia-bearing cylinders are, I think, cleft nuclei,[1] like some of the vorticellæ which they resemble also in the position of the cilia. And each epithelium cylinder remains a nucleus.

The very ovum itself, that is to say, the yelk-ball, I conceive to be a nucleus of enormous size. If so, my ovisac is its cell. (Sometimes, as is known, several ova are found in one ovisac;[2] so also are several nuclei sometimes found in an ordinary cell). The germ vesicle, its filling with cells, and the process operating in the early development of the ovum, furnish remarkable examples of the formation and reproduction of *cells within a nucleus*. Farther, the true cleavage of the yelk-ball in certain animals, admits of comparison with the cleavage of other nuclei.

"*Cell*-division," I repeat, is an unfortunate expression, it being only *nuclei* that are cleft. Yet there is but one kind of reproduction, viz., the fissiparous. For, though the membrane of the cell undergoes no actual cleavage, a mother cell does not reproduce without cleavage of its nucleus.[3]

Kölliker seems altogether to deny true cleavage in the ovum. Instead of cleavage, he supposes his primary nucleus, the pellucid centre of the yelk-ball, to "operate as a centre of attraction upon the yelk" and unite into "a globular mass, the first cleavage mass;" and he thinks that then the nucleus becomes resolved into two halves, these acting "as new centres upon the yelk," and thus breaking up the first cleavage mass into two. Each of these has its nucleus, which also becomes resolved into two halves. These in their turn acting as centres of attraction, double the number of yelk masses, thus increased to four, and so on. The editors of Kölliker "altogether demur to the notion that the 'nuclei' of the dividing yelk exercise any *attraction* upon the yelk substance," and for "demonstrative

[1] Phil. Trans. 1841, Pl. XXI., figs. 96, to 100.

[2] This I have elsewhere shown to result from division of the nucleolus before the formation of the cell, and when there exists merely a nucleolated nucleus. Edinburgh New Phil. Journal, Jan. 1854.

[3] See a Paper of mine, "On fissiparous generation." Edinburgh New Phil. Journal, Oct. 1843.

evidence that no such attraction exists," they refer to the observations of Reichert,[1] of Remak,[2] and of Nelson.[3]

It appears to me far from improbable that in the ova which Kölliker has particularly investigated, there may take place no true cleavage. But with his editors I altogether demur to the notion of "attraction" in its stead. I venture to believe that what Kölliker had before him and referred to attraction, was no other than the very process I described in the mammiferous ovum. Thus the "small globules" of Kölliker are the product of my transitory cells. And his "new nucleus, the primary nucleus of the embryo," is what I maintain to result in mammalia from the fecundated centre of the germ spot.

As to membranes being formed *around* the "cleavage masses," by which the latter become actual cells, as supposed by Kölliker, this is certainly a mistake. The very title of a paper by Reichert, mentioned by the editors when demurring to Kölliker's notion of attraction, shows that he denies the formation of a cell *around* the "cleavage masses."[4] And as to "a new nucleus" arising "around a new *nucleolus*," nothing of the kind takes place in the mammiferous ovum, nor anywhere else. Farther, for a refutation of Kölliker's idea, "that the investing globules developed by the successive multiplication of nuclei, become cells only in the latest generations," the process, according to him, being in the earlier stages "nothing else than an introduction to cell-development," I refer to a host of drawings of my own, of what were seen to be *true cells in the earliest generations*. And with regard to Kölliker's declaration, that "the germinal vesicle, in most cases, has nothing to do with it" (the supposed introduction to cell-development), it is sufficient to refer to what Johannes Müller saw in the molluscs developed within *synapta digitata*, where, he says, the germ vesicle *does not disappear, but yields the pellucid bodies in the interior of the cleavage masses*.[5]

CELLS OF THE OVUM COMPARED WITH OTHER CELLS.

I am indebted mainly to my researches on the cells of the ovum for what little I know of other cells. From their large size, the eye could penetrate into the former; but the latter, if seen first, would have yielded only isolated observations, leading to no general results, and by their usual minuteness, the microscope, in most instances, would have been defied.

[1] Müller's Archiv, 1846. [2] Müller's Archiv, 1851. [3] Phil. Trans. 1852.

[4] The title of Reichert's paper is: "Der Furchungs-Prozess und die *sogenannte*," [so-called] "Zellenbildung um Inhalts-portionen."

[5] Müller's Archiv. 1852. S. 19. For other confirmations see the Edin. New Phil. Journal, Jan. 1854. See also in the Phil. Trans. for 1840, my fact, maintained in opposition to all previous observers, that the germ vesicle does not disappear at the period supposed, but persists for a purpose which I there described.

In the mammiferous ovum, the foundations of young cells arise in no other way than by division of the nucleus of a mother-cell; and of myriads of young cells that form, all disappear excepting two.[1] More particularly described, yet still in a very general way, and without mentioning many details, the process is as follows:—The nucleolus becomes nodular, gives off its nodules, and thus segments the nucleus; the segments are given off in a zone. The nucleolus supplies their place by a fresh segmentation, and out of this there arises another zone internal to the first, and so on. The segments are young nuclei. These form cells. So that there arise concentric zones of cells.[2] At length, the inmost part divides into halves, and these two halves become two cells, at the expense of all the previously formed cells, which disappear.

No one can see the first two ovum cells ("cleavage masses") when about to give origin to the nuclei of four,[3] without being struck with the resemblance they bear to the red cells circulating in the blood of batrachians,[4] except that they are not flat. In their elliptical form, in their contents, and in their pellucid nuclei, they are very much the same.

Farther, no one can have made himself acquainted with the process just mentioned, as reproducing the ovum cells, without recognising more or less of that very process in other cells. And this not only in other *reproducing* cells, but in newly formed cells arising for whatever purpose. On this subject, see my papers in the Phil. Trans. for 1841. And in these, see especially the drawings from nature illustrative of the mode of origin of the epithelium tables and cylinders, and the pigmentum nigrum.[5] On comparing these with my drawings of the ovum cells just referred to,[6] the reader will find abundant evidence that the process is the same in both.

ASSIMILATION.

Why, in the development of the ovum, the formation of many cells which for every generation come into existence and speedily disappear by entering into the formation of the more aristocratic two? Why all this cell-life? I apprehend, with Carpenter, that the process is one of *assimilation*. Where cells are merely reproducing cells, their membrane is the wall of a living laboratory raised for the performance of its own functions by the more essential part.

THE NUCLEOLUS.

The ovum cells *absorb*, *assimilate*, and *reproduce*. So do other cells. In these functions the prime mover is the *nucleolus*.

[1] Phil. Trans. 1840.
[2] The interior of these secondary cells presents a process of the same kind.
[3] Phil. Trans. 1839. Plate VI., fig. 105½.
[4] Before these blood-corpuscles have been acted on by a chemical reagent.
[5] Phil. Trans. 1841. Plate XXI., figs. 85 to 100.
[6] Phil. Trans. 1840.

1. The nucleolus *absorbs*. The most remarkable example of this, is the suctorial power manifested by the protrusion of the parietal uncovered nucleolus into the aperture of the vitellary membrane, to take up the equivalent, corresponding, or counterpart, from the spermatozoon.[1] But where is there to be found a cell in which the nucleus is not at first *parietal* in its cell? And where does there exist a nucleolated nucleus in which the nucleolus is not at first *parietal* in its nucleus? Another proof of absorption by the nucleolus is furnished by the originally flat form of the nucleus. The mammiferous blood-discs or nuclei are even biconcave; a provision perhaps for absorption by the nucleolus on both sides. And I have only to refer to embryonic blood, for instance to that of the fœtal ox about an inch in length, for cells whose membranes at the part where the nucleolus lies present, like the germ vesicle, an *orifice*.[2] In mere nuclei, too, it is often seen that at the very part where the nucleolus lies the nucleal investment ceases—*the nucleolus lying uncovered and open to the exterior*.[3]

2. The nucleolus *assimilates*. This is shown in the ovum cells by the germ-spot process. There it is the nucleolus that initiates and completes every change. As I have already said, it becomes nodular, gives off its nodules, thus segments the more or less distinctly vesicular nucleus, and thus initiates the formation of concentric zones of cells. By absorption of the fecundating element, the nucleolus becomes a third substance. This repeats the germ-spot process, though no longer in a parietal part but *in a centre*. It becomes nodular, gives off its nodules, and thus initiates the formation of concentric zones of cells. Such continued *giving off* of its own substance—ever passing as it is from nucleolus into nucleus—implies an equal *taking up*. And what can it take up but that of which it is the centre? This it does take up, and is at length in a condition to reproduce itself by division into halves. What can the purpose of all this be but assimilation? In proof of assimilation by the nucleolus in other cells, I have only to refer to its change from a parietal to a central position in the cell—to the nodular nucleoli—to the segmented nuclei—and to the concentric zones in cells, of which I gave drawings from cells of the lobster, tadpole, cod, chick *in ovo*, rabbit, and fœtal sheep and ox.[4] This continued origin of new substance in the centre, pushing previously formed zone-like substance further out, serves, I think, to show that a nucleolus is ever passing into the state of nucleus. And farther, it seems to

[1] "It is the mysterious centre [nucleolus] of a nucleus which is the point of fecundation, and the place of origin of two cells constituting the foundation of the new being,"—Phil. Trans., 1840. P. 557.

[2] Phil. Trans., 1841. Plate XVII., fig. 28, α, α.

[3] Phil. Trans., 1841. Plate XVII., fig. 29 δ. Plate XX., fig. 63 α, β, γ.

[4] Phil. Trans., 1840 and 1841, and Edinb. New Phil. Journal, 1847. Plate I., fig. 39.

show *how* it is that, as was observed by Remak, the primordial utricle of Von Mohl is a nucleus enlarged.

3. The nucleolus *reproduces*. The cells of the ovum, as we have seen, give origin to daughter-cells. But according to my observations, no daughter-cells arise except through division of the nucleus of the mother-cell. And no nucleus divides until its *nucleolus* has divided. Compare this with a host of drawings I gave of blood-cells, of the corpuscles of pus, mucus, and epithelium, as well as with the elements of the connective tissue, the crystalline lens, and nerve.[1]

The phytozoon I have in a former page stated to be the nucleus of a cell. So that the mode of origin of a spermatozoon and of a phytozoon is the same as that of fibre, when the latter arises in a cell. From what I saw of the mode of origin of cilia also, it appears to be essentially the same.[2] I here refer to stationary cilia. For what are spermatozoa and phytozoa but locomotive cilia carrying about their bulbs?

We may go much farther. Compare, for instance, the young in the ovum of certain intestinal worms with my drawings of the coil of fibre given off by the nucleus in its cell.

Even in mammalia I found the so-called "primitive trace" or rudimental embryo to be no other than the altered nucleus of a cell. The history of this cell is as follows:—Cleavage, so-called, which was known to take place in the ovum of some of the lower animals, I found to take place in that of the highest class mammalia. I have already stated this cleavage to be no other than a process of cell-formation. With every division the cells are doubled in number, and each brood is smaller than the last. At length they become innumerable and very small, and are densely grouped into a mulberry-like mass which occupies the centre of a cavity containing colourless transparent fluid. Of this mulberry-like group of cells I gave many drawings.[3] It expands into the so-called germinal membrane, thus coming to line with cells the zona or vitellary membrane. This expansion reveals the existence of a cell in the centre of the group far larger than the rest, and in some instances having a diameter many times that of the cells around it. This large cell, passing to the surface, comes to lie immediately under the vitellary membrane. The "primitive trace" or rudimental embryo is no other than the altered nucleus of this large cell. Of this cell I gave many drawings, as well as of its nucleus.[4] These show the gradual alterations by which a round nucleus becomes the club-like "primitive trace," or rudimental embryo. They do not essentially differ from the alterations I found to take place in other nuclei, consisting in the

[1] Phil. Trans., 1841. Plates XX. to XXV.
[2] Phil. Mag, Sept. 1852.
[3] Phil. Trans. 1839. Plate VI. Ib. 1840. Plates XXVI. XXVII.
[4] Phil. Trans. 1839. Plate VI. figs. 113, 114, 115, 116 bb.; plate VIII. fig. 148. Ib. 1840. Plate XXVII. figs. 223 to 228, 232 to 234.

formation of zones continually pushed out from a centre. In no instance did I obtain more abundant evidence that changes in and enlargement of the nucleus are initiated by the nucleolus, than was furnished by the very nucleus in question. My drawings show that not only when the nucleus is round, but after it has assumed the club-like form and become the "primitive trace," *there is a continued origin of new substance in the centre* (*nucleolus*), "parts previously situated there," as I said, "having been *pushed farther out.*"[1] Each zone is made up of segments, each segment a young nucleus. Thus the alterations here, as in every other nucleus, consist in continual divisions.

This nucleus, then, being the embryo itself, I conceive that in physiological importance there is no nucleus that can be called its rival. In it we have the main result of that wondrous process of transitory cell-life in which really consists cleavage of the ovum. That process in assimilating spermatozoon and germ-spot, produces the nucleus in question—a third substance, which, thus composed of elements derived from both parents, begins forthwith to be developed into offspring inheriting properties from both. How this inheritance is brought about, we probably shall never know. But there is one thing perhaps not undeserving of notice, which is this: that, as the simplest forms of independent life are reproduced and perpetuate their race by fission, so by fission are reproduced the nuclei composing the most complicated organism; the first of these being, as we have just seen—a union of elements derived from both parents.[2]

[1] Phil. Trans. 1839. Sects 212, 266, Plate VIII, fig. 148; Plate VII., figs. 118, 121 A., 121 B.

[2] It is remarkable that Bischoff, when confirming my observations that cleavage, so-called, takes place in the mammiferous ovum, and leads to the formation of a mulberry-like group of cells, should have failed to see in the centre of the group a cell larger than the rest—its nucleus the important body now referred to—the very embryo itself. He saw, as it were, the nut-*shell*, but did not see the kernel. Perhaps the day may come when Bischoff will see the latter too. For he has lately contributed to the establishment of a fact of cardinal importance, which for a dozen years he had joined others in denying—the penetration of the spermatozoon into the interior of the ovum. This fact I recorded in the Phil. Trans. of the Royal Society of London for 1840; established it by further observations published in the Phil. Trans. for 1843; and in the Edinburgh New Philosophical Journal, for October 1843, gave a drawing of an ovum of the rabbit, containing in its interior nine spermatozoa, with the statement that I once counted more than twenty in an ovum of this animal. These facts of mine were neglected or denied until 1852, when in several of the lower animals they were confirmed by Nelson and by Newport in this country, and especially by Keber in Germany, the paper of which indefatigable observer, it seems mainly to have been that induced other physiologists on the continent to enter upon renewed inquiry. At length, in 1854, spermatozoa were seen within the zona of the mammiferous ovum. This was done by R. Wagner, Henle, Baum, and Meissner, and even by Bischoff himself. And the latter, in a paper dated 25th March 1854, now candidly acknowledges, that in denying the penetration of the spermatozoon into the interior of the ovum, he had done injustice to Barry, the mistake having been his own. There are other things that Bischoff

Expressed in the fewest words, my leading observations brought together in this communication are as follows:—1. The nucleus neither remains inert, nor is absorbed as useless. 2. It is a nucleus that contains the fecundating element. 3. It is a nucleus that receives the fecundating element. 4. Certain phenomena are presented by the nucleus. 5. These occur in a certain order. 6. They occur apparently in all nuclei. 7. They seem to imply absorption, assimilation, and secretion by the nucleus. 8. They are attended by continual cleavage. 9. They thus lead to reproduction of bodies having the same form—that of a nucleus. 10. And they thus lead to the production of parts which are so numerous and so different that all other parts may be supposed to have their origin in nuclei. 11. The prime mover in all these changes is that which has been called the *nucleolus*, the part which in mammalia receives the element of fecundation.

While in thus maintaining the importance of the nucleus to be beyond that of cell-membrane and "contents," I differ from Schwann as to what is the essential part of the cell, it will be obvious that, so far from tending to lessen the physiological importance of his great discovery, my facts add proof thereof. And farther, it will be seen that these facts of mine, locating as they do the prime mover in the nucleus, are about as directly opposed as possible to the views of a recent author of great talent, who conceives the value of the cell-theory of Schwann to be purely anatomical.

ARTICLE III.—*Contributions to Medical Jurisprudence.* By DOUGLAS MACLAGAN, M.D., F.R.S.E., etc.

Imputed infliction of injury upon a person asleep.

THE precognitions in the following case were submitted to me by crown counsel, for my remarks and suggestions in reference to the mode in which it is likely that the injuries were inflicted. As the essential facts of the case are recapitulated in the subjoined report, I have little to add by way of introductory narrative. The accused party was a tradesman in a distant and isolated part of the country. His wife, the injured party, had made sundry complaints to the elder of the church in the district in which she resided, that her husband had not been kind to her of late. She seems to have entertained

has yet to learn, concerning which he wrote that "Barry had seen too much." I have just mentioned one of these—the existence in the mulberry-like group of a cell larger than the rest with its all-important nucleus. Of other facts of mine, still denied by Bischoff, may be mentioned the following, viz., the escape of the ovisac from the ovary; the existence of bodies truly deserving the name of retinacula from the office they perform in regulating the position of the ovarian ovum of mammalia; the persistence of the germ vesicle beyond the period when it had by all been supposed to disappear; the part taken by the germ-spot in the production of the new being; and the fact that cleavage in the mammiferous ovum is effected by means of cells.

some jealous feeling of a young woman, who was a relative of her husband, but lived with them in the capacity of a domestic servant. There was no accusation of conjugal infidelity, and otherwise he seems to have been a person of good character. The lay office-bearer of the church above alluded to, however well intentioned, appears not to have been a very judicious adviser. Crediting the woman's complaints, and apparently not taking any measures to ascertain how far they were well founded, on the husband applying to have his name put on the list of those intending to join in the communion services, he had thought proper to advise him not to appear as a communicant. In a small and detached parish like the one in question, this was sure to attract attention, and it is to be borne in mind as bearing upon part of the subsequent conduct of the accused man. It was subsequently to this that the alleged injury was inflicted, the date being 26th October. The story, emanating entirely from the wife, soon got abroad, and reached the ears of the procurator-fiscal of the district, whose business it was to make such offences the subject of judicial inquiry. Preliminary investigations were made during the month of November. On the 21st of this month, the accused very foolishly went away from home, took refuge in the house of a brother, and then put himself on board of a coasting vessel bound for another part of the country. But if he seriously intended a flight from home he did not put it in execution. It appears that previous to the sailing of the vessel some one went alongside in a boat, and said that word had been sent to the place to which the vessel was bound, to capture him on his arrival. He therefore returned to his brother's house, and remained there till he was apprehended. The flight naturally confirmed the local authorities in their belief that there was some foundation for the charge; consequently on 1st December a petition, in the ordinary form, was presented to the sheriff, for a warrant to apprehend him, and the inquiry was thenceforward conducted in the usual way, the offence charged against him being designated by the public prosecutor "stellionate and real injury."[1] On the case being reported by the local authorities to crown counsel, it was thought proper, before proceeding farther, to ascertain if possible what was the probable mode of infliction of the alleged injury, and the whole precognitions were, for this purpose, placed in my hands. The result of my report was to lead crown counsel to put a stop to the proceedings, and set the accused free.

[1] Stellionate, a phrase not in very common use, is employed in Scotch law to designate forms of offence against the person which are not comprehended under any special *nomen juris*. The term is borrowed from the Roman practice, and seems to imply stealthiness in the mode of inflicting the injury. See Hume on Crimes i. 328, where a case is quoted, similar to the present, viz., applying fire to the privy parts. In English law it has a very different signification, being nearly synonymous with swindling. The etymology is "*stellionatus ita dictus a stellionis natura, quo nullum animal fraudulentius homini invidet.*"—Ainsworth.

By this negative process, which was judiciously adopted in preference to any farther inquiries, in order to avoid heartburnings and strife in a limited locality, we are, of course, prevented from positively ascertaining, by confession of the accuser or otherwise, whether or not my opinion, that the injury was self-inflicted, was correct. I submit to my professional brethren, as an instance in which (whether rightly or wrongly, I leave them to judge), medical jurisprudence, whose stern duty it is to aid in convicting the guilty, enjoyed the more agreeable privilege of setting free a person believed to be wrongly accused.

Edinburgh, 20th February 1854.

I have perused the documents in precognition against John Smith, which have been submitted to me for my remarks and suggestions in reference to the mode in which it is likely that the injuries were inflicted.

In order to save repetition in stating the grounds on which my opinions are founded, I beg to recapitulate shortly the narrative of the case as set forth in the documents.

On Wednesday, 26th October, Smith and his wife slept together. The woman states that she was quite uninjured when she went to bed. The husband lay down first, she followed in a few minutes. There was a fire in the room at the time they retired to rest.

On this night, apparently about two and a half hours before daylight, Mrs Smith states that she awoke and screamed with a pain, as if she had been burned on the upper part of her left thigh. She and her husband arose soon after, procured a light, and examined the part. She states that she saw a red line then, and that her husband discerned nothing; but he in his declaration states, on the contrary, that he did see a mark, that the skin appeared singed, and had a whiter appearance than natural. Nothing was applied to the injury at this time, and both returned to bed, though there is some contradiction as to whether they did so immediately or not. After daylight, apparently about half-past eight or nine o'clock, Mrs Smith showed her injury to Mary Mackay, who had slept in the room above with the servant, Jessy Thomson. This woman Mackay saw the injuries, and states that they appeared "quite black;" but according to Mrs Smith they were only a little darker than before. The skin was not broken except at one little point. Mrs Smith admits that, in describing her sensations to Mackay, she stated that she felt as if boiling water had been poured over her, but subsequently, in describing the same thing to Alexander Johnston, she said that she felt as if a finger had been drawn lightly over the part. The scream, which she states she uttered when she awoke, was not heard by either of the women sleeping up stairs, although the flooring of their apartment was only joists with deal boards laid over them, and Mackay's stating this fact called forth a querulous remark from Mrs Smith as to their sleeping very soundly.

On Friday, 28th October, Mrs Smith went to William Wilson, the elder of the district, to whom she had complained previously of want of affection on the part of her husband of late, and to Wilson she made known the injuries she had received two nights previously. She showed her injury partially to Wilson, but more fully to his wife, who describes the injury as bearing the appearance of having been inflicted with a hot iron.

Mrs Smith gave to Wilson a paper, which she found in her bed, containing a white substance, which was preserved with due formality by Wilson, and which, by the analysis of Dr ——, was subsequently shown to be sugar of lead; this, though perhaps hardly requisite, I have confirmed by analysis. It may therefore be summarily dismissed as having nothing to do with the present question. On this same Friday, 28th October, Alexander Johnston,[1] who had been sent for by Smith, saw the injury, which he describes as "commencing at the haunch-bone, on the left side, and extending down along the groin and half across the private parts, and also a similar mark on the outer edge of the private parts of the opposite side, and a spot upon her right thigh. The wound, or sore, had the appearance of having been inflicted with a hot iron. The mark was scarcely a finger-breadth, but the skin was all shrivelled, and had quite a burned like look."

Two days afterwards, Mrs Smith sent for Wilson, and asked him if he had quite forgotten her, said she was much worse, and that if she did not get medical aid she would die. It does not appear that she had made any such alarming statement to her husband. Wilson had taken Alexander Johnston with him on this occasion. From the description of the latter, the sores appear by this time to have inflamed and suppurated, and Johnston thought them alarming.

At this interview, "on her pressing entreaty," according to Wilson's statement, but, according to her, on Wilson's "repeated entreaty," and only with reluctant consent on her part, Wilson promised to procure medical aid for her. That same evening the husband, who seems now for the first time to have learned that the injury was thought serious, remonstrated with Wilson for taking upon himself to send for a doctor without consulting him (Smith).

Smith appears, at or about this time, to have asked the aid of Miss Simson, who is reputed skilful in burnings and festerings; but after seeing Wilson he wrote to a doctor at ———, who sent some medicine. Mrs Smith now sent a message to Wilson not to send for a doctor, but to come and speak to her next day, but he "declined being made a fool of any longer, and did not go near her."

The next account of the sores which we have is from Dr ——, the only medical man who seems to have seen them. This was, apparently, after legal proceedings had been commenced in the case,

[1] An intelligent gardener, who to his horticultural avocations conjoined a decided taste for phlebotomy, and other minor surgery.

viz., on the 18th November, twenty-three days after the infliction of the injury. Dr ——'s description is as follows:—

"I found four superficial ulcerations in the left groin, and one in the right groin. The longest of these was about ten inches, and varied from about a quarter of an inch to half an inch in breadth. The others were from about two to five inches in length, and from a quarter of an inch to an inch in breadth. There was some slight inflammation on the right and left labia, and slight excoriation, external to right and left labia, extending nearly to anus. These injuries were all undergoing the healing process."

The circumstances connected with Smith's leaving home do not require any recapitulation by me.

These are the facts deponed to in the case on which I am now to express my opinion.

It is *prima facie* evident that this injury could have resulted only from the application to the part, of some sort of cauterant, which term includes both chemical caustics and heated bodies. The history of the case shows that it could neither have arisen spontaneously, nor been produced by mere mechanical violence.

The chemical caustics include both liquids and solids. Of the latter, some are deliquescent, *i.e.*, they attract moisture, and tend to pass into the fluid state when exposed to the air; and these, in relation to the present inquiry, may be placed in the same category with the liquids. The only substances worth noticing as having been in Smith's house, are copperas (sulphate of iron), and hartshorn (aqua ammoniæ); the former does not injure the skin when applied to it, the latter will blister but does not burn.

Setting aside any argument as to the improbability of the accused party having in his possession any of the liquid corrosives, or of the caustics used by surgeons, it appears to me that the facts of the case are sufficient to warrant the conclusion that no caustic, liquid or solid, could have been applied here. The lines of injury are long, narrow, and straight, and it would be almost impossible, with every precaution, and with the full consent of the suffering party, to produce this effect with a liquid, or deliquescent solid caustic. In whatever position her body was placed at the time of application, it must have spread more or less, and could not produce an injury of the linear form, as described by the various deponents.

The descriptions given by the various witnesses are not very explicit or accordant with each other; Alexander Johnston speaks of only one long line resembling a burn; Mrs Wilson talks of the same line and of a shorter one, parallel to and below it; and Dr —— describes four ulcerations on the left groin, which, from their situation and linear shape, must have been in parallel lines. It seems not improbable that two lines of injury had, as Mrs Wilson describes, been made at the same time, that only part of the lower one had suppurated and ulcerated, and that thus the groin presented one long sore and three shorter ones when seen by Dr ——. Be

this as it may, they all describe the injuries as being of the linear form, and this is the great objection to accepting the theory of their having been produced by caustic. It is clear that the cauterant, whatever it may have been, had, after running along the left groin, touched the woman's private parts externally on both sides, and also slightly touched the right thigh or groin. Had it been a fluid caustic its course would have been different. Having been applied, as it obviously was, to the left groin, it would have flowed down between the left thigh and left *labium majus*, and would have cauterised this part as much as, if not more than any of the rest; but it is clear from Dr ——'s account that there had been no cauterising of this part, for he speaks only of slight excoriations on the outside of both labia, tending backwards to the anus, which are much more likely merely the result of the irritation from the matter, etc., of the sores. Moreover, in passing across the orifice of the vagina, from labium to labium, a liquid caustic could hardly fail to have passed inwards and injured the more delicate mucous membrane, and this, from the absence of all notice of it in her own or Johnston's account, had not occurred.

The solid caustics which would not spread by deliquescence are only two, quicklime and nitrate of silver. The former, in order to produce any cauterant effect, would require to have been in contact with the part for a length of time; the latter must have been rubbed on with considerable firmness for some time also, and even under these conditions they are hardly strong enough to produce such a result. Further, there are two circumstances which render the application of any caustic more than improbable; 1st. That no foreign matter, liquid or solid, was found by Mrs Smith on her person when, as she states, she awoke with the pain of the injury. 2d. That there is not the least trace of any corrosion or staining of the body-clothes or bed-clothes, which, had any caustic been applied to a woman in bed, must have to some extent occurred, and must have attracted attention.

The only agent, therefore, capable of producing this injury is, I conceive, the application of a red hot body. There was a fire in the room when the Smiths went to bed, and a portion of burning coal, stick, or peat, might have produced such a burn. But with any of these there must have been either black carbonaceous matter or ash left on the injured part, whilst all which Mrs Smith admits herself to have seen, was a red mark; her husband says a whitish one. It appears to me that the only article which could have been used here was a hot piece of metal of some sort.

The theory of those who accuse the husband of the assault, must be, that he had by a rapid stroke burned the parts while his wife was asleep in bed. To do this, without singeing any of the clothes, is a feat of dexterity which is, to say the least, extremely improbable. But in order to prove that he did it so as to burn her without burning any of the clothes, we must adopt all the following improba-

bilities:—That he, having been in bed before his wife, got out of it, procured the heated iron, carried it to her in bed, raised her clothes, inflicted a burn more than ten inches in length, replaced the clothes, got rid of his weapon, returned to bed, and pretended to be asleep, without rousing the woman to a consciousness of anything more definite, than that she had, according to one of her stories, felt as if hot water were poured over her; according to another, as if a finger had been lightly passed along her person.

I think it needless to pursue this argument any further: it is to my mind perfectly clear that this burn was self-inflicted—that the hand of Mrs Smith herself drew a hot iron over her person, and produced the injuries in question; and the following additional considerations corroborate this view.

1. The line of injury which obviously ran from left to right, affecting only a part of the body easily accessible to the party herself, and in a direction which might be expected if she had been applying to her own person a hot iron, such as a poker—unless it should appear that Mrs Smith is left-handed.

2. The superficial character of the burn, evidently the result of a rapid stroke, more like the work of a person anxious to produce an injury which might be shown and complained of, but which would not be dangerous or serious; not like that which would be inflicted by an assailant who could make up his mind to commit an offence of so brutal a nature. The length of time required to heal the sores is not unusually great, and does not indicate that the injury was a severe one of its kind.

3. The evident disposition on the part of the woman to make the most of her complaints to her friends and neighbours, as witnessed by her taunt to Mary Mackay for not having heard her alleged scream during the night; her complaint to Wilson on or about the 31st of October, that he had forgotten her, and that she would die unless she got medical aid. In contrast with this, it is to be observed that she never made any complaint of the kind to her husband. It is true that she might be supposed to be averse to holding any communication with him on the matter, if she believed him to be the inflicter of the injury, but then, on the very same evening, she puts herself unreservedly in his hands for the procuring of medical aid—is contented with letting him write on her behalf to a doctor at a distance, instead of getting a medical man to see her, though she had just said to Wilson that she thought she would die—and she readily dispenses, that same evening, with Wilson's services—a circumstance which seems to have led him to the not unreasonable conclusion, that he was being "made a fool of."

4. The part of the body injured quite accords with the theory of self-infliction. Mrs Smith, if we may trust the girl Thomson's account of her, "was generally in bad health, and somewhat peevish in disposition." I am inclined to suspect that she is probably hysterical, and she was evidently acting under a morbid jealous feeling

towards this girl, who was at once a servant and relative of her husband. It is well known to practical medical men, that women of this constitution very often present themselves as labouring under strange forms of disease, almost invariably involving the generative or urinary organs, not unfrequently subjecting themselves to much pain, with no other discoverable object than that of deceiving their friends or medical attendants. There is, therefore, nothing unaccountable, or even novel, in this woman injuring herself in the way which I believe her to have done.

5. The absence of all marks of injury on Mrs Smith's hands is much against the supposition of infliction by another party. When we remember, that even gentle titillation of a sleeping person, generally causes them instinctively to put up their hand to the part touched, it seems almost inconceivable that, when a burn of this sort was inflicted, the woman, even though not roused to her full senses, should have failed to put down her hands and grasp the weapon or hand of her assailant, and have received some amount of injury on his hurriedly withdrawing it. Supposing it to have been a corrosive applied during sleep, she must equally have put her hand to the part when it first began to smart, and her hands must have borne some marks of the caustic.

Insensibility from drunkenness, or some such cause, of which there can be no suspicion here, would alone account for an opposite result.

It perhaps hardly falls within my province to comment on the moral evidence as regards the accused party, but as the whole depositions have been placed in my hands I cannot refrain from observing, that his conduct during these occurrences, appears to me to be straightforward and manly, with one remarkable exception. I allude, of course, to his attempt to escape from the locality by the coasting vessel. But I respectfully submit that when we consider how he was placed—that he had been prevented from enjoying church privileges by the elder of the district acting upon the jealous complaints of Mrs Smith—that he had at length been made the subject of an accusation of a disgraceful and cowardly assault upon his wife—that in such a community he had become a marked and degraded man—we may without much difficulty accept his explanation, "that he was ashamed to show himself amongst gentlemen on account of the stories raised against him," and that he was glad to escape on any terms from a home which had thus been rendered miserable to him. There is little doubt on my mind that but for this foolish, ill-advised act on Smith's part, the criminal proceedings against him would never have been heard of.

The remit of crown counsel bears that these papers are submitted to me for suggestions as well as remarks. I beg, therefore, to offer the following: —

1. Let inquiry be made as to whether, on the night in question, Mrs Smith slept on the right or left side of her husband, and whe-

ther he or she be left handed. Seeing that the husband lay at the front of the bed, and that no one could get access to it from the other side, it is not improbable that by duly ascertaining the relative positions of the parties during sleep, some facts may be elicited which would either confirm or invalidate the opinion which I have expressed above.

2. If consistent with legal practice, let Mrs Smith be re-examined *in extenso*, let her give her account of the whole proceedings, and thereafter, she having been warned that this has been under the review of persons accustomed to judge of such matters, let the question be directly and decidedly put to her, "did she not herself inflict the injury on the night in question, and in the manner supposed by me, shortly before going to bed?" In cases of feigned disease or injury, a question of this kind, after a course of inquiry tending in quite an opposite direction, will frequently extract the truth or make falsehood apparent.

[See the case of Elliot Millar, imputed poisoning, reported by me, Monthly Journal, Dec. 1848, p. 384.]

ARTICLE IV.—*On the Remedial and Anæsthetic uses of Intense Cold.*

By JAMES ARNOTT, M.D., London.

ALTHOUGH the subjects of the remedial efficacy of congelation and local anæsthesia from cold have been for some years before the public, they are as yet little understood and appreciated. This has resulted, partly from their having been imperfectly explained, in consequence of the publications respecting them being severally incomplete, and partly from the strength of the prejudice against extreme cold. Dr Rowley, who, in his attack on cow-pox, declared that the accounts which he had heard of the terrible effects of communicating the "cruel and beastly" disease were enough to "freeze the soul," was not probably more horror-stricken than some have been by the proposal to freeze the body;[1] and the introducer of vaccination was hardly more abused than the proposer of congelation has been. It is in the hope that this prejudice may be thereby abated, and the subject rendered better understood, that the following brief statement is published. Even in France, where both the remedial and anæsthetic uses of intense cold have been turned to account for some time by M. Velpeau and other leading practitioners, there is still much doubt about the best mode of applying the agent. In a paper in the *Bulletin de Therapeutique*, of the 15th ult., M. Richet, Surgeon of the Hospital Saint-Antoine in Paris, reports thirteen operations in which local anæsthesia had been produced by the very imperfect means of the quick evaporation of ether.

[1] See an extract from his pamphlet in the Monthly Journal of Medical Science for September 1847, p. 147.

As no remedy has been longer in use, and few are more valued than the local application of moderate degrees of cold, or a temperature ranging from that of dissolving ice to about 70° of Fahrenheit, it may at first appear singular, that a greater or more powerful remedial effect should not have been sought by increasing the dose of the agent, or employing a lower temperature, in the same manner as we have sought and found much greater remedial benefit in many cases by using mercury, antimony, quinine, and other drugs, in larger doses than had been customary. The reason is that medical men were under a most erroneous impression respecting the effects of very low temperatures on the body. Because a temperature of zero stops the circulation, and because the vitality of a part has been lost by its *long-continued* congelation, whether caused by exposure to severe cold in winter, or by the incautious use of ice in hernia and other diseases, it was hastily and erroneously inferred that there was danger of loss of vitality from *short-continued* congelation. The mistake would not be greater to infer from the fact, because a long continued stoppage of the circulation through a limb from an improper application of a bandage has occasioned gangrene, that it would be dangerous to use the tourniquet in operations.

The correction of this error will be deemed of no little importance when it is considered that in short continued congelation, judiciously applied, we have an unfailing means of immediately arresting inflammation wherever it can be reached by the remedy; of not only giving speedy relief from pain in many diseases, but in consequence of the organic changes produced by it, of obviating the return of pain; and in malignant disease, of producing an amount of benefit much exceeding that yet accomplished by other means. Although much inferior in importance to these results, it is yet another great benefit conferred by intense cold, that the pain which would be otherwise caused by the greater number of surgical operations can be prevented by it with perfect safety; and not only can pain be prevented, but the inflammation proceeding from the surgeon's knife, that so often proves fatal, may also be obviated by the same means, and with almost equal certainty. It will be proper to consider the remedial and anæsthetic effects of intense cold separately; but before doing so, it is necessary to mention how this degree of cold is produced and applied, as well as to attempt an explanation of its mode of operation.

That degree of cold may be called intense which immediately benumbs the part to which it is applied, speedily stops the circulation through it, and congeals the adipose matter. I have usually produced these effects by placing what are termed frigorific mixtures, either immediately in contact with the skin or mucous membrane, by means of a net of thin gauze containing them, or by allowing them to act through thin bladders, or metallic vessels of appropriate form; but there are various other ways of effecting the

same object, some of which are preferable for certain purposes. Substances passing rapidly from the solid to the fluid, or from the fluid to the aeriform state, strongly abstract caloric from other bodies in contact with them; and substances, either solid, fluid, or aeriform, already sufficiently cooled by artificial means, may be placed in contact with the part; the first, as solid metallic balls of appropriate shape; the latter two, when forming strong currents. When cold is produced by the common frigorific mixture of ice and salt, and applied by means of a gauze bag or net, the following is a convenient mode of proceeding:—If the congelation is not to be extensive or long-continued, a piece of ice of the size of a large orange will be sufficient. This is well pounded in a coarse cloth or bag, and the powder being placed upon a large sheet of paper, is thoroughly mixed, by means of a paper-folder, with about half its weight of common salt. The mixture is then put into a net of about four inches diameter, and as soon as it begins to dissolve, it is ready to be applied. The net is not kept motionless on the part, but is frequently raised in order that fresh particles of the mixture may be brought into contact with the skin; and the water that escapes from it may be absorbed by a sponge, or allowed to fall into a basin placed underneath. If the surface to be acted upon is of small extent, a very thin and large copper spoon containing the mixture, or a solid brass ball of about a pound weight, which has been immersed in ice and salt, will often answer, and be a neater mode than the net.

The moment a gauze net, or a thin metallic vessel containing ice and salt, is applied to the skin, it is benumbed. There is hardly a sensation of cold produced, and no tingling or smarting. If the contact of the frigorific be continued a few seconds longer, the surface becomes suddenly white, in consequence, doubtless, of the arrest of the circulation; and this change of colour is attended with a slight smarting like that produced by mustard. There is now complete anæsthesia, which, if the frigorific were removed, would remain complete for several minutes. But if the frigorific be allowed to act, another change is produced—the adipose matter under the skin is congealed, and the part becomes hard as well as white. The depth to which the benumbing influence of cold will extend depends upon a variety of circumstances, as the degree of cold, the duration of the application, the vascularity of the part, whether pressure is used or the circulation is suspended, etc., etc. After the usual application of cold for anæsthesia, the circulation soon returns to the part, and the skin assumes a red colour which lasts for several hours. If the congelation has been considerable, there is now some smarting felt, unless the natural heat be more gradually restored by pouring cold water on the part, or by placing on it a little pounded ice, or a bladder containing iced water. If the application has not exceeded the first stages, there is no smarting, and no necessity, therefore, for such precautions.

The redness produced does not, as might at first sight be supposed, indicate an inflammatory condition, but the very reverse. The tonicity of the small arteries appears to be lessened or suspended for a time, and instead of being inflamed, the part is rendered unsusceptible of inflammation. Parts cut after congelation heal by adhesion or the first intention more quickly than they otherwise would; and, as has already been said, we possess in this expedient a certain and prompt remedy for every inflammation accessible to its complete influence.

I. *Remedial uses of intense cold.*—The remedial qualities of intense cold may be described as antiphlogistic, anodyne or sedative, and specific; and it is useful in the diseases for which other remedies possessing these qualities have been employed, viz., in inflammatory, painful or irritative, and malignant diseases. The circumstance which limits its application in these, is the impossibility of extending its influence beyond a certain extent or depth, although it is certain, from its effects in deep seated disease, that this influence, whether it be direct or sympathetic, is more extensive than would at first be supposed. It may be laid down as a rule, that in every case in which the local application of moderate degrees of cold has been found of service, the use of well-regulated congelation would prove much more useful; and in those diseases of similar character, in which moderate cold has not been employed from the idea that their seat was beyond its reach, congelation might be tried with reasonable hope of success. Intense cold has this immense advantage over other powerful remedies of the same class, that it may be used with impunity—if it does no good it will do no harm. Who will venture to affirm this of bleeding, mercury, antimony, opium, chloroform, arsenic? Neither in my own practice, nor (so far as I can learn) in the practice of others, has there been any untoward result from the use of congelation. Its action being confined to the diseased part, and not uselessly expended on the rest of the system, affords the explanation. Other topical remedies have much the same character for safety, but what other expedient of this class has a tenth part of the power of intense cold?

Instead of enumerating the diseases in which this agent has been employed according to the above classification, I shall mention, first, those in which it has been more or less successful; and, secondly, those in which it might, reasoning from analogy, be tried with hope of advantage. In administering intense cold as a remedy, the common or a more powerful frigorific has been generally applied directly to the part, or with the intervention only of the thin gauze containing it; and the duration of the congelation has been from one to ten minutes.

In the spring of the year 1849, I requested the house-surgeon of the Brighton dispensary to apprize me of every case of acute lumbago that came under his notice, and, in all of these, amounting to nine, I employed congelation with perfect and permanent success. The

net containing the ice and salt, was passed to and fro for five minutes, over a surface of about eight by four inches, the skin being blanched during the whole of this period. In only two or three cases was it necessary to apply the remedy twice. Several of the patients rose immediately afterwards from their beds to which they had long been confined. In most cases of chronic rheumatism, the remedy has been equally successful; and this, on account of the frequency of the disease, is one of its most valuable applications. Sciatica has generally yielded to it, but by no means so easily. In acute rheumatism, the local inflammation of the joints is, by this means, invariably and completely relieved, and that portion of the accompanying fever thence arising, is consequently removed. The disease, thus treated, will run a painless course of about a week's duration. In no case, of about a dozen, in which congelation was almost exclusively employed, was there extension of inflammation to the heart; and I am persuaded that the best plan of preventing this, is to subdue the inflammation of the joints from which it generally originates. I did not use the remedy in cases where the heart was already affected, though I have since learned that congelation is employed in the hospital at Vienna (where it was introduced some years ago by Dr Waters of Chester), as an application to the chest in rheumatic carditis. That this affection of the heart would occasionally occur during the treatment of acute rheumatism by congelation is very probable, because it often arises, as the same affection of the joint does, from a morbid condition of the blood over which the remedy can have no control; and that such an occurrence, in the present feeling on the subject, would be called metastasis from cold, is very certain; but I am convinced that it will yet be acknowledged, though probably after many years, that this affection would be much decreased in frequency by the adoption of any means capable of quickly subduing the accompanying arthritis. When it is considered what an immense amount of eventual mischief arises from the organic disease of the heart that occurs under the common modes of treating rheumatic fever, to say nothing of the patient's present sufferings and tedious confinement, it is to be lamented that prejudice should oppose any measure of greater promise. In rheumatic gout, the relief has been as marked from congelation as in lumbago. In ordinary inflammation of the joints it has also been exceedingly useful. Ophthalmia has been immediately cured by keeping the frigorific in contact with the gently closed eyelid for three or four minutes. Glandular inflammation in the neck and groin, yield to a high degree of cold with equal facility. I have been told that in orchitis, its beneficial operation is immediate; and I have little doubt that, from its closeness to the surface, the urethral inflammation causing orchitis, would be quickly suppressed. Congelation has often at once converted an irritable into a healing ulcer, though sometimes the patient has complained of the pain of the operation; it is probable that had the salt in the mixture been prevented coming in contact with the irrit-

able surface, this would have been in a great degree prevented. Certain acute inflammatory affections of the skin are equally under its influence, as erysipelas, eczema, impetigo. It has not often failed in prurigo, but in only one case of psoriasis has it appeared to be of service. Painful nodes are at once relieved by this means, and the inflammation subdued. I have only used congelation in carbuncle as an anæsthetic previously to cutting it, but it is probable (judging from its effect in severe boils) that the incision might have been dispensed with. It has been mentioned to me that severe cold has been employed with the same view in whitlow, of which it is certainly a sufficient cure. The inflammation following sprains, contusions, and other similar injuries, is perfectly under its influence; and the same may be said of burns. In one of my publications on the subject, I have related the excellent and speedy effect of congelation in a case of meningitis, and also in a case of peritonitis: I have not had the opportunity of trying it in other affections of this description. Headache of various kinds has at once yielded to the application, for a minute, of a frigorific over the painful part; and in neuralgia affecting the side, it has generally proved efficacious. In neuralgia attacking the face and other parts, it has often succeeded and often failed. If the seat of the disease be deep in the brain, little can be hoped from this remedy, although there are few obstinate cases of neuralgia in which it does not deserve a trial. Toothache is generally at once relieved by it if properly applied; and there is no remedy for the painful affection of the mouth caused by mercury comparable to congelation. A spoonful of dissolving ice and salt is repeatedly put into the mouth, until it becomes benumbed. In one case of severe scurvy of the gums, where I feared a loss of the teeth, extensive congelation of the gums immediately arrested the disease.

In many of the diseases just enumerated, the promptness of the cure is as remarkable as its certainty. In military and hospital practice this advantage is very prominent.

In cancer the effects of congelation have been various. From my own experience, and that of others, I think that in its early stages, and when from its size the tumour can be thoroughly brought under the influence of the remedy, it will be cured by it. In all stages the progress of cancer will be arrested or retarded, and the pain accompanying it assuaged. The difficulty in advanced cases is to cause a sufficient degree of cold to pervade the tumour. The French translator of a recent paper of mine on the subject (*L' Union Medicale* for May), thinks that the frequent occurrence of cysts in cancerous tumours may facilitate this. But if layer after layer is acted upon, it may be enough. In cancer of the womb the frigorific is applied by means of a speculum, and one stronger than ice and common salt will generally be required. The opinions of Dr Hughes Bennett respecting the nature of cancer have much influenced the mode in which I have used congelation in its treatment. M. Velpeau

states in his recent elaborate work on diseases of the breast, that he has employed *long-continued* congelation as a substitute for caustic in cancer; but of this effect of the agent I have no knowledge.

There are other diseases in the treatment of which severe cold would probably be very useful. It might be applied with such a hope to the spine in tetanus, or to the scalp in certain varieties of mania. After gunshot and other severe wounds, it would prove a powerful preventive and cure of inflammation. Even in pleuritis and other deep-seated inflammation of the chest, as well as in various uterine affections, benefit might rationally be expected from it. In two cases of epidemic cholera, I administered a succession of draughts of a temperature of about 25° Fahr., with apparently excellent effect; and I cannot doubt that the application of cold to the interior of the stomach, which, as appears by the recently published report of the College of Physicians, is the only treatment of cholera which has been unanimously approved of, has not been carried far enough. If the irritation of the mucous membrane be considerable (as it must be, to account for the exhausting and fatal discharges), the temperature of ice merely, is not sufficient to subdue it.

II.—*Anæsthetic Uses of Severe Cold.*—As patients now expect to have every operation performed without pain, both they and their surgeons will be glad to have an easy and agreeable means of accomplishing this, in all the common operations, unaccompanied with the dangers of chloroform. What can be less troublesome in opening an abscess, for instance, or making a cutaneous incision, than touching the skin for a moment with a small brass ball that has been immersed for a few minutes in ice and salt, or a thin spoon filled with such a mixture? It is true, that in deep-seated operations such a means can only suspend the sensibility of the skin; but it is the incision of the skin which constitutes the most painful part of every operation, and if this be benumbed, a smaller, and consequently less hazardous dose of ether or chloroform than has usually been administered, would be enough to remove the sensibility of the other tissues. These deep seated operations, however, constitute a small minority, and if the list of recorded deaths from etherization be referred to (now amounting to more than fifty) it will be found that in three fourths of the number, complete anæsthesia might have been produced with perfect safety by cold.

M. Velpeau, who introduced anæsthesia from cold into France, has, in a lecture on the subject recently reported in the *Gazette des Hôpitaux*, expressed the doubt, whether in some operations, the hardening of the tissues by this means, might not prevent their being cut with ease. I have not found this to be the case, nor does he himself allude to this supposed disadvantage when, in his work on diseases of the breast, he mentions that he has excised tumours after anæsthesia from cold.

The fear of re-action I have already adverted to in the prefatory

observations. Instead of re-action being produced, the anæsthetic is a preventive of inflammation from the wound; and were it used for this purpose alone, it would be invaluable.

Local anæsthesia from cold may, as has already been observed, be produced in a great variety of ways. Some of these may be applied so as to cause immediate congelation, but it is questionable whether the anæsthesia is not more extensive and lasting when more slowly caused. Such details, however, are unsuited to the general view of the subject intended by the present communication, which, I fear, has already exceeded its proper bounds.

ARTICLE V.—*Cases of Tape-Worm unsuccessfully Treated by the Extract of Male Fern and Kousso.* By GEORGE PATERSON, M.D., F.R.C.P. Edin., Tiverton.

THE difficulty of effecting a cure of tape-worm, and the disappointment which has attended the use of all the remedies handed down to us for that purpose, are sufficiently attested by the eagerness with which new remedies, or new forms of remedies previously in vogue, are resorted to by the profession. The following cases, that have recently come under my observation, lead me to apprehend that we are still as far as ever from the knowledge of any certain means of eradicating the troublesome parasite, notwithstanding the flattering representations to the contrary which have lately abounded in the "Medical Journals," and the weight so justly due to the authority and experience of many by whom these representations have been endorsed. It is not impossible that some of these supposed cases might have turned out in the same manner as the following ones, had equal opportunities been afforded of noticing the subsequent history of the patients. But, however that may be, I am satisfied that in two at least of the instances now to be recorded the remedies in question have been fairly tried and found wanting; and that, therefore, they do not always succeed, as we have been led to believe they do, in effecting a perfect cure.

CASE I.—On the 16th November 1852, a stout-built, dark-complexioned man, æt. 38, by trade a currier, and of irregular habits, applied to me to relieve him of a tape-worm. He complained of intolerable itching of the nose and anus, much gnawing pain at the stomach, loss of appetite, vomiting after meals and on rising in the morning, and, for the last two years, what he described as a "desperate pain" of the right side beneath the arm. These symptoms were accompanied with depression of spirits, tremulousness of the hands, and a somewhat anxious appearance, and he was subject to disturbed sleep and frightful dreams, with all which latter symptoms his irregular habits had probably something to do.

These symptoms, to which he has been more or less subject for several years, were at first ascribed to common worms, and under this supposition he took a variety of medicines without any effect. The real state of the case was not suspected until six or eight months previously to his application to me, when, being at Gloucester, and having drank some cider, for the first time for several years,

which produced a relaxing effect upon his bowels, he found that something unusual had passed from him, and this proved, on examination, to be several living joints of a tape-worm. After this many more continued to come away, amounting, he says, to thousands in the course of a few weeks. And ever since connected portions of the worm and single joints have been passing from him, not only when at stool, but at work, and even during sleep, so that he has found them about his bed on rising in the morning. Portions of the worm larger and thicker than those voided downwards have also come off by the mouth in round balls.

He is a native of Plymouth, but has been working at his trade in various distant parts of the country for the last fifteen or sixteen years. When the symptoms of irritation first attracted his attention he was working in Staffordshire, at Newcastle-under-Lyne. Latterly has worked in Exeter and its neighbourhood, but had only just come to Tiverton.

The first thing I prescribed for him was a dose of turpentine and castor-oil, which acted powerfully on the bowels, but brought off no portions of the worm. He was then directed to take a draught containing half a drachm of the oleo resin of male fern, then recently recommended in the "Edinburgh Monthly Journal," and which I had expressly procured from Messrs Duncan and Flockhart, of that city, and to follow this up in the course of a few hours by a dose of castor-oil. The following day he brought with him upwards of twenty feet of tape-worm, the greater part of which had come off entire, but dead, after taking the medicine prescribed. The male fern had not produced any action of the bowels by itself; but soon after taking it he felt as if something had given way and were descending in his bowels, accompanied with great faintness and prostration. The worm did not come off until after the oil had begun to operate. The portion of worm discharged tapered exceedingly towards one of its extremities, the joints becoming extremely small and slender. Other equally minute joints were found broken off from the rest of the worm passed; but, as is generally the case, it was impossible to distinguish the head. All the uneasy symptoms, however, disappeared from that moment. By way of precaution, I repeated the medicine about eight days afterwards, and again a third time eight days after that, but no more portions of worm came away.

Thus far nothing could have been more satisfactory than the effects of the male fern. It confirmed to my mind all that had been reported in its favour, and I set down the case as a decided cure. But after the lapse of two months, on the 25th of January 1853, this man again applied to me on account of a return of his symptoms. It appeared that he had only remained free of them about four weeks. Then, one morning while going to his work, he was seized with overpowering sickness, vomited blood, and fainted in the street. In the evening the old itching of the nose and anus returned, and on awaking the next morning he discovered about the bed from thirty to forty joints of tape-worm, all dead. He had seen no more since that day; but, as the itching and depression of spirits continued, he believed that there were more to come away. I immediately made him take the male fern in the manner formerly prescribed. About three hours after taking it, followed by the castor-oil, a dozen joints or more, dead and discoloured, were brought off. The uneasy sensations immediately ceased; and, although I took the precaution of repeating the dose as before, no more joints came away. I met him several times after in high spirits, and flattering himself that he was now fairly quit of his troublesome inmate.

On the 18th of April he applied a third time for relief. This time he had remained quite comfortable for nearly three months—in fact, until a week before his application—when, while walking along the street, he felt a return of pain in his right side, and a little itching. Conscious of what these symptoms indicated, he examined his clothes immediately on reaching the work-shop, and found that some joints had passed from him. More came away while he was at work, and from that time more or less had been passing nearly every day, all alive, and all single. He stated that they had been passing "in scores," and that on the pre-

vious day there had come from him "one living mass," more than ever he had passed at one time, except after the first dose of the male fern. As several trials of this remedy had failed to eradicate the parasite, I now resolved to try what the kousso would do, and ordered him to take half an ounce in the manner usually recommended. Half an hour after taking it, he felt as if something were dislodged within him, with much pain and exhaustion. Almost immediately upon this the bowels acted, and he brought off upwards of twenty feet of tape-worm, a very considerable length of which was unbroken. The joints of this were generally larger than in the one brought away by the male fern, and presented more of the tapering and gradual diminution observed in it.

As respects a complete cure, however, the kousso has proved equally ineffacious with the male fern. For, on the 12th of July, the man again presented himself with a return of his complaints. He had continued quite comfortable until a fortnight previously, when he was again suddenly seized in the street with faintness and vomiting. He then began to bring off live joints, and had been doing so ever since, with his old symptoms as bad as ever. Another dose of the kousso was prescribed for him, which brought off several more feet of tape-worm, with much pain and griping. His symptoms were once more relieved, and he gave up attending. The man has since left the town; but I am informed that he still continues subject to returns of his complaint.

Case II.—A young woman, a domestic servant, æt. 24, and a native of this town, where she has lived nearly all her life, has for some years been troubled with symptoms of tape-worm. They were until recently supposed to be owing to some of the more common kinds of worms, as several lumbrici have been passed on different occasions. But in February 1853, while cleaning a grate in the parlour, something came away by the anus, which proved to be the joints of a tape-worm, and since then others have followed at intervals. In this instance, I tried the male fern and kousso in succession, but both completely failed to bring off any portions of the worm, only producing great pain, faintness, and prostration, while several yards, in broken macerated pieces, have come off under the operation of turpentine and castor-oil alone. But this, although it appears to have more power in the present case than the other remedies, and has therefore been more than once repeated, has failed equally with them in effecting a cure. She still continues frequently to complain of returns of the troublesome itching and pains in the stomach and bowels, and now and then passes portions of the parasite.

Case III.—A widow, æt. 32, a native of Tiverton, where she had lived for fourteen years, applied at the Dispensary last July with symptoms of advanced phthisis. She brought with her a prescription for the pomegranate bark which I had given her two years previously, but which she had never yet taken. This led me to make inquiries, from which I learned that she was still the subject of tape-worm, as she had been at the time she previously consulted me, and for a period of ten years, portions of the worm had passed at intervals. I immediately ordered the prescription she produced to be made up and taken; but no decided effect following upon it, I next prescribed for her a dose of the male fern. This, within half an hour after being taken, caused much nausea and pain in the bowels, and brought away several feet of tape-worm. Some castor-oil was taken according to the usual directions shortly after, and when it began to act on the bowels brought off more portions of the worm. The peculiar symptoms were at once relieved, and did not return for many weeks that she remained under my observation, though she still continued occasionally to bring off what appeared to be disorganised portions of the worm. She afterwards became a parish patient, and about the end of the year died of phthisis under the care of the medical gentleman of the district in which she resided, who informs me that the fact of her having been the subject of tape-worm never came to his knowledge,

although he was in attendance upon her for several weeks. It is not, of course, intended to adduce this case as an instance of the failure of the male fern. But before we set it down as one in which this remedy effected a complete cure, it will be well to bear in mind that the pulmonary disease had for some time past been the principal object of concern to the patient; so much so that on her application at the Dispensary it was only through her producing an old prescription that the circumstance of her being the subject of tape-worm was elicited. The parasite, in fact, does not appear to have caused the same amount of distress and inconvenience in this case as it does in some others; for not only was the old prescription for its relief neglected to be taken, but she had married and become a widow since the period when that prescription was given. It is therefore quite possible that she may have had returns of this complaint up to the time of her death without directing the attention of her medical attendant to it, especially as in her state of debility she would dread the energetic operation of the appropriate remedies.

Remarks.—1. It would appear from the above cases that the kousso and oetherial extract of male fern, are, like all other remedies that have hitherto been prepared for tape-worm, liable to, at least, occasional failure. At the same time it is certain, from the effects that followed upon their administration in these instances, that they are remedies of great power, very obnoxious to the parasite, and therefore valuable additions to our means of treatment.

2. I should be disposed to believe from what was observed in these cases that the same remedy is not suited alike to every case; but that in one subject the kousso, in another the male fern, and in another the turpentine, will be found to be the most appropriate remedy. Thus, in the second case, neither the male fern nor the kousso brought off any of the worm, while turpentine invariably did so. In the first, on the contrary, turpentine and castor-oil were repeatedly given before the male fern, without having the least effect. Again, we sometimes find the worm, not only detached and dislodged, but carried downwards and expelled by the male fern and kousso, unaided by any purgative; while at other times they require to be followed by the administration of a purgative dose. As to the comparative energy of the kousso and male fern, it would appear from the above cases (in which both were successively given to the same person) to be nearly equal, and in the phenomena attendant upon their operation both remedies are very much alike.

3. The first of the cases I have narrated is in many respects a remarkable one. It furnishes a striking portraiture of the symptoms of the disease in a very aggravated form; and the immense number of joints that were discharged at various times, both singly and in pieces of several feet in length, would seem to indicate either that there must have been more than one of the parasites in the man's body, or else that it is capable of reproduction and of attaining its former growth in an incredibly short space of time after large portions of it have been detached.

4. The same case affords an instance of the worm finding its way into the stomach, portions of it being stated to have been brought up by vomiting in round balls. This is a more common occurrence

with other kinds of intestinal worms. Thus I have met with several instances, both in children and adults, where lumbrici have escaped by the mouth or nose. In one case of an hysterical and hypochondriacal female the circumstance of one of these worms having escaped by the mouth produced an impression that she still had one in the windpipe. The sensation, which was partly hysterical and partly perhaps due to an enlargement of the thyroid body, led to continual efforts to bring up the worm and dislodge it with the finger, which necessarily produced increased local irritation, and the failure of which aggravated her despondency and low spirits. She vainly resorted to several medical men, including myself, for relief—neither reasoning nor such remedial measures as the nature of the case suggested having succeeded in overcoming the delusion.

5. In the second case we have an instance of the co-existence of another kind of worm with the tape-worm. This is worthy of notice, principally as showing (what has been pointed out by Dr Watson and others) that the term "solitaire," which some French writers have applied to this worm as a distinguishing characteristic, is not correct.

6. Besides the above three cases falling under my own notice in the course of a few months, I have heard of another instance under the care of one of the medical gentlemen of this town, in which the kousso brought off several yards of a tape-worm, but equally failed to eradicate the disease. One would almost be disposed to argue, from so many instances of a somewhat rare affection observed within a short period among persons, the majority of whom are natives of the place, that the disease is more than usually common in this locality. On inquiry, however, among the oldest practitioners of the town, they inform me that this is by no means the case, and that whatever fortuitous circumstances may have concurred to multiply instances within so short a period, their experience of such cases is not larger than that of the generality of medical men in other places.

ARTICLE VI.—*Report of a case of Carbuncle occurring in Mr Syme's practice, illustrating especially the Pathology of that disease.* By JOSEPH LISTER, M.B., London, F.R.C.S., Resident Surgeon to the Clinical Wards of the Royal Infirmary.

THOMAS DAVIDSON,—æt. 52, admitted into the Royal Infirmary, Feb. 9, 1854, a weaver, residing at Sinclairton near Kirkcaldy. States that he has generally enjoyed good health, and that three weeks before his admission he was in no respect worse than usual; he had been in full work and had eaten and drunk his usual quantity without excess in either respect. At this time his attention was directed by a sensation of itching to the back of his right shoulder, and on putting his finger to the part, he found a small elevation about as big as a barley-corn and very tender to the touch. This grew rapidly and became the seat of intense pain, and continued to increase in size till his admission, but had been less painful for a few days preceding it. When he came to the Infirmary a large elevated mass existed behind the right shoulder, of circular form, about six inches

in diameter, rising gradually from the level of the skin around: of livid red colour surrounded by brighter redness of an inch or two of the adjacent skin. Its circumferential part was of brawny consistence, while the central part was soft and pulpy, but not fluctuating, and in this central part there were numerous small circular apertures, which did not admit the probe for more than a very short distance; the instrument could be passed a little way under their margins, which were formed by a vascular superficial layer as thin as paper. At the centre of the tumour these openings were confluent.

On the day of the patient's admission Mr Syme made a very free crucial incision through the tumour, extending down to its very base, and reaching a little way into the bright red surrounding skin. A good deal of bleeding occurred, and the colour of the tumour became rapidly and very remarkably changed to a pale bluish red tint. The cut surface, which in the centre measured nearly two inches perpendicularly, presented numerous small collections of pus scattered through it, and many spots of yellow lymph; the rest of the tissue was evidently the dermis expanded by the inflammatory exudation, and towards the centre of the tumour in a shreddy sloughy state. The patient was not under chloroform, and says he hardly knows whether the pain of the incisions was worse than that which he had suffered from the carbuncle a few days before admission. Mr Syme ordered milk diet, which has been gone on with to the present time (Feb. 14th), while linseed-meal poultices have been applied twice a day. Under this treatment the carbuncle has daily improved, induration diminishing and the mass melting down, partly in the state of slough and partly in that of pus; the surrounding redness is almost totally gone, no extension whatever of the disease having occurred since the incisions were made. The pale tint of the skin that occurred at the time of the incisions, never became deepened, except at one part, where the interval between the incisions was greater than elsewhere, and there it remained red and hard for a day or two; but there also the free drain afforded by the incisions has some days ago removed both redness and induration. Two days after the incisions were made, he regained his appetite, which he had lost ever since the carbuncle became intensely painful, though not for several days after the commencement of the disease.

A few days after I again made a careful examination of the affected part. At this time the sections of the carbuncle made by the incisions were assuming a healthy appearance, particularly towards the circumference of the mass. These sections were now covered with granulations, and this, together with their great thickness, gave them an appearance certainly very unlike that of sections of the dermis. The incisions had, however, extended a little beyond the carbuncle into the healthy skin around, and on tracing the sections outwards to the parts where the skin was healthy, I found that the thick diseased parts passed insensibly into the sound portions, whose sections were also covered with granulations, and differed from the diseased only in their thickness.

The slough soon separated completely, leaving the external portion of each of the four flaps made by the crucial incision, and these gradually assuming the character of healthy skin, a granulating sore remained, whose cicatrization presented nothing remarkable.

Remarks.—This case illustrates some important points in the pathology, etiology, and treatment of carbuncle. Mr Syme long since pointed out[1] that carbuncle is essentially a disease of the true skin, that it is by no means necessarily connected at its outset with an impoverished or enfeebled state of body, although, doubtless, dependent on some constitutional vice, and that being an inflammation of peculiar intensity, it requires in its early stages general antiphlogistic treatment, and local blood-letting, in the form of free incisions,

[1] Vide Principles of Surgery, by James Syme, first edition, 1831, page 619.

which, if practised sufficiently early, cut short the inflammation, and prevent it from running on to sloughing.

The case before us was a typical example of carbuncle, and its commencement as a pimple in the skin, combines with the appearances which it presented on admission, to show clearly that the pathology above alluded to is correct, so far as this individual case is concerned.

In a case of extensive carbuncle in the gluteal region which occurred about a month earlier, I was fortunate enough to obtain a slice from a part in an early stage of diseased action. This slice, which was cut perpendicular to the surface, included a small part of the subcutaneous fat, which was perfectly healthy. The dermis, however, was greatly thickened, and presented the appearance of numerous spots of greenish-yellow lymph intersected by bluish glistening bands of fibrous tissue, the dense stricture of the corium being expanded into a loose net-work, in whose meshes the lymph lay. The only part of the dermis that did not contain more or less of this deposit of lymph was a very thin and highly vascular layer immediately beneath the epidermis. The microscope showed that the blood-vessels of the most superficial part of the corium, including the papillæ, were gorged with blood. This layer was about equal in thickness to the epidermis (which is thin in this region), and corresponds to the part which formed the margin of the openings in Davidson's case, described in the above report as being as thin as paper. A superficial observer might suppose this thin layer to be the skin, and regard all parts beneath it as subcutaneous tissue; but it required only a little careful observation to perceive its true relations. I confess that I had previously been accustomed to look upon carbuncle as essentially a disease of the subcutaneous tissue, but this slice convinced me, as it did every one to whom I showed it, that in that case at least the cutis vera was the seat of the disease; and it is to be observed that this, like Davidson's, was a fine example of the affection in question. No doubt, the subcutaneous textures may, and often do become affected secondarily, but the important practical fact which these two cases appear to establish, is that the great mass of a carbuncle is composed of thickened dermis, which may be freely incised without fear, whereas the subcutaneous textures in some regions contain important parts which it is very desirable to avoid. It may be remarked that the extraordinary expansion which carbuncle produces on so dense, and, at the same time, so sensitive a structure as the dermis, sufficiently accounts for the intense pain that always accompanies it.

With regard to the cause of the disease, it must be confessed that in the case above reported, none can be assigned. According to the patient's own account, he was in strong active health at the time when the carbuncle commenced, neither plethoric nor the opposite, and no irregularity had occurred either in his diet or in his employment. The constitutional vice, of whatever nature, was latent.

The free incisions made throughout the diseased mass were productive of the most palpable benefit, and evidently cut the disease short at once; and although the central part was already in the state of slough at the time of the patient's admission, yet considering that the size of the tumour was then still increasing, I cannot doubt that the red brawny circumferential parts, which afterwards recovered their natural characters, would have sloughed if the incision had not been carried freely through them. As the disease yielded to the local treatment, the patient recovered his appetite, which he had long lost, but he never grumbled at his milk diet, although it was inadvertently continued much longer than Mr Syme had intended.

It will probably be tedious to some to read so minute a description of an affection with which they have long been familiar; but to those who do not happen to have examined the disease carefully themselves, and who know that a different pathology is commonly taught, the importance of the subject will, I hope, make the particulars which I have given acceptable.

Part Second.

REVIEWS.

Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants, and Contributions to Phytogenesis. Translated from the German of Dr Th. Schwann and Dr M. J. Schleiden, for the Sydenham Society. By HENRY SMITH, F.R.C.S.E., etc., London. 8vo. 1847. Pp. 268.

Manual of Human Histology. By A. Kölliker. Translated and Edited by GEORGE BUSK, F.R.S., and THOMAS HUXLEY, F.R.S., for the Sydenham Society. Vols. I. and II. London. 8vo. 1853–54.

Lectures on Histology, delivered at the Royal College of Surgeons of England. By JOHN QUEKETT, Professor of Histology, etc. Vols. I. and II. London. 8vo. 1852–54.

Grundzüge der Pathologischen Histologie. Von Dr CARL WEDL. Wien. 8vo. 1853. s.s. 825.

Chemie und Mikroskop am Krankenbette. Von Dr MARK AUREL HOEFLE, etc., Zweite Ausgabe. Erlangen. 8vo. 1850. s.s. 220.

An Introduction to Clinical Medicine. Lectures IV. and V. *The Microscope as a means of Diagnosis.* By J. HUGHES BENNETT, M.D., F.R.S.E., etc. Edinburgh. Fcp. 8vo. 1853. Pp. 134.

The Microscope and its application to Clinical Medicine. By LIONEL BEALE, M.B., etc. London. Small 8vo. 1854. Pp. 303.

The Micrographic Dictionary; a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. By J. W. GRIFFITHS, M.D., and ARTHUR HENFREY, F.R.S., etc. London. 8vo. Parts I. and II.

IT cannot fail to strike those who pay any attention to the rise and progress of different branches of knowledge, that each possesses its period of development and growth. At first, facts are laboriously made out, the usefulness of which, for a long time, remains unknown. But when these have accumulated, some master mind combines them together, points out their affinities and dissimilarities, and suggests the law which governs their production. Thus science is added to discovery. Then commences a period of activity, in which the facts are greatly increased in number, and the original law is more extended or modified. Then, also, the new study is applied to the wants and necessities of man, and practical experience is added to facts and theory. At this period in its history the science has reached adult age, and now commences its career of usefulness. It seeks alliances with other branches of knowledge, and as a result, new facts, new theories, and new practical applications, are born and multiplied. In this way, after struggling through the stages of infancy and youth, the science reaches a vigorous manhood; it gives and receives support to its neighbours, and at length, losing much of its individuality, melts into that whole which constitutes the sum of human knowledge.

The science of histology appears to us to have run through the first stage of its course, and to be now entering upon the period of utility and practical application. With some, indeed, it is beginning to be lost in the general laws of biological science. From the time of Borelli and Malpighi we observe it laboriously accumulating facts. We can trace it through numerous difficulties, and determine the circumstances which opposed its progress, and the means by which these were removed. Since the celebrated publications of Schleiden and Schwann became known in 1840, a scientific aim has been given to histological investigation, and our knowledge of the structure and development of the various tissues of plants and animals has increased with unexampled rapidity. Physiology and pathology have in this way been revolutionized within the short period of twelve years. We now behold it very generally applied to practical medicine. The art of diagnosis is improved, and this, combined with a more extensive acquaintance of morbid processes, suggests plans of treatment which have been useful in curing diseases and prolonging life.

There can be no question that it was in Germany that histology was first moulded into a science, and taught by its distinguished professors to medical students. The improvements which had been

made in the achromatic microscope, and especially their construction by French and German opticians at a moderate cost, enabled students to prosecute researches in this new direction, and the thesis of the graduate, the periodicals, and, subsequently, systematic works, soon exhibited the results of their inquiries. In France, notwithstanding the instructions of several Germans established in Paris, histology has made very little progress, and, with the exception of one Agregé, we know of no professor who is in any way distinguished in this branch of knowledge. In this country systematic and practical courses of lectures on histology were first commenced in Edinburgh, during the session 1841-2, and have been continued ever since. A large quantity of French microscopes have been imported, so that the students in the Scotch as well as in the German schools, were enabled to observe for themselves. In Scotland, the science has made great progress, is widely diffused, and its practical applications to medicine have for several years been carried to their utmost limits. In England, histology has assumed peculiar features. No doubt it has been prosecuted there by many distinguished men in a scientific spirit, as is evinced by numerous communications inserted in the Philosophical, Linnæan, and Medico-Chirurgical Transactions. At the same time, it has assumed too much of an industrial character, from some of its cultivators having associated themselves with the manufacturers of microscopes. In consequence, the means have been somewhat confounded with the end. Very excellent, but at the same time, very expensive and very cumbrous microscopes came to be considered as a *sin qua non*, and histology was talked of, as microscopy, micrography, or as microscopical science! Astronomy is also investigated by means of optical instruments, yet how unnatural it would appear if its cultivators talked of *telescopical* researches, looked upon a star as a *telescopic* object, regarded observatories as places for *telescopical* research, established a *telescopical* society, and supported a journal of *telescopical* science. Such, however, it cannot be denied is the spirit in which microscopy is largely studied in London, to the prejudice of histology. In Ireland, very little progress has yet been made in the study, although the efforts of two young teachers there will, we hope, ere long be followed by some results.

The various works we have placed at the head of this article exhibit the present aspect of our literature on this subject. The Sydenham Society has undoubtedly done good service by its translation of the Memoirs of Schleiden and Schwann, and of the systematic work of Kölliker. The former are classical works in histology, and must be carefully studied by all who pretend to unravel the difficulties of texture. The latter is a work of which it is impossible for us to speak too highly. Its extent precludes the possibility of analysis, and there are so many points which are new; there is so much of original investigation; so large a number of facts which require confirmation, that it would be premature, even now, to speak

with too much confidence regarding them. Our thanks are justly due to Messrs Henry Lee, Busk, and Huxley, for the careful manner in which these most important works have been rendered into English.

The lectures on histology by Professor Quekett are characterised by a wonderful knowledge of individual facts, a most extensive acquaintance with texture throughout the vegetable and animal kingdoms, and a most painstaking care in the description and illustration of microscopical objects. We believe, that in everything that relates to the microscope, and in all that is connected with the skilful preparation of the textures, Professor Quekett has never yet been surpassed. Under his direction has been formed in the Museum of the London College of Surgeons, an histological collection, second to none now existing, if we except that of Harting at Utrecht. His vascular preparations especially, will compete in beauty and delicacy with the most finished productions of the Vienna school. Science will ultimately be greatly indebted to his labours, and we can confidently recommend his work as an emporium of well observed facts, the study of which is rendered easy by profuse and most accurate illustrations.

Pathological may be said to be coeval with physiological histology, but could not assume anything like precision until a study of the healthy textures was known. The expressions healthy and morbid, however, are, after all, but relative terms, and the more successfully we study the phases of disease, the more are we convinced that physiology and pathology only constitute one science. They must be studied together, for physiology could not exist without the observation of morbid processes, and the formation of a true pathology must necessarily be based on a sound physiology. So in histology, the history of cells, fibres, and other elementary textures could never have been known without a combined observation of them in their abnormal as well as normal conditions. The work of Dr Wedl exhibits this most clearly, as it everywhere presupposes a full acquaintance with physiological histology. We have long held the opinion that the systematic works of Vogel, Gluge, and Rokitansky on this subject were premature, and entertain doubts whether the production of Dr Wedl is not so. But we are willing to confess that it exhibits pathological histology in Germany, much more advanced than any previous work does. With the labours of British histologists the author seems altogether unacquainted. We have great pleasure, however, in recommending this work to our readers as the most perfect systematic work yet published on the subject, full of instructive and novel facts, and richly illustrated with original woodcuts, exhibiting laborious research. The book is further remarkable for the goodness of its paper and printing.

The advancement of physiological and pathological histology, having modified our conception of the nature of many diseases, and

pointed out the alterations in structure they frequently present, led naturally to a change in our views of treatment, and an attempt at improving our diagnosis. The influence of histology on therapeutics, however, is a topic not yet ripe for discussion, but is one which rightly belongs to an improved knowledge of clinical medicine. At present the microscope like the stethoscope must be considered an instrument essential to the physician, especially in the diagnosis of diseases of the blood, kidney, stomach, and skin. It is the careful study of the relations existing between histological morbid changes, the symptoms attending them and their treatment, that ought to govern our efforts in rendering the microscope useful at the bed side. This is the point of view in which it is considered by Drs Hoefle and Hughes Bennett. Whereas we cannot but consider the work of Dr Beale as possessing a misnomer. Clinical medicine occupies a very small portion of his book, which, in fact, is a guide to the use of the microscope generally, and to the method of preparing and mounting microscopic objects. It is profusely illustrated with figures, many of which are repeated more than once, and, in a work on clinical medicine, seem singularly out of place. For instance, chapter eight treats of how to make vascular preparations, and to render the text clear to the meanest capacity, we have the figure of a syringe and two stopcocks inserted. A beautiful woodcut of a common tenaculum holding a piece of thread, shews with what instrument the vessel should be tied. We are also gravely informed that corks will be found useful occasionally to close the injecting pipes, and figures of two corks are given to impress that fact on the mind. These are followed by an accurate representation of a water bath holding several injecting cans. There is also a plate containing careful copies of the grit in vermillion (p. 103). What has all this to do with clinical medicine? On the other hand, many important subjects truly clinical, are very meagrely illustrated, and very carelessly described, such as all that relates to morbid growths, vegetable parasites, blood, various secretions, discharges, etc. The urine is well treated of, and chapter sixteen, on the application of chemical analysis to microscopical investigation, is a useful one. Notwithstanding we think Dr Beale's book to have been wrongly named; it will be found very serviceable to the student, as a guide to the use of the microscope. It is a good illustration of the London microscopical school, to which we have alluded.

The last work on our list, is certainly a very extraordinary undertaking, being a dictionary, which the authors wish to be regarded as an index to our knowledge of the structure and properties of bodies revealed by the microscope. Although this is eminently a work of the microscopical or micrographical school of science, we cordially recommend it as one likely to be very useful to the histologist. When concluded, we may notice it at greater length.

We have thus shortly directed the attention of our readers to the subject of histology, in its scientific and practical relations, to all of

which the different works placed at the head of this article constitute sufficient guides. We need scarcely say, that although in our opinion every encouragement should be given to this new branch of science, it can only be rendered permanently useful by being associated with anatomy, chemistry, and that sound practical instruction in medicine and surgery, which the various schools in this country are so well calculated to impart. Above all, we would impress on our young friends, that careful observation and good sense are better qualifications for its study, than the possession of expensive instruments, and that the best microscope is, after all, a mere tool in the hands of a scientific and practical man, for arriving at useful ends.

An Inquiry into the Pathological Importance of Ulceration of the Os Uteri. By CHARLES WEST, M.D., etc. Pp. 95. London 1854.

A Treatise on Acute and Chronic Diseases of the Neck of the Uterus. By CHARLES D. MEIGS, M.D., etc. Pp. 116. Philadelphia and London, 1854.

THE department of obstetrics treated of in these two works has been of late much in the minds of professional men, and, we are constrained to say, without corresponding profit or results. It has been also much in the hearts of professional men, and the emotions excited have hitherto not been often of a pleasant or inspiring character. The division of obstetricians into parties has certainly not been always the result of truth-seeking differences, nor have the partizans on either side gained any marked success as yet against their opponents. But this speculum controversy, though it has borne an ugly character, and although the best men in the profession have kept aloof from its quarrels, has, like many other questionable things, led at length, to decided progress and to valuable results.

Dr West comes before us with authority, fresh and unbiassed, and craves the indulgence of all persons from whom he may differ. "For (says he) I am fully sensible how often I may need for myself that candid interpretation of my statements, and that lenient judgment of my errors, which I hope always to manifest to others."

Before we proceed to analyse Dr West's little book, we may at once say that it is a real contribution to the progress of obstetrics, that every part of it bears the impress of a cautious and logical mind which has thought soundly on its subject matter, and has been guided solely by the love of truth. It is a work which will add greatly to Dr West's reputation as an author. We are not now to criticise the use of statistics in the work, and the way in which Dr W. contrives to make his conclusions appear to rest upon them. We shall merely say that they are interesting and valuable as *simple* data, and that, as stated, they fortunately tally with the author's views. We feel confident that they form only a small part of the basis on which the author's conclusions really rest.

Dr West's book consists of three chapters, being the Croonian lectures for 1854, delivered under the auspices of the Royal College of Physicians of London.

In his first lecture, Dr West states the subject of his inquiry.

At a time when, from various causes, the knowledge of the healthy structure and of the natural functions as well as of the diseased structure and perverted functions of the uterus was still very imperfect and defective, M. Récamier began to employ the speculum in the treatment of some of the severest affections of the neck of the organ. And it was not long till the value of this instrument was tested in the investigation of other more frequent diseases of the concealed generative organs. The great result which followed this innovation of Récamier's was the important discovery that in many cases of leucorrhœa, the morbid discharge was not the mere consequence of constitutional weakness, of ulceration of the vagina, or of inflammation of its mucous lining, but depended upon a morbid condition of the vaginal portion of the womb. On looking through the speculum at the exposed cervix, the surgeon could see, with more or less distinctness, patches of red discolouration, having often a granular appearance. These he soon learned to call *ulcerations*, and was occasionally gratified to find that by caustic applications he could at once remove the redness, and control the leucorrhœa.

"The ulcerations to which such important results are [by the speculum party] attributed are for the most part mere superficial abrasions of the epithelium investing the lips of the os uteri, whose abraded surface is of a vivid red colour, and finely granular. In other cases, in which the absence of epithelium is less complete, the surface seems beset by a large number of minute, superficial, aphthous ulcerations, between which the tissue appears healthy, or slightly redder than natural. The ulcerations of the os uteri seldom or never present an excavated appearance with raised edges, as ulcers of other parts often do; but either their surface is smooth, or it projects a little beyond the level of the adjacent tissue. They are usually, but not constantly, of greater extent on the posterior than on the anterior lip, are sometime confined to the former, but very rarely indeed limited to the latter. They appear to commence at the inner margin of the os uteri, whence they extend outwards, and sometimes, though by no means invariably, the short extent of the canal of the cervix uteri which can be brought into view by the speculum, appears denuded of its epithelium. The adjacent parts of the os uteri vary considerably in their appearance; sometimes their natural pale rose tint is preserved up to the edge of the abrasion, which is marked by a distinct well-defined line, while at other times the whole surface is of a much more vivid red than natural, and the line of demarcation between the abraded and the healthy surface is irregular and indistinct, the one encroaching on the other. The orifice of the uterus is generally more open than in a state of health; and the disappearance of the abrasion, which always takes place from the periphery towards the centre, is accompanied by the gradual closure of the previously patent orifice. The state of the tissue of the os and cervix varies; sometimes there is a very marked softness of the parts, the condition resembling that of the uterus soon after abortion or delivery, while at other times it is much harder than natural; but it certainly is not at all a common occurrence for extensive abrasion of the os uteri to co-exist with a condition of the organ such as would seem healthy to the touch. The secretion from the surface varies considerably in different cases, and the chief part of the leucorrhœal discharge from which patients suffer is derived from within the canal of the cervix, or from the cavity of the womb—not from

the abrasion itself. Still, in some instances, those especially in which the ulceration presents a very marked granular character, the discharge derived from this source alone is far from inconsiderable. The degree of sensibility which the ulcerated surface possesses also varies greatly; now and then the slightest touch is extremely painful; but, in the majority of cases, the ulcerated surface is not more sensitive than the adjacent parts, nor is the neck of the uterus, whose os is abraded, by any means constantly more tender to the touch than the same part of an organ entirely free from that affection."—Pp. 12-14.

By an error, certainly not without parallels in the history of medicine, the influence of these ulcerations on the pathology of the uterus came to be very much overrated. We do not wish here to make any reference to the question of the propriety of the frequent and oft-repeated use of the speculum and caustic in female diseases. The point to which we draw attention at present is one of pure pathology, namely, the extent to which these ulcerations, with or without their connected inflammation and induration of the cervix and fundus uteri, should be regarded as causes of sterility, abortion, and symptoms and affections other than those inseparably connected with leucorrhœa. These are found generally to co-exist with an enlargement of the body and cervix of the uterus, and often also with ulceration of the cervix and leucorrhœa.

"Under such circumstances the menstrual function is usually in some respect or other ill-performed, while secretions are likely to be furnished from the organ differing in quantity or quality from those which proceed from it in a state of health; the performance of all the sexual functions is very apt to be attended by pain; impregnation is less likely to occur, and if pregnancy should take place, there is very great probability of its coming to a premature termination."—Pp. 12.

Often connected with an imperfect recovery after abortion or natural delivery,

"This set of symptoms, however, or at least many of them, are met with independent of pregnancy and its consequences, supervening sometimes, indeed, under the influence of causes which evidently, and in a marked manner, interfere with the generative functions, but coming on at other times slowly, and, as far as we can discover, without cause. How are they to be explained? Do they proceed from an invariable pathological occurrence, which is present in every case, how wide soever may be in other respects the points of difference between them—or are they the indications of disordered function, which may depend on causes as various as those which produce vomiting or occasion dyspnœa? The inquiry is manifestly an important one; its elucidation will be the object of these lectures. It has been said that there is an invariable, or almost invariable, cause of these symptoms—that, be the remote occasion of them what it may, inflammation and ulceration of the neck of the womb is their immediate cause—that the key to the right understanding of uterine diseases is to be found in the correct appreciation of the importance of this condition; and the cardinal point in their treatment consists in the adoption of means for its cure."—Pp. 12.

Having thus enunciated the question to be discussed, Dr West proceeds. First of all he shows that the cervix uteri is more lowly organised than the body of the organ, that it is physiologically of secondary importance, that it is much less subject than the body of the uterus to alterations in its intimate structure, and comparatively insen-

sible to rude modes of therapeutical interference, and that therefore the assumption that some slight abrasion of its mucous membrane (ulceration) is capable of causing a list of ills so formidable as are attributed to it, ought to rest for its support on some better foundation than any inference deducible from anatomical or physiological data. He then details the results of anatomical investigation, and in conclusion, from observations upon 62 cases states that,

"In by far the majority of cases, the ulceration, when present, was not merely trifling in extent, but that it had not given rise to so much irritation of the neighbouring tissues as to produce any appreciable congestion of the mucous membrane in its vicinity; while the changes in the uterine substance alleged to depend upon it were oftener present without than in connection with it; and, moreover, none of the alterations about the os and cervix of the womb were so considerable as those which were apparent in its cavity."—Pp. 31.

In the second lecture Dr West describes the course and consequences of ulceration of the prolapsed uterus, and shows that uterine functions and structure are not seriously disturbed thereby. He then proceeds to the most important part of his argument, namely, that derived from the results of clinical observations made at St Bartholomew's. By isolating the large class of cases of women suffering from symptoms frequently attributed to ulceration, and dividing these into two classes; the first containing those where ulceration was observed; the second those where it was absent; he obtained the statistical results upon which he seeks to found his conclusions. These conclusions, it is needless to say, correspond with the whole scope of the work, and are as follows :—

"1st. Uterine pain, menstrual disorder, and leucorrhœal discharges—the symptoms ordinarily attributed to ulceration of the os uteri—are met with independently of that condition almost as often as in connection with it.

"2nd. These symptoms are observed in both classes of cases with a vastly preponderating frequency at the time of the greatest vigour of the sexual functions, and no cause has so great a share in their production as the different incidents connected with the active exercise of the reproductive powers. But it does not appear that ulceration of the os uteri exerts any special influence, either in causing sterility or in inducing abortion.

"3rd. While the symptoms are identical in character in the two classes of cases, they seem to present a slightly increased degree of intensity in those instances in which ulceration of the os uteri existed.

"4th. In as far as could be ascertained by careful examination, four-fifths of the cases of either class presented appreciable changes in the condition of the uterus—such as misplacement, enlargement, and hardening of its tissue, while frequently several of these conditions co-existed. An indurated or hypertrophied state of the cervix uteri was, however, more frequent in connection with ulceration of the os uteri than independently of that condition.

"5th. The inference, however, to which the last-mentioned fact would seem to lead, as to the existence of some necessary relation—such as that of cause and effect—between ulceration of the os uteri and induration of its cervix, is in great measure negatived by two circumstances :—

"1. The number of instances in which an indurated cervix co-existed with a healthy os uteri.

"2. The fact that, while induration of the cervix was present in 25 out of 46 cases in which the ulceration of the os was very slight, it was altogether absent

in 9 out of 16 cases in which the ulceration was noted as having been very extensive."—Pp. 58, 59.

Dr Meigs's work is interesting and amusing, but so rambling and discursive in its composition that we would find it utterly impossible, were it desirable, to give any good account of it. Ulceration of the cervix, polypus, uterine hypertrophy, fibrous tumour, retroversion, physiology and pathology, are jumbled together in admirable confusion; and the whole are dressed in such terms as would defy an entire college of physicians to interpret. We shall notice very briefly a few of these points in order to give our readers some idea of this American production, and to deter the author and his admirers from attempting a free and easy style of writing at the expense of scientific exactness and truth.

Describing pregnancy as a *physiological hypertrophy*, Dr M. says that,

"In this process of normal hypertrophy the several elementary textures are evolved in their equal generical ratios; and the same kind of equable development takes place in all the cases of true pathological hypertrophy."—P. 27.

"Unequable development of the several elements, converts the womb into a tumour, degenerates it, and renders it incurable by medicines."—P. 28.

Whether this deserves the name of nonsense or error we shall not attempt to decide. It is sufficient to remark that the natural growth of the womb in pregnancy is not an *hyper*trophy at all; that, so far as we know, Dr Meigs nor any one else has described the *equal generical ratios* of the several elementary textures: that, in the same way, equable and unequable developments of elements are unintelligible terms; and that the womb is never converted into a tumour, although tumours may be developed in it. After all, however, we have a hazy notion of Dr M.'s meaning, and we shall not be officious, but let Dr M. himself put it into other words, at the same time giving him an opportunity to contradict his previous statement. Speaking of retroflexion with hypertrophy, he says,—

"It is a case in which, by the nutrition of one segment or half of the womb, it has grown smaller or larger than the other symmetrical half or segment; for, it is clear, if both the anterior and the posterior halves should grow or be developed *pari passu*, the organ could be only straight, and could be by no means crooked or bent, as in the above diagram. The quantity of the convex must greatly exceed the quantity of the concave half; otherwise, the womb could not be bent, but must remain straight or fusiform".—Pp. 90, 91.

Here Dr Meigs describes surely a case of true pathological hypertrophy, but he forgets his grand principle and gives it us as manifestly unequable. The one description is just as inaccurate as the other.

Accompanying Dr M. we are introduced to such novelties as the "complexion of the womb;" "the corpus mucosum uteri;" "the vital status of the corpus mucosum;" "the cure of the balance of the perversions;" "destructive, indifferent and antiphlogistic contacts of

nitrate of silver;" "a case strongly tinctured with carcinoma;" "the generical habits of nutrition of the womb;" "hœmatomatous degeneration;" "the indefeasible tendency of all soft parts to consolidation;" etc. etc.

Before concluding we give one of Dr M.'s cases, in which bad description and Quixotic treatment are both exemplified :—

"In one case, a lady from the distant South, and in the most wretched health, had the excavation so filled up with a hypertrophied and solidified mass, that there was scarcely space sufficient to allow an introduction of the index finger betwixt this mass and the floor of the pelvis upon which it rested. The neck and os were near the symphysis. It was a case of complete immobility of the uterus, which seemed as big as a fœtal head in a labour at term. By means of a Para gum-elastic bottle, a very small one, stuffed with mattress-hair, we made an oviform pessary, a little bigger than the thumb. This was forced into the vagina, and retained by an outer compress. In a few days, one somewhat larger was substituted. The elasticity or spring of the caoutchouc bottle steadily pressed the mass upwards, and the size of the balls being occasionally increased, we had the satisfaction to find, after no very protracted service, that the intro-pelvic tissues had acquired a natural character, and the lady returned to her country in very good health, which she enjoys."—Pp. 70, 71.

In spite of the great preponderance of faults in Dr Meigs's book, it is one that will interest the obstetrician from the very carelessness with which the author throws aside every trammel, whether scientific or literary, and indulges his strong American common sense in an erratic excursion over the whole field of uterine pathology. It cannot fail to be suggestive, and if its doctrines do not confuse and mislead the reader, its practical precepts may leave in the mind some useful impressions. The work exhibits great powers of description, much intimacy with disease, and general sagacity in treatment. The plates appended to it are cleverly executed, the colouring in some parts very true, but like the text they invite a lengthier criticism than we can find place for in our journal.

Clinical Lectures on Pulmonary Consumption. By THEOPHILUS THOMPSON, M.D., F.R.S., Physician to the Hospital for Consumption and Diseases of the Chest, etc. London, 1854. 8vo. Pp. 211.

LECTURES on pulmonary consumption! Let not the reader be surprised. Our author has claims upon us and must be heard. He is physician to the Brompton Hospital, his field of observation is very extensive, and diseases of the chest are the especial objects of his investigation. These lectures have been already published in the "Lancet;" they are now revised, and "at the recommendation of some of his professional friends," Dr T. Thompson here brings them more prominently before the notice of the medical world.

The practitioner will find nothing surprising in this work. Our author does not pretend to astonish the world by novel methods

of cure, and superior pathological theories; he is contented with a quiet enunciation of his opinions on the subject-matter of phthisis—opinions tested by experience, and founded thereon. Though a prolific press be ever ringing the changes on this subject, "à nous ecorcher les oreilles," yet we think our readers will thank us for giving them, in brief, the benefit of Dr Thompson's experience herein; the comparison of his opinions with those of equally experienced men cannot fail to be of value.

And firstly on the *Treatment of Phthisis*. The superiority of cod-liver oil over every other actual remedy is admitted by Dr Thompson; 600 gallons of it are annually consumed at the Consumption Hospital—a fact which tells its own tale. It is given in doses of one or two drachms at first twice a day, the dose being increased to half an ounce three times a day. "I have seldom found any advantage from going beyond this limit." Dr T. approves of the use of liquor potassæ, and mixes this at pleasure with the oil; of the benefit of alkalies in certain stages of the disease there can be no doubt. Sometimes the stomach will not suffer the oil, and then Dr T. rubs half an ounce or so of it as a liniment into the thorax night and morning; and "satisfactory results have been sufficiently frequent to authorise the measure."

Cod-liver oil has no pernicious influence in promoting either hœmoptysis or diarrhœa; if anything, its effects are rather astringent. A few drops of creosote often render the stomach tolerant of its use; infusion of walnut leaves (a remedy much vaunted by some foreign writers in scrofula) is an agreeable vehicle.

Here Dr T. pauses to ask if a remedy with such powers will ever fall into disrepute under the fluctuations of fashion; it has, it appears, already done so. Dr Percival, half a century ago, dispensed 50 or 60 gallons of it annually with surprising good results in cases of old people whose joints were stiffened, etc. "Except," he says, "bark, opium, and mercury, I believe no medicine in the materia medica is likely to be of more service."

Vegetable oils, almond, olive, Dr T. decides, have no important therapeutical influence; he never could attribute any benefit to them. Fish oils resemble each other in medical properties. Neat's foot oil gives results as favourable as cod-liver oil, and has been found to answer where this has failed; "it must be regarded rather as an equivalent than as a rival." Cocoa-nut oil appears an exception to the other vegetable oils; from late experiments, Dr T. finds it as efficacious as the cod-liver oil; it is cheaper, more palateable, and has been found of avail where the fish oil had failed.

Hœmoptysis, according to Dr T.'s experience, rather tends to retard than accelerate a fatal issue of phthisis, therefore he would not have undue haste used in arresting it. Acetate of lead is the most powerful of direct astringents; gallic acid is not so prompt and effectual. Turpentine is probably one of the most certain and

suitable remedies, in doses of about twenty minims, given at intervals.

In *diarrhœa*, Dr Thompson has found no remedy which has acted so satisfactorily as trisnitrate of bismuth, given at intervals of four or six hours in doses of five grains, combined with three grains of gum arabic and two of magnesia; it is not prompt in its action, and must be persevered in; its good effects are permanent; it gives a better tone to the digestive organs.

For arresting *night perspirations*, Dr T. speaks most highly of oxide of zinc, given in doses of four grains, with a like quantity of extract of henbane; he has used it of late "with most satisfactory results." For allaying *cough* "no remedy has acted so promptly and satisfactorily as the inhalation of fifteen or twenty minims of chloroform."

Respecting the *diagnosis of phthisis*, Dr Thompson, in his introduction, speaks of the impediments to a clear understanding of the subject of auscultation, caused by the defective language in use by which its phenomena are expressed, and by too great refinements in diagnosis. To what he here says we fully subscribe; but we cannot congratulate him on having suggested a satisfactory remedy for the first of these evils. It would not be hard to show, had we space, that his tabular view of auscultatory signs is leavened with some of the defects he condemns; for instance, Dr Thompson has a "crackling" for his *third* division of sounds; it consists of three or four dry sharp sounds, and signifies "unsoftened tubercle;" his *fourth* division of sounds is "crepitation," and resembles "the rubbing of a lock of hair between the fingers, or the sounds produced by salt thrown in the fire." Now, we appeal to any one's common idea of language, if the word "crackling" does not convey much more correctly to his senses the latter variety of phenomena than the former; authors, indeed, have ever adopted the term "cracklings of pneumonia." Crackling Dr T. makes to represent what is ordinarily called dry crepitation, or dry crackling rhonchus, but what on earth is the sound of salt on the fire but a dry crepitation? wherein, then, lies the distinction? Yet the former—crackling—represents "unsoftened tubercle;" the latter—crepitation—pneumonia. We cannot pursue this subject here in its details, but we will admit that our author's manner of dealing with it is correct in principle. We may just, passingly, remark, however, that Dr T. has erroneously exhibited, in this preface, as illustrative "of the contradictory character of the language," etc., the remarks of Skoda, where he states that "crackling is a *dry* sound, and indicates the presence of *fluid*," etc. If Dr T. will turn to page 129 of Dr Markham's Translation of Skoda, whence he derived his illustration, he will find that the statement is not Skoda's, but Fournet's, whose division of sounds Skoda is criticising.

Speaking of the movements of the chest in disease, Dr T. remarks that "in practice the attentive eye soon detects the difference of

form and movement associated with diseases to which I have referred." To this observation we fully subscribe. We have never yet been able to discover the practical utility of "chest-measures." Their accurate application is very difficult, and requires much experience; any *practical* information they convey may be equally well obtained by the eye. Where the difference in the movements of the two sides of the chest is great enough to justify the observer in drawing conclusions therefrom, it may readily be detected by the eye; where this organ is not able to detect the difference, its degree is so slight as to be of no account in diagnosis. We make these remarks, because we do not wish to see needless difficulties scattered in the student's already sufficiently encumbered path.

Dr T. has paid much attention to "waxy inspiration"—the interrupted respiration of authors. He considers it a sign "which rarely disappears, excepting when superseded by definite signs of phthisis, but that it often continues for years without the supervention of that disease; when phthisis does ensue, there is reason to apprehend rapidity in it progress." It is most frequently observed at the upper part of the left lung; "although patients in whom it occurs unassociated with other signs may be considered on the verge of phthisis, it is gratifying to feel authorised to assure them that they present no evidence of tubercular disease." If it be an indication of tubercular disease, it is reasonable to conclude that it is the earliest local sign with which we are acquainted. It should be always sought for in patients whose countenances are expressive of deficient or imperfectly aerated blood.

Prolonged expiratory murmur Dr Thompson considers a sign of much importance. In 2000 consumptive patients, it proved to be the most remarkable physical sign in 228; hœmoptysis had occurred only in 91 of these cases. When it is heard extensively, there is reason to dread that the disease will make rapid progress.

Dr Thompson devotes one lecture to the appearance of the gums in consumption, and lays considerable stress on the value of this symptom, and not unjustly, if his views regarding it are confirmed. In twenty-six men, twenty presented the "gingival margin," six were free from it; the twenty had distinct symptoms of consumption; of the six, five were free from consumptive symptoms. But in six of the twenty the streak was very faintly marked. It is much less frequently observed in females than in males, and more common in hospital than private patients. Its chief value consists in its very early appearance of the disease; its absence in men affected with doubtful symptoms of consumption may be held as a favourable sign; its presence "in woman is almost conclusive evidence of the existence of the tubercular element in the blood." "When in either sex it coincides with a pulse not materially altered in frequency by change from the sitting to the standing posture, the presence of phthisis may with high probability be assumed." The further advanced the disease, the more marked is this sign. The

streak may be perhaps the first indicator of the tubercular disease in some instances. Its absence, whatever be the pectoral symptoms, gives us reason to believe that the constitution is not extensively involved. This sign requires further investigation to test its real value.

We have thus briefly referred to some of the most interesting topics treated of in these lectures, and our readers may gather from the outline here given that the matter of them is essentially practical. We cannot close the volume without warmly recommending its careful perusal to the attention of student and practitioner.

Memoirs of the Life and Scientific Researches of John Dalton, Hon. D.C.L., Oxford, etc., etc. By WILLIAM CHARLES HENRY, M.D., F.R.S. London: Printed for the Cavendish Society, 1854. Pp. 249.

TEN years nearly have elapsed since the death of the great chemist John Dalton, and fears were beginning to be entertained that he would be added to the list, already far too long, of uncommemorated British philosophers, when the work, whose title heads this page, was announced as in preparation.

That it was not sooner given to the world was not the fault of the author, who was, to all intents, left literary executor by Dalton, but did not obtain the necessary documents till last spring. He lost no time in making use of the papers, and the result is before us, adding another to the valuable works which the Cavendish Society has been the means of making public.

Dr Henry's qualifications for acting as biographer of Dalton, who, in effect, selected him for the office, are such as are found united in few writers of lives. The son of the late Dr Henry of Manchester, he was educated under the affectionate eye of a man of great general accomplishments, skilled in most of the physical sciences, and a chemist alike original and profound. For several years in his fresh youth, he was the pupil of Dalton, whose regard for his present biographer was shown by the bequest to him, long after, of his chemical and other apparatus, besides an equal share with his three formal executors, in the disposal as they judged best, of his literary and general manuscripts. In later years Dr Henry graduated in medicine at Edinburgh, and prosecuted the study of natural philosophy, especially of chemistry, in the best schools on the Continent, from which he returned to a life of studious leisure, of whose fruits we trust this work is but an instalment.

Our space does not permit us to enlarge upon the merits of the Memoirs before us, but we can find room to say that in four respects it is a volume of great interest:—

1st. It contains a full and very valuable account of the career of Dalton as a natural philosopher (which title we use in its widest sense), with a detailed recital of the steps that led him to the discovery of those great laws of chemical combination, which, taken as a whole, are generally named the Atomic Theory.

2d. A comprehensive and very interesting historical sketch is given of the opinions which have prevailed from early times regarding the existence of such things as Atoms, and we need not urge that the question is one of importance to all the physical sciences.

3d. An equally full, and not less interesting account is given of the bearing of the great discoveries in chemistry, as well as in other branches of physics, which have been made since the announcement of the Atomic Theory, on the truths which it announced. This portion of the work, as well as others, is enriched by letters to Dalton or his biographer from Sir Humphry Davy, Berzelius, Faraday, Liebig, Herschel, Babbage, and others, whose estimate of the worth of Dalton's views is of abiding value. Dr Henry himself criticises his master's opinions with the affection and reverence of a loving pupil, but at the same time with a candour in stating objections to his doctrines, and an unfailing impartiality in weighing the latter against opposing views, rare in the disciples of a great teacher, and still rarer in biographers.

4th. The personal history of Dalton is related with all the details which it has been possible to collect concerning it. His life was not eventful in the romantic or heroic sense, but like the life of every other honest, earnest, gifted, and original thinker and worker, its incidents are full of interest and instruction. Nor are amusing details wanting, all the more amusing for the grave character of those who are concerned in them. But for these and other particulars we must refer the reader to the work itself, from which he will learn, that Dalton was, if not a poet, at least a rhymer; that he *had* a heart, and fell in love once, if not oftener; that he liked a Saturday in the middle of the week, and spent it in playing bowls; that he was frugal and temperate, but did not object to an evening pipe of tobacco; that he was an indifferent lecturer and a clumsy experimenter; that he was unable to distinguish colours from each other, and was in consequence defended from the charge of inconsistency as a member of the Society of Friends in wearing a scarlet doctor's gown, because scarlet appeared of "the same colour as dirt" to him; that in this gown he was presented at Court, where he was taken by the majority of those present, for a provincial mayor come up to be knighted, and excited the envy of the courtiers by the special recognition taken of him by William IV. Many curious particulars will also be found concerning his early schoolmaster days; his cordial welcome to Paris by the philosophers there, whilst still little known in his own country; his hearty reception by the British Association; the circumstances attending his receipt of a pension from government;

his sitting to Chantrey for a bust; his serene life; peaceful death; and lasting place of honour among us.

M. Flourens is about, we understand, to read an Eloge on Dalton to the French Academy. The eloge will be chiefly founded on Dr Henry's memoirs, which will form a permanent and noble monument to the memory of the great English chemist.

Part Third.

PERISCOPE.

PHYSIOLOGY.

PROFESSOR PURKINJE ON THE TOPOLOGY OF SENSIBILITY.

This eminent physiologist has published an interesting paper on the above subject, in which he discusses various sensorial phenomena, with the view to ascertain whether the point whereat the sentient organ receives the impression—be it of a visual, auditory, olfactory, gustatory, or tactile nature—is the precise point of the organ wherein sensibility is excited. Our space will only permit us to allude to the experiments which he made on himself to elucidate the *topology of cutaneous sensibility*.

In allowing a shower-bath douche to fall on the whole dorsal region, while his body was in the upright position, he felt, at first, merely the sensation of a current directed from above downwards; but this sensation was succeeded by another of an ascending current, and this by a new one of a descending stream, and so on. He then allowed the shower douche to fall on his back, while he was lying in a horizontal position, so that, by all the drops of water falling upon the skin perpendicularly and simultaneously, any error which might have been caused before by the direction of the running fluid might be entirely obviated. Yet, even under these circumstances, he still felt very distinctly the two alternating currents between the nape of the neck and the loins, and these sensations continued during the whole time of the bath. How is this subjective sensation to be accounted for? To suppose that the impression produced on the skin of the whole dorsal region occasions first an exaggeration, and immediately thereafter a diminution of its sensibility, will not explain the alternating sensations, and the perception of the two alternate and opposite currents. Let us suppose, however, that the sensibility varies at different points of the back—for example, that it is greatest at the cervical, and that it gradually lessens towards the lumbar region. We can understand, by such an hypothesis, that the simultaneous fall of the water-drops will be felt first and most vividly at the nape; less quickly and less distinctly towards the loins; that the sensibility, blunted for an instant in the upper part of the back, will thus have time to recover its primitive intensity; and that the succession of sensations may be repeated indefinitely. But this is not enough, for the succeeding impressions ascend from the loins to the nape; we must, therefore, conjecture that the sensibility of the lumbar region, as well as that of the intermediate points, rests and recovers itself more quickly than that of the neck. For example, supposing the duration of this repose of sensibility in the back, from above downwards, could be specified thus—6, 5, 4, 3, 2, 1, 2, 3, it would follow that the *pause* or diminution of sensibility would last six units of time at the neck, but only three at the loins, whence, revived thus speedily, this sensibility would again ascend to the cervical region. This hypothesis, however, would lead to great confusion of the tactile impressions, did we not, at the same time,

admit that sensibility and reaction proceed increasingly from the one extremity to the other, according to the following formula :—

6	5	4	3	2	1
5	**6**	4	3	2	1
4	5	**6**	3	2	1
3	4	5	**6**	2	1
2	3	4	5	**6**	1
1	2	3	4	5	**6**

and so on conversely. This supposition is supported by the analogy of the peristaltic and anti-peristaltic movements of the intestines, observed in animals, when the abdominal cavity is opened. Moreover the skin is more contractile on the posterior region of the trunk than it is elsewhere; and there in various animals (as cats, dogs, hyenas, and birds), the hairs or feathers are possessed of the highest degree of erectility.

In order to ascertain whether other parts of the body are endowed with the same kind of sensibility, Purkinje made a similar series of experiments on the anterior part of his body, by means of two straight tubes, perforated by a series of holes. He obtained thus phenomena analogous to those described, but less distinctly marked, while on the face, the cranium and the extremities, they were not perceptible. As regards the influence which the temperature of the water exercised on the duration of the ascending and descending currents, he found that when the temperature of the atmosphere was 17° Reamur, that of the skin 24° R., and that of the water 15° R., that at least 12 seconds elapsed before the return of the current could be felt. On raising the temperature of the fluid to 36° R., (and allowing a diminution of 5° of heat during the fall of the drops), he found that only 8 seconds were required for the manifestation of the phenomena. Hence he concludes that the colder the water the more time is requisite for the restoration of the cutaneous sensibility, or the relaxation of the contracted fibres of the skin.

When repeating these experiments, Purkinje remarked that the sensation of the current, generally described the form of an ellipse, the two lateral halves of which seemed to be simultaneously, but inversely traced from the cervical to the lumbar region. In certain cases, it seemed to him that the two halves of the ellipse communicated an unequal sensation, and did not exactly correspond to each other, which probably was due to some inequality of the temperature. He considers the elliptic form of the current to be *en rapport* with the fact pointed out by Weber, that in the median line of the dorsal region, the skin is possessed of the lowest degree of sensibility, and that the parts most susceptible of impressions are those situated on either side of the spinal column, between the lower end of the cervical, and the conclusion of the lumbar portions,—which form, in fact, a sort of ellipse.

He then set himself to work to ascertain whether the fluid used in these experiments (viz. water), had anything to do with the production of these singular phenomena. For this end, he allowed river sand to fall through a sieve, upon his vertebral column ; and the same sensations of the double currents were experienced. Analogous results were obtained when he used very fine leaden shot; but when either of these substances were allowed to fall from a considerable height, the sensations of the currents became indistinct, probably on account of the pain thereby occasioned. A stream of air thrown forcibly upon the back, by means of a peculiar mechanism, as also a sort of continuous friction or brushing of the parts produced similar results.

Purkinje concludes his paper by suggesting that these phenomena may be of some utility in the diagnosis of certain morbid conditions, as well as in deter-

mining the choice of different kinds of baths.—*Vierteljahrschrift für die Praktische Heilkunde: Jahrg.* 1854. *Bd. I.*

MOLESCHOTT ON THE FUNCTIONS OF THE LIVER AND SPLEEN.

According to Liebig, the bile is an intermediate product, by means of which certain elements of the body are transformed into carbonic acid and water. If this doctrine be true, it ought to follow that animals, from which the liver has been removed, should give off, in a given time, and in proportion to their weight, less carbonic acid than animals in which the organ has been left intact. Dr Moleschott removed the liver from a number of frogs, and succeeded in getting a few of them to survive the operation. The most of the surviving frogs lived only three days, but some of them even for fifteen days. Moleschott then placed the frogs thus mutilated into an ingenious apparatus, which only admitted sufficient air to maintain their respiration, and in which the carbonic acid expired was absorbed by a contrivance of Liebig's, and transformed into carbonate of potash.

He began his experiments by placing in the apparatus as many sound and uninjured frogs as weighed, collectively, 100 grammes.

The carbonic acid exhaled by these 100 grammes of frogs, in 24 hours, averaged 430 millegrammes. He next experimented with 100 grammes of frogs deprived of their livers, and found that, in the same space of time, they only excreted, on an average, 192 millegrammes of carbonic acid. The author chose for his experiments, as far as possible, animals similar in their age, sex, condition, etc. He also extended his researches, in a similar manner, to animals deprived of the spleen, and found that the quantity of carbonic acid expired by them, in a given time, was to the amount of the gas given off by uninjured healthy frogs, as 1 to $1\frac{2}{5}$, thus proving that the spleen has less influence than the liver.

Moleschott also observed, that the blood of those frogs from which he had removed the livers contained *no sugar;* and this fact he considers confirmatory of Bernard's opinion, that, in the production of sugar, the liver does not act as a simple secreting organ, separating the sugar from the blood, as urea is thrown off by the kidneys. Indeed, we know that, after the removal of the kidneys, the blood becomes surcharged with urea.—*Müller's Archives, Jahrg.* 1853.

WAGNER ON THE TERMINATIONS OF THE NERVE-FIBRES.

This physiologist has recently made a communication to the Institute of Göttingen, on the mode in which the sensitive nerve-fibres terminate in certain tissues.

In the teeth Wagner has observed them ending in free extremities. The loops frequently seen are not their terminations, for, uniting in a plexus, they form a nerve-fibre, whose free extremity ends in another place. The primitive fibres are rarely divided.

In the tongue there are no terminal loops; the primitive fibres do not unite, but proceed singly side by side, and their double contour ceases abruptly. He has seen the fibres occasionally prolonged, but containing no nervous substance. He found no touch-bodies in the fungiform papillæ.

In the labyrinth he found three kinds of fibres: 1st, Those having free terminations; 2d, Arciform fibres forming loops, which end in an unascertained manner; 3d, Extensively ramified fibrils, probably communicating with the ganglionic cells. There are a great many of these cells in the vestibule, some of which are apparently bipolar. Wagner thinks that there exists here a disposition of the nervous filaments analogous to that described by Müller and Kölliker in the retina.

In the glands the termination of the fibres in loops cannot be demonstrated.

Wagner thus sums up the results of his researches:—"The sensitive nerve fibres, as well as those which govern the development of the tissues (the "trophic" fibres), are arranged just as those which are motor; they frequently

divide, form no terminal loops, and end in free extremities, which are either lost in, or cease to exist with, the elements of the surrounding tissues; or which join the ganglionic corpuscles, or end in the pacinian bodies."—*Schmidt's Jahrb.* 1853. Bd. LXXIX., Hft. 7.

PROFESSOR FICK ON THE TOPOGRAPHY OF ANIMAL HEAT.

A series of interesting experiments has been performed by Dr Fick, with the view of ascertaining the relative temperatures of the different organs and regions of the body. He gives the details of twelve experiments made upon dogs, in which he carefully noted the temperature by placing the thermometer in the carotids, the jugulars, the right and left ventricles, the interior of the thorax, the vagina, the urethra, the rectum, and the brain. Our limited space precludes us from entering into the minutiæ of Dr Fick's paper; we may mention, however, two interesting points which his researches have established. The first is, that there does not exist (as has hitherto been supposed) a perceptible difference between the temperature of the right and left ventricles of the heart. And, secondly, he has shown that the highest degree of temperature is to be found in the vagina and rectum, where it ranges from 105·75° to 105·79° Fahrenheit—that is, 1·12° higher than in any other region of the body. The animal heat of the rectum is always higher than that below the tongue, when the mouth is closed; indeed the difference of temperature between these two parts may even be as high as 3° F.—*Müller's Archives.* 1853. Hft. 3.

DR KIERULF OF CHRISTIANA, ON THE INFLUENCE WHICH DILUTION OF THE BLOOD HAS ON THE URINARY SECRETION.

This author made a section of the left ureter in various dogs, and he collected the urine secreted by means of a glass tube. This operation was performed on the first dog between 8.55 and 9.40 A.M., with very trifling loss of blood. Shortly before the operation the dog passed a considerable quantity of normal straw-coloured urine. At 9.45 a small quantity of blood was abstracted from a vein; and at 11.40, 495 grammes of distilled water, at the temperature of 32° R. (104° Fahr.), were injected into the jugular vein. Five minutes thereafter, the urine was mixed with a considerable quantity of blood, and its amount was also increased. A second bleeding was performed at 11.55, and the urine was collected till 4.9 P.M., when it contained blood, but in a very small quantity. At 4.50 the dog was bled for the third time, and at 5 o'clock some sanguinolent urine was passed by the intact ureter. Next day the urine was of the normal clear straw-colour.

Other experiments of a like nature gave analogous results. He instituted other researches to ascertain whether the mixture of blood in the urine was due to an increase of the pressure of the circulating fluid, or to some change in its composition. He found that the urine passed after an injection of defibrinated blood, into the veins, was perfectly clear. Dr Kierulf makes the following deductions from his experiments:—

1*st*, That *considerable dilution of the blood causes at first the excretion of albumen by the kidneys, and thereafter an admixture of blood in the urine.* This latter effect he regards as due neither to increased pressure of the blood, nor to rupture of the capillaries in the kidneys.

2*d*, That the injection of water into the circulation increases the salts in the blood, although it diminishes the solids in the urine.

3*d*, That the quantity of urine secreted in a certain time does not correspond to the proportion of water in the blood.

The author considers that these experiments, or others analogous to them, may yet cast light upon the obscure pathology of Albuminuria.—*Zeitsch. für Rationelle Medicin, von Henle ünd Pfeuffer. Bd. III. Heft* 3. 1853.

PATHOLOGY AND PRACTICE OF PHYSIC.

REPORT OF CASES TREATED IN PROFESSOR HEBRA'S CLINIQUE FOR SKIN DISEASES, IN VIENNA.

Dr Wertheim has recently published, in the Vienna *Zeitschrift der K. K. Gesellschaft der Aerzte*, an elaborate report of the cases (2961 in number) treated in Hebra's wards during the year 1852, of which the following is a condensed account :—

I. VARIOLA.—*Its average mortality; varieties; its contagiousness; effects during pregnancy; immunity of child whose mother has had the disease during its intra-uterine life; effects on menstruation; protective power of vaccination and previous infection; abnormal cases; treatment.*

For this disease 517 patients were under treatment (253 males, 265 females), of whom 32 died, *i.e.*, 6·18 per cent. of the whole.

Of the fatal cases, 11 occurred among men, and 21 among women, which gives an average mortality of 4·36 per cent. for the males, and of 7·32 for the females. This great difference arose from the circumstance of 22 of the female patients being either in the pregnant or puerperal state, of whom 9 died in the institution. The author considers the results of the year's practice confirmatory of the opinion that variola vera, v. modificata, and varicella (which are often regarded as totally distinct and different), are essentially identical with one another.

In 77 of the 517 cases the disease could be clearly traced to contagion.

Among the 265 women there were 16 pregnant, 5 of whom were delivered in the small-pox ward; 1 in the seventh, 3 in the eighth, and 1 in the ninth month of gestation. The patient in whom premature labour occurred in the seventh month had intense variola, which occasioned it. The event occurred during the decrustation stage, and the infant when born was covered with pustules.

To elucidate the interesting question whether a child is proof against the contagion of small-pox, whose mother has had variola during her pregnancy, the following experiment was made :—

Franziska Tuczek, æt. 35, in the seventh month of gestation, was admitted on the 12th February, for variola vera ; and left the institution recovered on the 8th March. On the 19th April she gave birth to a boy, whose skin was perfectly healthy, exhibiting no trace whatever of the variolous eruption. On the 18th May vaccination of the boy was attempted with vaccine lymph (which had been tried successfully on another child of the same age), but without any effect being produced, although eight punctures were made. On the 25th May, a second vaccination was attempted, under similar circumstances, but again with the same negative results.

The frequent co-existence of variola with the pregnant and puerperal states, led Hebra to observe whether the disease had any specific effect on menstruation. In 38 cases menstruation occurred simultaneously with the small-pox ; in 20 of these it happened at the normal period (viz., 4 weeks after the last catamenial flow) ; in 11 cases it appeared from 8 to 14 days too early, and in 7 cases, from 8 to 14 days too late. In 27 cases he found the menstruation to be entirely suspended.

Hebra believes variola to be three times more common among those who have never been vaccinated, nor had small-pox, than among those who have either been vaccinated, or have once been attacked by the disease.

Several abnormal cases are reported. One woman, who had been vaccinated, took *variola vera ;* and in her the decubitus occasioned a sore which exposed the right trochanter, and abscesses in the gluteal muscles, so that she died of pyæmia. In 5 cases *variola hæmorrhagica* occurred among non-vaccinated persons, who had taken the disease for the first time ; and the symptoms were especially severe on the trunk. In one of the cases, in which the symptoms were

very severe and long continued, the apices of the lung became infiltrated with tubercle, during the suppuration stage of a pneumonia which had supervened, and which carried off the patient. The most serious case admitted during the year, was that of a woman suffering under the disease in the stage of decrustation. The whole surface of the body was covered with dark brown and very fœtid crusts, which concealed deep confluent ulcers, and considerable accumulations of pus; the corneæ of both eyes were softened and perforated by pus escaping from behind; there was present a high fever, and an intractable diarrhœa. Death ensued with pyæmic symptoms. In a young woman, æt. 25, there appeared, twenty-four hours before death, the very unusual complication of an eruption of the bullæ of pemphigus. In another case *diphtheritis cutanea*, to about the extent of a hand's breadth, occurred in the mammary region, conjoined with pleuro-pneumonia, and intense inflammation of the glandular parenchyma. The disease did not extend, and the patient recovered.

The *treatment* of variola by Hebra, is generally of an expectant character. The use of artificial sulphureous baths was tried, by way of experiment, in all the stages of the disease; but as no satisfactory therapeutic results were thereby obtained, their employment was soon abandoned, and expectant treatment was resorted to again. When pustular ophthalmia occurs, he finds the use of the solid *argenti nitras* beneficial; and in the conjunctivitis which often supervenes, he employs collyria containing alum conjoined with some narcotic.

II. Scabies.—*Its treatment; Trial of Hardy's method in* 50 *cases; Results.*

The mode of treatment proposed by Hardy, as capable of curing scabies in two hours, was tried in 50 cases. The patients were, first of all, made to rub the whole body, with the exception of the face, with greasy soap (schmierseife); immediately thereafter they were put into a tepid bath for an hour, during which time they diligently cleansed themselves with soap. On leaving the bath they were made to rub themselves forcibly for another half-hour with itch ointment on all the places where the *acari* were known or suspected to be, and after lying an hour, in coarse sheets, another bath is taken. According to Hardy they may then be dismissed cured. But no such success occurred in these cases. The very vigorous frictions with the coarse soap produced excoriations where none previously existed, and heightened those which were present, and so aggravated the general symptoms that patients thus treated had to remain much longer in hospital than those who had been treated in the usual manner. A modification of this treatment was successfully tried, which consisted in rubbing the affected parts with soap or sulphureous ointment for two days, *without using the tepid bath immediately* (but only after the expiry of this period), by which means the troublesome eczema and irritation were found to occur much less frequently.

III. Eczema.—*Universal and local; sites of local variety:* Treatment,—*Cold epithems; ung. sulph iodidi; tar and oleum cadini; new mode of cure by warm baths only.*

For this affection 273 individuals were treated in Hebra's wards during the year; of whom 192 were men, and 81 women. In 12 cases the eczema was universal, in the remainder partial. Among the former class of cases are reckoned not only those in which the disease is everywhere present at the same time; but those also in which the eczema beginning in a certain region of the body, extends thence by a steady and symmetrical progress to all the other parts in succession. In two cases in which this occurred the patients (a girl æt. 20, and a boy æt. 15) stated that the disease commenced on the hairy scalp, that it extended thence to the neck and the interscapular regions; that it then proceeded down the back, and spread along the upper and lower extremities. The eczema when *local* was found in 42 cases on the hairy scalp; and of these 42 patients, 30 were females, in most of whom the uterine functions were deranged. The remaining cases of local eczema occurred chiefly on part

exposed to the air, as the hands and face, or on those which were subjected to friction or pressure from the dress,—as those pressed on by the stays and garters in women, and by the hat and boots in men.

Treatment.—In recent and in mild cases cold epithems are used, care being taken that the applications are not *too* damp, in order to avoid the artificial eczema often produced by long-continued moisture. In cases of longer standing cold applications alone are insufficient to effect a cure; and with their use must be conjoined the employment of other local applications. These consist of coarse soap, diluted and concentrated solutions of potash, and an ointment of iodide of sulphur. For this ointment Hebra gives the following formula:—"℞ Sulphuris Iodidi ℈i. Unguent. Simpl. ℥iij. Misce." This ointment is rubbed on the parts affected, twice or thrice daily, and the cold fomentations are continued at the same time. When by this treatment the existing inflammation is subdued, and there appears, as is often the case, a form of *pityriasis rubra*, then birch-tar, or latterly, *oleum cadini* (*Huile de Cade*, obtained by dry distillation of the wood of *Juniperus Oxycedrus*) are applied. The latter remedy causes less irritation of the tender parts. Both substances are thus used:—they are carefully applied by means of a camel-hair pencil to the parts affected, which are then allowed to remain uncovered for an hour or two, till they become dry, and a thin transparent pellicle is formed. Cold epithems are no longer necessary, unless there be considerable tenderness. The application of the ol. cadini is renewed so soon as the previous layer is wholly or partially removed; and this is repeated until the skin below recovers its normal condition.

Hebra tried a new method of cure in two cases of universal eczema, which in both cases had existed for a year. During a period of fifteen weeks he made the patients remain eight hours daily, and even take their meals, in a tepid bath. This plan of treatment was liked very much by the patients, and completely cured them in the time mentioned.

(*To be continued.*)

DR TH. VON DURSCH OF MANNHEIM ON DIABETES MELLITUS.

Probably one of the most elaborate papers ever written on this disease has just been published by the above author. It is founded upon two very interesting cases of diabetes, of which he has given very careful clinical reports, and also accounts of the pathological lesions found after death. With indefatigable perseverance and most praiseworthy zeal, he carefully ascertained, every day for several months, the nature and amount of the food and drink taken by his patients; the amount of fluid contained in the aliment; the quantity of urine excreted, its specific gravity, and the amount of sugar it contained; the number of the stools, the proportion of their watery constituents; the amount of water exhaled by the lungs and transpired by the skin, etc. The results of these most laborious and minute investigations he has condensed into two large synoptical tables, which are appended to the memoir. In one of these full particulars are given of the effects of different kinds of diet upon the total amount, specific gravity, and saccharine constituents of the urine. As far as our limited space permits, we shall now glance at the general results thus tabulated; *first*, when the patient was put upon a mixed diet for forty days; *second*, when a farinaceous diet alone was allowed during eight days; and *thirdly*, when animal food was given during a period of five days.

1. *Effects of mixed diet on*

(*a*). *The specific gravity.*—The average density of the urine, while this regimen was adhered to, was 1037·8; it was higher in the mornings and evenings (1038) than during the day (1036).

(*b*). *The amount of the urine.*—The daily average was 5234 cubic centimetres. The quantity voided was greater in the morning (1971 c.c.) and evening (1331 c.c.) than during the day (1430).

(*c*). *The per centage of sugar.*—The average amount of saccharine matter was 9.134 in the 100 grammes. The per centage was lower in the morning and mid-day urine (8·9) than in that passed at night (9·4).

(*d*). *The total amount of sugar.*—The average daily amount of sugar excreted during the whole period was 477.7 grammes; the lowest was 350, and the highest 615 grammes.

2. *Effects of farinaceous diet on*

(*a*). *The specific gravity.*—This continued nearly the same as with mixed diet; the average was 1037·6. With this regimen also it was lower during the day than at night or morning.

(*b*). *The amount of the urine.*—This was increased to 5604 cubic centimetres as its daily average. It was greater in the morning (2165 c.c.) than during the day (1737 c.c.), or at night (1701).

(*c*). *The per centage of sugar.*—This continued nearly unchanged, being on an average 9·39 in 100 grammes.

(*d*). *The total amount of sugar.*—In this a considerable increase was visible, while the farinaceous diet was continued. The average quantity of sugar daily excreted amounted to 526·4 grammes; and the urine in the morning contained more (201 gr.) than at noon (159g.), or at night (165 gr).

3. *The effect of animal diet on*

(*a*). *The specific gravity.*—It remained nearly unaltered by this regimen, as happened with both the other diets. Its average was 1037·2; and it was lower in the morning (1036) than at other times (1037).

(*b*). *The amount of the urine.*—This was considerably diminished, the average quantity *per diem* being 4588 cubic centimetres. The average amount was much greater in the morning (1816 c.c.) than during the day (1324 c.c.) or at night (1448 c.c).

(*c*) *The per centage of sugar.*—This was also lessened, being on an average 8·232 in 100 grammes.

(*d*). *The total amount of sugar.*—Here likewise a striking diminution was manifest. The average quantity daily excreted was 379·8 grammes; this was greatest in the morning (139 grs.), and less during the day (114 gr.) than at night (126 gr).

Dr von Dursch discusses several of the questions relative to diabetes, and brings to bear upon them the weight of his experience and careful observations. As regards the disputed point, whether the quantity of the urine voided in this disease surpasses the amount of the fluids absorbed, he thinks that the question has not been properly considered, and that we ought to compare the amount of water in the urine, etc., with that contained in the food and drink taken. During his investigations he ascertained the amount of the cutaneous transpiration and pulmonary exhalation by frequently weighing his patient; and he also carefully noted the quantity of water contained in the fæces. He has succeeded thereby in satisfying himself that the water given off by the patient equals exactly the amount of the water absorbed by him.

In conclusion, the author believes, from all his researches, that diabetes principally depends on the sugar normally existing in the blood being undestroyed and unappropriated; and he is of opinion that all kinds of food are capable of producing sugar.—*Henle und Pfeuffer's Zeitsch. für Rationelle Medicin.* 1853. Bd. iv., Hft. i.

CASES OF GANGRENE OF THE LUNG SUCCESSFULLY TREATED BY INHALATIONS OF TURPENTINE. BY PROFESSOR SKODA OF VIENNA.

These cases are three in number, and Dr Skoda has made use in each of them of the vapours of turpentine, along with sulphate of quinine. The first case is that of a domestic servant, who was affected with gangrene limited to the upper lobe of the right lung. After six weeks of this treatment all the physical signs had disappeared, and vesicular respiration free from râles was audible. The patient was seen three months later and remained perfectly well. The subject

of the second case was an innkeeper, of strong constitution, somewhat advanced in life, in whom a gangrenous cavity existed in the lower lobe of the right lung. The disease, dated from the 11th of March 1852. On the 21st of March, inhalations of turpentine were commenced; the patient employed them without any repugnance for five or ten minutes every two hours, and in addition took every two hours a grain of sulphate of quina. In three weeks the expectoration which had been very abundant was reduced to a sixth. In six weeks the patient could leave his bed, but the expectoration continued sanious and of a fœtid odour. His strength returned, his appetite improved, and in the month of June his general appearance was satisfactory, although cough and expectoration, occasionally fœtid, continued. The right side of the thorax was painful, the breathing difficult, but dulness on percussion had entirely disappeared; the respiration was indeterminate and accompanied by feeble sibilant râles. The patient went to the country and continued the turpentine inhalations twice a day till the month of July, by which time the cough and expectoration had entirely disappeared. In December he had perfectly regained his flesh and strength, was free from cough or pain, no retraction of the thorax had taken place, and vesicular breathing was everywhere audible. The third case was that of a butcher, suffering from gangrene of the lower lobe of the left lung. He turned ill towards the end of May 1852. On the 4th of June, he began to employ turpentine inhalations. Within eight days the fœtid expectoration had ceased, and the patient believing himself recovered as the pain and oppression had diminished, ceased the use of the turpentine, as it was disagreeable to him. On the 19th of June a violent shivering came on, accompanied by cough, dyspnœa, and fœtid expectoration. The inhalations were resumed, and gradual improvement took place. They were, however, again suspended, but a fresh exacerbation occurred. His appetite was gone, and his emaciation and weakness was such that about the end of July he could not leave his bed. Fowler's solution was then had recourse to, and under its influence as well as that of the inhalations, the patient recovered so far that he could leave his bed and go to the country. By the middle of October he had regained flesh and a considerable degree of strength, but a feeling of pain and oppression remained below the scapula and in the left side of the thorax, while there was a little cough with slight fœtid expectoration from time to time. Vesicular respiration was audible to the base of the lung, but dulness continued to occupy an extent equal to the size of a pleximeter, in the neighbourhood of the apex of the heart; at this level faint râles were audible. The turpentine inhalations were ordered to be continued. At the end of January the patient believed himself cured, for ten days he had had no chest symptom, when on the 10th of February, after a slight fit of coughing, he expectorated two ounces of blood, and after it some fœtid pus. There were no signs of a new infiltration. The blood probably came from the gangrenous cavity, the walls of which had become callous.—(*Zeitschrift für K.K. Gesellschaft der Aertze zu Wien, in L'Union Médicale*, Feb. 16, 1854.)

[The inhalations are practised by pouring a little oil of turpentine upon boiling water, and respiring the vapours.]

BETZ ON THE THYROID ASTHMA OF YOUNG CHILDREN.

Medical men are divided on the subject of thyroid and thymic asthma; some acknowledging, and others altogether denying, their existence. Alois Bednar, for example, in his recent work (*Krankheiten der Neugeborenen*, Pt. 3, page 77), does not believe in the existence of asthma resulting from hypertrophy of the thyroid gland alone. He relates ten cases of thyroid enlargement, nine of which were attended with asthmatic symptoms; but in all of these nine cases there existed other pathological affections of the air-passages, sufficient to account for the dyspnœa. He alleges that the thyroid gland may be enlarged without the respiration being affected, but confesses that most cases of hypertrophy are attended by a difficulty of breathing; and he considers the dyspnœa so induced as not asthmatic in its character. Friedrich Betz, of Heilbronn,

has published an interesting paper on the subject, in which he regards the impediment to the entrance of air, in cases of thyroid hypertrophy, as existing neither at the upper part of the trachea, nor in the larynx, but higher up, at the entrance to the air passage, where the enlarged gland projects into the floor of the mouth. The thyroid body has this peculiarity of conformation in newborn infants, so that both its lateral lobes extend backwards, between the pharynx and the vertebral column; and, according to Albers (*Constatt's Jahresbericht*, 1848), the lobes may even become blended together, so as to form a fleshy ring round the larynx and top of the pharynx. When, therefore, its extremities become swollen they constrict the parts enclosed within them.

This affection is easily recognised by swelling of the upper part of the neck, and sensible enlargement of the thyroid body. Expiration and inspiration are both stridulous; the sounds continue without intermission, only remitting during sleep. The mouth is filled with frothy saliva; the hands, feet, lips, and face, are cold, and bluish; there is no fever; there is an occasional paroxysm of coughing; and the sleep is short and broken. Suckling is impossible, for the attempts to swallow fluids increase the suffocative dyspnœa. In severe cases, death occurs after from one to four days; in those which are milder, the swelling gradually subsides, and with it the dysphagia and dyspnœa. It assists our diagnosis to remember that this disease is limited, the first eight to fourteen days after birth.—*Jour. für Kinderkrankh*, 1853, Sept. and Oct.

PROFESSOR BERG ON CAPILLARY HEMORRHAGE IN THE BRAIN OF AN INFANT.

The above author reported to the Association of Swedish Physicians at Stockholm the particulars of a case of this occurring in an infant, which died of capillary bronchitis, of five or six days' duration, in the hospital for children in that city. During this illness there were manifested no symptoms whatever of cerebral affection. After death, the pia mater was found to be congested; the surfaces of the cerebral hemispheres were sound, but the whole of the white substance was found softened into a granular mass by extensive capillary hemorrhage. The *ramollissement* extended completely through both hemispheres, and was most complete at their posterior parts, which were of a panada-like consistence. The crura cerebri and cortical substance were not affected. Berg remarked that it was wonderful that all this mischief could have existed without the appearance of any physical symptoms during life. —*Journal für Kinderkrankheiten.* 1853. Bd. XXI., p. 141.

PROFESSOR BERG ON PYOPNEUMO-THORAX IN A YEAR-OLD INFANT.

This case was that of a child, brought to the Stockholm Hospital, having, on the left side, half an inch under the margin of the ribs, a tumour the size of half a walnut. In respiration the tumour became enlarged, and a gurgling noise was heard in it, even at a considerable distance from the patient. Auscultation revealed a metallic sound (*metallklang*) in the tumour, in addition to the gurgling râle; and the left lung was found normal, but having a very weak vesicular murmur. The tumour had appeared after a protracted bronchial catarrh; and Berg considered it to be a case of pyopneumo-thorax, only partially cured, in which the lung had become adherent to the thorax. He recommended for its treatment external pressure, to counteract its expansion, and the internal administration of cod-liver oil.—*Journ. für Kinderkrankheiten.* 1853. Band. XXI., p. 142.

EULENBURG ON THE SOLUTION OF GUTTA PERCHA IN CHLOROFORM (TRAUMATICIN)

This fluid, when applied externally, is very useful in the treatment of skin diseases. In a case of *Psoriasis diffusa* of a year's standing, its daily application, for a period of three or four weeks, caused the patches completely to disappear and the skin to heal. In two cases of eczema also, one affecting the axilla and the other the inner surface of the thigh, the author used it with

perfect success; the former case being cured in three, and the latter in four weeks.—*Medicin. Centralzeitg.* 1853. 20.

HESCHL ON PERFORATIONS OF THE INTESTINES IN TYPHUS.

During a period of ten years, according to Heschl's report, there occurred in the Vienna General Hospital 56 cases of perforation among 1271 of typhus. Of these 56 cases 31 occurred in the "stage of necrosis," 11 in later stages, and 14 were undescribed in the registers The perforations usually occurred in the necrosis stage; in the more advanced stages they seemed to originate in suppuration of the intestinal walls.—*Zeitsch. der Gesell. der Aerzte zu Wien.* June 1853.

SURGERY.

VIRCHOW ON THE GROWTH OF HEALTHY AND RACHITIC BONE.

(*Concluded from page* 460, *Vol. XVIII.*)

After the foregoing considerations on rachitis as it affects the extremities of long bones, Virchow proceeds to investigate the disease as it appears in their shafts, and also in flat bones, and in doing so he throws much interesting light upon normal bone-growth. He gives a detailed account of the minute structures observable during osteogenesis, and he sums up the general phenomena of the process thus: "The cartilage and periosteum both become developed towards the bone; and in the layers thus deposited medullary spaces and cancelli appear. These are formed by a gradual hardening (*sclerosis*) of the matrix, *i. e.*, of the capsular and intercellular substance, and by the cellular elements beginning to assume, or (where it pre-existed) continuing to retain, a stellate form. The formation of the medullary spaces takes place by the gradual softening of the fibrous matrix by the process of cell-development therein."

In rachitic bones, according to Virchow, we find, 1st. Increased growth and thickening of the periosteum, with progressive change of substance in the areolæ and bony network; 2d. Defective ossification of the cancellated structure, occasioned by continuance of the deep layer of the compact cortical substance; 3d, Partial cartilaginous development within the areolæ.

It is well known that rachitic bones are liable to become bent, and these distortions are generally said to be due to the influence exercised upon the softened osseous structures, by the weight of the body, or the force of muscular action. At the point of the curvature, as shown by Duvernay, the medullary canal is generally either very much narrowed, or totally obliterated. The important curvatures generally depend upon a powerful *infraction* of the rickety bone. Bones, in this disease, are peculiarly liable to fractures and infractions; indeed Guersant believes that three-fourths of all the fractures which occur during childhood are attributable to rickets. All authors agree that fractures and infractions may be present without being detected, and that they may often be confounded with simple curvatures. In many cases these fractures are merely partial, so that the fractured ends of the bone are maintained in close apposition by means of the uninjured periosteum. And we can easily understand that simple *uncomplicated* curvature does not very often occur, when we remember that below the soft young bone there exists a firm cortical layer of hard osseous substance. Distortion, however, is not always due to infractions or fractures; for it may result from unequal osseous development, or from certain diseased conditions of the cartilage, as enlargement and softening. The distortions of the vertebræ, ribs, and pelvic bones, are generally induced by the latter morbid conditions; the exciting causes of curvature in such cases being either the weight of the body, the pressure of opposite parts, or the force of muscular action, which, however, is very weak in rachitic children.

Infractions are chiefly found in the long bones of the extremities, and in the ribs; seldom in the scapula and clavicle. When they occur, the bone, at the

fractured point, is convex externally, and concave internally ; the medulla at this part is destroyed or extruded ; and the medullary canal becomes narrowed or obliterated. Callus formation begins in the concavity of the infraction, and often completely fills it. The callus, at first, slowly consolidates ; it then assumes a spongoid, and thereafter a spongy texture. Both halves of the medullary canal are often closed, meanwhile, by a layer of compact substance ; but, by a great transformation of all the structures at the part, a continuous canal for the medulla is generally established once more. From the resemblance between rachitic and traumatic fractures, it is evident that the bending of the bones in rickets is only part of a direct healing process, and that even the most extreme cases of curvature may be made amenable to properly applied treatment.

As regards the essential *nature of rachitis*, Virchow considers that it does not consist of a mere softening of the bones ; and that it is *no malacia*. Those osseous structures which are formed prior to the occurrence of the disease retain their original firmness, although those do not which are formed after its invasion. The characteristic rachitic curvatures appear in the cartilage which has either remained or become soft ; while the bones, on account of their thin cortical layer receiving no increase of growth from compacted structures, are peculiarly liable to infractions and fractures.

Chemistry has shown that in rickets there is a deficiency in the inorganic ingredients of the bones, the earthy salts being reduced to the lowest minimum. The microscope shows this to be due to arrest of the normal bone-growth, those calcareous matters not being deposited which give solidity to the osseous structures. This, Virchow thinks, may result from,—1*st*, Insufficiency or earthy salts in the nutriment ; or, 2*d*, From some impediment to the deposition, within the osseous structure, of the earthy salts in the nutriment, occasioned by some peculiar condition either of the blood or of the ossifying parts, or by some peculiarity in the circulation and nutrition of the bone itself.—*Virchow's Archive. für Pathol, Anatomie, und Physiologie.* Bd. v. Hft. 4, 1853.

PIROGOFF'S MODIFICATION OF SYME'S AMPUTATION.

The point in which Pirogoff's operation differs from that of Mr Syme, is the preservation of the posterior portion of the os calcis, which is left to fill up the heel-flap. The proceeding is obvious, and easy of execution. The incisions are made exactly as directed by Mr Syme: after practising the first from one malleolus across to the other, the operator need only prepare the integuments about a line or two backwards from the inferior surface of the os calcis; he then proceeds to the execution of the second incision across the front of the joint ; the astragalus is then disarticulated, and the os calcis divided by the saw behind the posterior extremity of the astragalus. The posterior extremity of the os calcis is preserved by this proceeding, instead of the whole being peeled out, as in Mr Syme's operation ; the heel-flap is thus completely filled out by the bone. The saw is then applied to separate the malleoli, and a thin slice of the articular surface of the tibia ; the cut surface of the last-named bone is then brought into apposition with the cut surface of the os calcis, and the skin-flaps accurately united by sutures. The advantages of this operation are obvious:—1st, We obtain a longer stump than by Mr Syme's operation, and the extremity of the stump is firmer, and far better adapted to bear pressure with impunity. 2dly, The healing process takes place quicker, and is less likely to be retarded and disturbed, as after Mr Syme's operation, the cavity of the heel-flap being filled up by the bone. 3dly, There is no danger of the heel-flap being deprived of the necessary supply of blood, as it will be easy to avoid dividing the posterior tibial artery high up in the wound, as often happens during the execution of Mr Syme's operation, even by the most skilful hand.—*Medical Times and Gazette.*

MIDWIFERY.

PAULSEN ON UTERINE TUBERCULOSIS.

The author considers this pathological condition to be comparatively rare, only two cases having occurred in the Copenhagen Hospital during ten years. On these two cases, and on four others, recorded by Reynaud, Louis, Boivin, and Duges, Paulsen founds an interesting monograph on the subject, a very condensed abstract of which we shall lay before the reader.

Pathology.—The author considers the following points well established :—

1st. Tuberculosis always occurs *primarily in the mucous membrane*, and thereafter secondarily in the parenchyma of the uterus. It occupies the mucous membrane alone in most cases. Its occurrence *solely* in the submucous or muscular tissues of the organ is doubtful, although the thickening of the mucous membrane by the deposit, and the thinning of the muscular layers consequent upon the expansion of the uterus, are very apt to mislead the observer as to its real site.

2d. Tuberculosis occurs either in the form of fine greyish granulations (which Rokitansky says is seldom), or as a simple infiltration into the inner layer of the mucous membrane, which ultimately softens, becoming a cheesy puriform mass, and destroying the walls of the organ.

3d. This destructive process may penetrate more or less deeply into the fibrous structure of the uterus, but perforation rarely occurs. As it advances, the walls of the organ become greatly hypertrophied, and its cavity distended by tuberculous exudation, so that its circumference is two or three-fold increased.

4th. Tubercle generally occurs in the fundus or upper part of the body of the uterus ; and in general it does not extend beyond the os internum. The cervix is occasionally affected,and then its mucous membrane becomes thinner than that lining the rest of the organ, and also changed in its glandular structure. When tuberculous, the author has observed the cervix become deranged in its follicular apparatus, discharging thin glutinous mucus.

5th. Although Rokitansky has never observed implication of the vaginal portions of the cervix, Paulsen considers that this certainly *may* happen, in the form of very small and fine granulations, or of erosions and small ulcerations, which result from infiltration of the follicles, and are arranged in little groups. This, however, he has never seen, and, like most pathologists, he believes that this part of the uterus enjoys almost complete immunity from the disease.

6th. Uterine tuberculosis is very often, almost generally, combined with a similar condition of the Fallopian tubes ; but it may occur *alone*, and uncomplicated. Where both conditions exist together, the tubal tuberculosis is always the primary disease, and extends to the uterus. The Fallopian tubes, when affected, are swollen and tortuous, firm and tense ; the structure of their walls is changed, becoming hard and white ; and the mucous membrane of their fimbriated extremities assumes the appearance of a cauliflower excrescence.

7th. Paulsen, like Louis, never saw tuberculosis of the vagina. In one case he observed extensive ulceration of it, but this was caused by the excoriating properties of the secretion from the uterus.

8th. The development of uterine tuberculosis seems never to extend over more than a year.

Diagnosis.—This disease of the uterus is liable to be confounded with the following pathological conditions of the organ :—

1st. *Purulent collections in its parenchyma*, where the liquor puris has been absorbed, and a whitish-yellow, cheesy mass remains, resembling tubercle in appearance.

2d. *Discoloured clots of blood.*

3d. *Fibroid masses*, the so-called " flesh-tubercles." These may contain pus in a fluid or concrete state.

4th. Lisfranc's *tubercular induration of the uterus*, a condition in reality connected with carcinoma or epithelioma.

5th. *Mucous follicles, granulations, or vegetations pathologically changed* in the canal of the cervix. The ovula of naboth are sometimes as large as peas, from being filled with a firm transparent substance.

Etiology.—The uterus is generally an organ *secondarily* affected, and this only in cases of very extensive tuberculosis. The exciting causes of tubercles in it are very obscure. Kiwisch imagined parturition to be one, but this seems doubtful, as many cases occur in women who have never borne children.

Symptomatology.—This is very incomplete and uncertain. The most constant symptoms are amenorrhea, leucorrhea, and pain, the first occurring most frequently. The speculum does not assist us in our diagnosis.—*Hospitals Meddelelser. Bd.* 5. *Hft.* 4.

[*Note by Translator.*—Uterine tuberculosis is certainly very rare, Louis having only found it in one case out of 350. According to this pathologist, the cervix may become cartilaginous in consistence from a deposition of tubercle; but Paulsen's remarks as to the immunity of the cervix are confirmatory of the observations of most modern writers. Below we have given a rare case, recorded by Virchow, of tuberculosis affecting the vagina.]

CREDE'S CASE OF STRANGULATION OF THE FOETUS BY THE UMBILICAL CORD COILED EIGHT TIMES ROUND THE NECK.

This occurred in the child of a primiparous and syphilitic woman, æt. 23, which was born in a state of decomposition. The child was 19 inches in length, and from its umbilicus, the cord, 57 inches long, proceeded over the right shoulder to the back of the neck, which it firmly encircled eight times. The eight loops were placed above one another in two rows—the four lower being coiled in a spiral manner, from below upwards, and the others being arranged in an inverse order. The end of the eighth loop extended over the left shoulder and abdomen to the placenta, into which the cord was centrally inserted.—*Zeitsch. für Gebürtsk I.* 1853.

VIRCHOW ON PROSTATIC CONCRETIONS IN THE HUMAN FEMALE.

It has been alleged by Leuckart (see his article on "Weber's Organ" in *Illust. Med. Ztg. I.* 2), that in women there exists a true rudimentary prostate, consisting principally of mucous follicles, and situated between the beginning of the urethra and the reflection of the vagina. Virchow also admits the existence of this body, and says that he has often found, at the neck of the bladder,—especially in old women, where the internal orifice is thickened,—round, greyish-yellow enlargements, in which there are gradually formed firm, dark-coloured bodies, lying embedded in the mucous membrane. These bodies he considers exactly identical with or analogous to the concretions found in the prostatic portion of the male urethra.—*Virchow's Archiv. V.* 3. 1853.

VIRCHOW ON TUBERCLES FOUND IN THE VAGINA.

In the abstract which we have elsewhere given in the *Periscope*, of Paulsen's remarks on "Uterine Tuberculosis," we have stated that the vagina is very rarely the seat of this disease. Virchow, however, has lately described a case of this rare pathological condition, the first he had ever seen, which occurred in an old woman who had suffered from dysuria, occasioned by tuberculosis of the urinary organs. Not only were her kidneys found thus diseased, but patches of tubercle were scattered over the upper part of the bladder, as well as over its neck and lower surface. The vagina was similarly affected, groups of tubercles being found here and there in the centre of reddened portions of its surface. These tuberculous patches were of a grey pearly appearance, and were most abundant at the external orifice. Ulcerations could nowhere be detected. When microscopically examined, these bodies were found to consist of numerous soft cells, having that very granular structure which is so characteristic of young tubercle.—*Virchow's Archives für Path. Anat. etc. V. Bd.* 3 *Hft.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.

May 3, 1854.—J. TAYLOR, M.D., Vice-President, in the Chair.

[PATHOLOGICAL MEETING.]

CARDIAC MURMURS WITHOUT VALVULAR DEFORMITY. BY W. T. GAIRDNER, M.D.—*Dr W. T. Gairdner* stated that the result of his inquiries on this subject were as follows:—1. It is impossible in the dead heart to imitate precisely the physiological conditions of the closure of these valves, so as to prevent reflux. The nearest approach to these conditions is when water is injected, with moderate force, into a ventricle in a state of tonic contraction. The slightest amount of relaxation in the muscular fibre of the dead ventricle determines regurgitation. 2. On the right side (as was observed by Hunter and Wilkinson King) regurgitation is more certainly and more freely produced in experiments on the dead heart than on the left. The chief reason of this difference, however, is the usual absence of rigid tonic contraction after death in the right ventricle, and the greater readiness with which its thin walls yield to distension. No argument can be drawn from this difference, as to the existence of a "safety-valve function" in the living and healthy heart. 3. The impossibility of securing in the dead heart the physiological conditions of closure in the living, together with other considerations derived from the structure of the valves, prove that a certain amount of tension must be exercised upon the tendinous cords, through the muscular structures of the ventricle, before the counter-pressure of the blood upon the valves can be exerted so as to close them against regurgitation. 4. Too great tension is fatal to the closure of the valves, as well as too little; and there is reason to think that the columnæ carneæ are so disposed as to maintain, by a very beautiful and delicate adaptation, a nearly uniform state of tension of the valves throughout the contraction of the ventricle. 5. Not only must a graduated tension be exercised upon the tendinous cords, but the columnæ carneæ must be approximated at their bases, so that the whole of the tendinous cords act, as it were, from one point in the centre of the ventricle, towards which all the edges of the valvular fringe are drawn. Thus the cylindrical curtain, which hangs from the auriculo-ventricular opening during the diastole of the heart, is converted (as Bouillaud has correctly pointed out) into a cone with the apex downwards; and the counter-pressure of the blood upon the under surface of this conical curtain brings its edges into apposition, and completely closes the valve against regurgitation. 6. To sum up the physiological conditions of closure of the auriculo-ventricular valves: it is indispensable, *a*. That they shall be acted on by the columnæ carneæ, and kept thereby in a certain state of tension; *b*. That the counter-pressure of the blood shall then come into play, and float the valves upwards towards the openings which they are destined to cover; *c*. That the forces *a* and *b* shall be simultaneously exerted on the valves when in a favourable position for complete closure: *i.e.*, with the columnæ carneæ and chordæ tendineæ massed together, and the free edge of the valve drawn towards the axis of the opening. 7. All these conditions may be shown to concur in the systole of the healthy heart. 8. The condition *a* is vitiated to a greater or less extent by disease or degeneration of the columnæ carneæ or chordæ tendineæ; also by great hypertrophy or dilatation, and more particularly by hypertrophy with predominating dilatation of the ventricles. In certain cases of simple hypertrophy, however, with inconsiderable dilatation, the increase in size affects all the structures *pari passu:* and in these it may happen that the valve preserves its

normal relations by the elongation of the tendinous cords and muscular columns. 9. The condition *c* is vitiated wherever dilatation predominates largely over hypertrophy; for then the contractions of the ventricles are inadequate to bring the columnæ carneæ together at any period of the systole; they remain therefore permanently apart, and the tendinous cords instead of drawing the edges of the valves *towards* the axis of the opening, may actually cause them to diverge from it. It is unnecessary to do more than point out that sacculated aneurisms of the ventricle, globular polypi between the columnæ carneæ, and other morbid conditions, may produce a like result.

Cancer of the Pericardium. By D. R. Haldane, M.D.—In the specimen exhibited the internal surface of the parietal layer was chiefly affected. Small cancerous masses were found on the left ventricle, and a larger one at the roots of the vessels. The posterior part of the lung was also affected, but the cancer of the pericardium was distinctly isolated, and no connection could be traced between it and the disease in the lung. Several other organs contained cancerous deposits, especially the bronchial glands and the liver.

Suffocation from a Piece of Meat. By D. R. Haldane, M.D.—In this case the death had been sudden. The friends reported that there had been no symptoms of choking, and that the patient had expectorated a little blood. A large piece of meat, with a mass of gristle in the centre, was found in the pharynx. It was ascertained that the man had been in liquor at the time, and had been observed pushing his finger down his throat; by which the slight hæmorrhage might be accounted for.

Professor Miller urged the importance of careful examination of the throat with the finger in all cases of sudden death, and adduced some cases in illustration; particularly one which had occurred to the late Mr Liston, of a poor woman, who, in a protracted voyage from Shetland, had suffered from starvation, and, on landing at Leith, had made to the nearest eating-house to satisfy her cravings, but in her precipitation nearly choked herself with a lump of beef.

Cancer of the Pancreas. By D. R. Haldane, M.D.—In this case there was intense jaundice. The gall-bladder was much distended, to be accounted for by the pressure of the enlarged pancreas, and the fact that the common duct was involved in the disease. Contrary to what has been generally observed, the cancer had not primarily attacked the head of the gland, which, for nearly the extent of a square inch, appeared to be perfectly healthy, and from this portion normal pancreatic fluid was obtained. The rest of the gland, however, was affected and remarkably indurated. There was no cancer of the pylorus or duodenum; but cancerous nodules, containing a large amount of mineral matter, and evidently retrograde in character, were found in the liver and on the mesentery. Careful microscopic examination had satisfactorily determined the cancerous nature of the pancreatic disease in this preparation.

Dr W. T. Gairdner said, that the patient had been for two months under his care in the hospital. The case he regarded as one of great interest in the present crisis of our knowledge of pancreatic disease. For seven months prior to death the patient complained of various dyspeptic symptoms. After three months' residence in the hospital he had been dismissed, after trial of various remedies, without much benefit; and he returned only about a fortnight before his death, affected with increased emaciation, intense jaundice, ascites and great tympanitis; but no anasarca. He gradually sunk into asthenic coma and died. The stools never contained any of that fluid fat to which attention was directed in these cases by Dr Bright. They were, on the contrary, dark and scybalous, and afterwards, during the jaundice, clay-coloured. The patient had not taken much fatty food, except a little milk, which on both occasions formed part of his diet.

Aneurism of the Popliteal Artery. By James Miller, Esq.—The preparation had been obtained in consequence of the supervention of gangrene,

necessitating amputation, after ligature of the superficial femoral artery. The aneurism was of the size of a large orange, and presented a distinctly bilobulated appearance, from the passage of the popliteal nerve across its surface. The vessels lay to the side, and the vein especially had been subjected to pressure, but its coats were healthy and canal entirely pervious. The patient was thirty-eight years of age, and an iron-moulder by trade. On his admission into the hospital, which took place very shortly after the discovery of the disease, he was in a very unfavourable state. His pulse was quick, tongue foul, and he had a general scaly syphilitic eruption. The œdema of the limb was great, and the tumour large and evidently rapidly enlarging. The agony he suffered was extreme. No time was to be lost, and the next day Professor Miller tied the superficial femoral; the operation was easily performed, and the femoral vein was never seen. In the evening the temperature of the foot was low, and the day after the dorsum of the foot presented the peculiar measly look so much dreaded as the precursor of gangrene, and the coldness of the limb increased. A non-stimulating practice was had recourse to, and the limb was encased in cotton, but the gangrene gradually advanced. An eligible line of demarcation formed below the knee, but from the unsatisfactory state of the tumour Mr Miller amputated low down in the thigh. The constitutional symptoms by this time had become serious; there was well marked hectic, and a bed-sore had formed over the hip of the affected side. The ligature on the femoral had come away at the usual time, and the wound was nearly cicatrised at the time of the amputation, nearly four weeks after his admission. The patient was progressing favourably, though he was evidently labouring under an intensely aneurismal diathesis, and a popliteal aneurism is forming in the other limb.

Professor Miller regarded the gangrene which had occurred in this case as purely the result of the sudden and complete interruption to the arterial circulation by the ligature. Pressure, however, under the peculiar circumstances, he believed would have been still more disastrous; and he was clearly of opinion, notwithstanding the unfortunate issue, that the case was one of a class in which the ligature was the preferable practice.

Professor Miller mentioned the particulars of another case of popliteal aneurism, admitted the same day as the preceding, in which pressure was being hopefully applied. Both of Carte's instruments had been employed, but were ultimately discarded on account of the uneasiness they gave rise to. What he (Professor Miller) had found most serviceable in the present case was a simple lump of lead, which was maintained in position by an elastic band, as had been used in the Dublin hospitals.

Dilatation of the Bile-duct and Gall-bladder. By W. T. Gairdner, M.D.—The case was interesting in a clinical point of view. The patient, aged 90, had suffered from ague, and latterly had become jaundiced, with no very urgent symptoms. About a fortnight before death, Dr Gairdner discovered a tumour in the epigastrium and right hypochondrium, inseparable from the margin of the liver and dull on percussion, and in its centre a rounded elastic nodule, which projected from the tumour. It was supposed to be encephaloid. Soon afterwards the man died much emaciated. Previous to the dissection it was observed that the dull percussion and tumour in the right hypochondrium had disappeared, or become so much modified as not to suggest anything decidedly abnormal. On dissection the solution of these facts became apparent. A very much enlarged gall-bladder was so matted to the lesser omentum and colon as to form a species of tumour. The gall-bladder was, however, found to be only one-third full, and the colon half full of air, so that the mass presented little resistance to pressure, and was highly tympanitic on percussion. The whole of the biliary ducts were much dilated, the common duct admitting one finger without difficulty down to the duodenal papilla; the orifice of the papilla admitted a director, and was superficially eroded over a space of four

or five lines in diameter. No decided obstruction of the ducts could be discovered, unless it might be considered to be the result of spasmodic contraction of the orifice dependant on the ulceration.

Cancer of the Cardia and Œsophagus. By W. T. Gairnder, M.D.—The cancer of the cardia and of the cardiac end of the stomach, and of the lower part of the œsophagus, was in an ulcerated condition. The pericardium was found thickened, and the pleura contained recent lymph, and was full of fluid. The case was not correctly appreciated during life. There was intense emaciation, but no dysphagia or vomiting. The patient had little appetite, and appeared to be phthisical. He suffered from cough, and had effusion into the pleura, which had been evacuated several times to the amount of one or two pints. The preparation showed that the ulcerated opening of the stomach was quite pervious, and thus the absence of dysphagia was explained. Diarrhœa, resembling that found in phthisis, had been a prominent symptom; but, though old tubercles were found in both lungs, and ulceration of the small and great intestines, the ulcers were rather of a dysenteric than of a physical character.

Aneurism of the Aorta. By W. T. Gairdner, M.D.—Two preparations illustrative of the disease were exhibited. In the first case, the aneurism, by passing on the pulmonary plexus and on the recurrent nerve, had given rise to a set of symptoms, to which Dr Gairdner had previously directed the attention of the Society. The diagnosis was correctly made some weeks before death, though there were no direct physical signs of the presence of an aneurism. The patient died by hæmorrhage.

In the second case, there had been intense dyspnœa, apparently due to laryngeal obstruction. On examination by the finger, however, no swelling or inequality of the glottis could be detected. As the patient seemed at the point of death, tracheotomy was performed. On auscultation, the respiratory murmur was found to be very feeble in the chest, particularly on the left side. The existence of a thoracic aneurism was here also diagnosed from the peculiar symptoms, though no unequivocal physical signs were present. The patient died in a few hours after the trachea had been opened. It was clearly ascertained that at no period had the expectoration ever been bloody. The aneurism on dissection was found to press on the left recurrent, and on the left pulmonary plexus. It presented a very large opening into the left bronchus, fully half an inch long and very ragged, and yet hæmorrhage had never occurred. The explanation was afforded by the presence of a large laminated clot, which effectually closed the entire sac. A small aneurism was situated on the anterior wall of the larger one, and was filled with a clot, and this, with the other and larger clot, so encroached on the aorta, that it was difficult to believe that the canal was open for the passage of blood at all. The left radial pulse had been noted to be a little less strong than the right. The pulse in the lower limbs had not been examined.

Tendinous Degeneration of the Heart and Ossification of the Coronary Arteries. By W. T. Gairdner, M.D.—The patient had perished suddenly, after complaining of the usual symptoms of bronchitis, but not in a very marked degree. The heart had been supposed to be dilated and hypertrophied, and the right ventricle was considered to be chiefly implicated, from the existence of the bronchitis, and from the comparatively loud second sound heard in the pulmonary artery. On dissection, however, the left side of the heart was found to be the affected one, and the cause of death to be an ossification of the coronary arteries, and a tendinous degeneration of the heart, and especially of the whole length of the columnæ carneæ. The discrepancy between the diagnosis and the appearances, on dissection, could only be explained on the principles laid down in the paper which he (Dr Gairdner) had read that evening, viz., that the second sound in the aorta had become suppressed in

consequence of the hypertrophy and dilatation leading to regurgitation through the mitral orifice.

ABSCESS CONNECTED WITH THE RIGHT SUPRA-RENAL CAPSULE OPENING INTO THE DUODENUM THROUGH THE BILIARY PASSAGES. BY W. T. GAIRDNER, M.D. —In this case, pain had been complained of in the right lumbar region, and an abscess had formed in connection with the back of the liver. The duodenum, which was filled with half digested food, contained a peculiar orange yellow mass. The abscess was not full; it was connected by an opening with the bile ducts, which were dilated down to the duodenum. Their walls were lined with a substance corresponding to that found in the duodenum. This substance had evidently formed a part of the wall of the abscess, and had been expelled by the ducts. The exact seat of the abscess was not so evident. It was quite isolated, and situated behind the liver; the hepatic tissue being evidently only secondarily affected. Not a trace of the right supra-renal capsule could be found, and Dr Gairdner was strongly inclined to suspect that the abscess had been originally situated in that organ.

THE LARVA OF THE ŒSTRUS BOVIS IN THE HUMAN SUBJECT. BY J. MATTHEWS DUNCAN, M.D.—A specimen was exhibited which Dr Duncan had extracted from the skin of a girl, aged twelve years, who had lately come from the country, where she had been employed in herding cattle. There was a small tumour, like a boil, with a small opening on its apex. There was, however, no redness or appearance of inflammation. On attentive examination, Dr Duncan saw something moving in the interior, and without difficulty extracted the larva alive; a little fluid containing blood and purulent matter escaping at the same time. The animal consisted of eleven segments, and presented all the characters of the larvæ of the œstrus bovis. On inquiry, Dr Duncan found that it was the third which the girl had observed on her person. It appeared that she distinctly remembered having been severely stung while engaged attending on the cattle in the summer. About the end of February, she felt a swelling on the spot of the sting, which moved about, ultimately becoming fixed. The second one appeared on the back of the neck; and, in the present instance, the worm was first perceived over the spine, at the dorsal vertebræ; it then progressed into the neck, disappeared, and was again felt on the right side of the neck, whence it was extracted. It was well known that the ova of œstrus bovis are deposited in the autumn on the backs of black cattle; they remain there during the whole winter; increase in size in early spring; form a nidus, and live on pus—the result of the irritation of their presence; they get very large, and fall out, and, after creeping a short distance, become torpid, and assume the chrysalis form, and in the course of the autumn, that of the full fly. (*Vide* the researches of Bracy, Clark, Reaumur, etc. etc.) No human bot has been described; indeed, its existence is highly problematical, as it would be easily discovered and described, and in a single season entirely extirpated. In Europe, none of the œstridæ infest man. In Surinam and the West Indies, a few cases are on record of their occurrence. Humboldt in his travels makes an indistinct allusion to a parasite of this kind, which infested the bellies of the Patagonians.

Professor Miller asked, what proof Dr Duncan had of the animals having travelled such a distance over the girl's body?

Dr Duncan could only appeal to the analogous fact observed in the case of the two first animals which had infested his patient, who was a sharp girl, and very distinct in her relation of what had previously occurred. The instinct of the animal failing in the present case was very remarkable, for, as a genus, the œstridæ are noted for their instinctive skill in reaching their habitat. For example, one species is known to infest the stomach and bowels of the horse, the eggs are deposited on the knee of the animal, and give rise to irritation. If they are not licked off, of course the eggs are lost; but they are generally washed down the horse's throat by means of the saliva, and reach their habitat,

where they remain till next spring. They are then passed with the evacuations and become perfect bots. The œstrus ovus, a species infesting the nostrils of the sheep, has been found, though rarely, in man.

June 7, 1854.—J. TAYLOR, M.D., Vice-President, in the Chair.

CASE OF FEMORAL ANEURISM. BY JAMES MILLER, ESQ.

The patient, a plumber by trade, was admitted into the Royal Infirmary, on the 27th of December, 1853, with a femoral aneurism of the left side. There was synchronous pulsation to the sight and touch, and no discoloration of the integuments, which were traversed, however, by a few large veins. The patient complained of weakness and numbness of the limb, which was the seat of occasional cramps. It appeared that a small swelling had been observed on Sept. 1st, which from the first presented distinct pulsation. No cause could be assigned for its appearance, except that the patient had been in the habit of lifting heavy weights. A month after its having first attracted his notice, on the occasion of his raising a very heavy load, the tumour assumed a reddened appearance, and afterwards continued to enlarge. The patient had contracted syphilis about a year before. A consultation was held, and the ordinary operation of placing a ligature on the external iliac artery, was performed on the 14th of Jan. 1854. The pulsation in the tumour ceased immediately on the application of the ligature. Four hours afterwards secondary hæmorrhage occurred from the *wound*, and from six to eight ounces of blood were lost. Chloroform was re-administered, and three vessels were tied.

The only points of interest in the operation were, first, that the incision was made higher up than ordinary, in consequence of the extension upwards of the tumour, the subsidence of which, however, since the operation had lowered the line of incision, which, as the cicatrix testified, occupied nearly its usual site; and, secondly, the external iliac artery was found remarkably bent upon itself, like the internal iliac, from the presence of the tumour, and to lie more deeply seated, thus rendering the operation more difficult.

A good night was passed, and it was noted next day, the 15th, that there was no pulsation in the tumour, and that the temperature of the limb was unchanged. The antiphlogistic regimen was enforced, and the patient was laid on his left side. Jan. 16th. Pain was complained of in the wound, but there was no pulsation in the tumour; the limb was warm. Jan. 18th, There was slight cough, which occasioned pain in the wound. Jan. 20th, The stitches were removed; the discharge was healthy. Jan. 22d, The ligatures on the small vessels of the wound came away. Feb. 4th, The ligature on the external iliac came away; it had been evidently loose for some days, probably from the 21st of Jan.; the tumour was diminishing in size. Feb. 20th, He was gaining strength; he complained of slight irritability of the bladder. March 2d, The catheter was passed. He got up from bed. The wound was almost closed; the tumour was harder and less. March 7th, A slight thrill was perceptible in the tumour. He had been taking too much exercise; was ordered to bed again. March 8th, The thrill was gone; a small quantity of pus escaped from an opening in the wound. After this, improvement continued, and he was dismissed on the 4th of May 1854, the wound still discharging slightly. At present he was an out-patient on account of his general health.

Mr Miller said that some apology might be due to the members of the Society for submitting to their notice this ordinary case of successful deligation. He had done so, however, for two reasons. 1st, That the simple historical fact of the result might be chronicled for the use of the statistician; and 2d, That attention might be directed to what appeared to him to be of chief interest in the case, viz., that contrary to ordinary experience in such circumstances there was no return of pulsation in the tumour after the vessel was ligatured until a long interval of nearly two months had expired, when, after too free motion of the limb, a thrill was felt in the tumour, which also underwent a slight increase of size, and again, on rest, both these symptoms disappeared. With the exception

of a slight comparative hardness on its circumference, probably due to the presence of the cyst, the tumour itself never became hard, but remained soft and fluctuating, reminding Mr Miller very strongly of what is observed in a severe bruise from the presence of coagulated blood. The only explanation which he (Mr Miller) could give of the absence of the pulsation and the soft condition of the tumour was that the vessel communicated with the aneurism by a small opening which was closed by means of a small clot, such as was described by Dr W. T. Gairdner at the last meeting of the Society as having been present in a case of thoracic aneurism, opening into the bronchus. [See Monthly Journal, p. 79.] The contents of the aneurism at the time of the operation he believed to have been mainly fluid, and to have been prevented communicating with the arterial current by the small clot which was impacted like a cork into the aperture. The silence and fluidity of the tumour were thus accounted for, and absorption went on as in the case of an ordinary severe bruise, the cyst contracting as the contents disappeared.

Dr Andrew Wood thought that Mr Miller had satisfactorily accounted for the fluidity of the tumour and the long-continued absence of the pulsation in his interesting case; he (Dr Wood) would like to hear, however, how Mr Miller explained the anomalous recurrence of the thrill at the end of the second month.

Mr Miller stated that to his mind the explanation was sufficiently simple. He thought it extremely probable that the small clot in the aperture of communication having, along with the tumour itself, undergone a degree of absorption and become diminished in size, had temporarily been displaced in consequence of the patient taking more severe exercise than he had been accustomed to during his convalescence. On rest and quiet being enforced, however, the plug resumed its position, and the tumour its former conditions.

CASE OF SPONTANEOUS GANGRENE IN A CHILD EIGHT MONTHS OLD. BY JAMES SIDEY, ESQ.

The gangrene had appeared on the head, face, and hands, the appearance of which was well illustrated by a series of casts. The right ear and the entire hairy scalp were of an intensely black colour, and on both cheeks patches existed about the size of a half crown piece. The right thumb and the backs of both hands were similarly affected. The child was noted to have been restless and feverish on May 22d; and, on the 23d, a slightly darkened ring was found to have formed round the thumb, about the middle of the first phalanx; in a few hours, the whole thumb was gangrenous, and the dorsum of the hand became involved. On the ear, the gangrene commenced with the appearance of a flea-bite, and subsequently extended rapidly to the scalp, assuming a remarkably regular form, and giving the child the appearance of wearing a black scull-cap. The pulse was observed to be very feeble, and the mouth to be unaffected. Death took place in twelve hours from the first appearance of the gangrene on the thumb, the child being sensible, and continuing to suck well, up to a few minutes before death. Its previous health had been tolerably good. The only medicine it had taken prior to the supervention of the gangrene was a little ipecachuan wine and carbonate of soda, for an attack of hooping cough, from which it was convalescent.

Dr Andrew Wood said that the case was a remarkably interesting one, and forcibly reminded him of cases of blood poisoning, as *e.g.* from ergot of rye, which were attended with gangrene. It was very evident that in the present case the peculiar appearances were not due to any affection of the blood-vessels, but rather, as he had stated, to a poisoning of the blood itself. The symmetrical disposition of the gangrene, the suddenness of its approach, and the rapidity of the fatal termination, were especially noticeable.

Dr Warburton Begbie had once met with a case of spontaneous gangrene of the scalp, which was a very rare lesion. The patient in whom he had observed it was labouring under typhus.

Dr W. T. Gairdner mentioned the particulars of a case where the tip of the nose, after assuming a bloodless and discoloured appearance, became gangrenous.

The patient was a young lady who, on dissection, was found to have laboured under a tubercular disease of the kidney. Part of the ureter and bladder was also affected.

Dr Matthews Duncan had observed a somewhat similar case to that related by Dr Gairdner, in the Infirmary at Aberdeen. The patient was a woman, and had disease of the heart and kidneys. Several weeks before death, the tip of the nose became gangrenous, and a line of separation was formed. A single toe or finger was also gangrenous.

ANEURISM OF THE POPLITEAL ARTERY. BY JAMES MILLER, ESQ.

Mr Miller again drew attention to the case of popliteal aneurism, read at the last meeting, (p. 77), and re-exhibited the preparation, a section having been made of the aneurismal tumour. Mr Miller had formerly suspected the contents to be purulent; but it was now clearly ascertained that the tumour contained no pus, but was filled with coagulated blood—the central portion of which was slightly fluid. The opinion he had formerly expressed, that the gangrene had been the direct result of the ligature, and of the ligature alone, was confirmed, and was accounted for by the sudden interruption to the flow of blood. 1. No reaction took place in the limb after the operation, and the gangrene was immediate. The peculiar chronic form of the gangrene was to be remarked. 2. No phlebitis could be detected; and 3. The ligature came away on the fifteenth day, and no secondary hæmorrhage occurred. On the 23d April, amputation was had recourse to; and on the 27th, it was noted in the case-book of the ward that the sore on the stump was weak in character, and that another popliteal aneurism had begun to form in the other (left) leg. May 18th, The stump was healing; one of the bed sores was nearly well. Stimulants and generous diet were ordered. May 20th, The left limb was beginning to swell; the tumour had rapidly enlarged, and the same excruciating pain was complained of as in the other aneurism. A consultation was held, and ligature of the superficial femoral was determined on; the operation was performed the same day. May 27th, There was no mottling of any part of the limb. A little lividity had been observed on the dorsum of the foot four or five days after the operation, but that had passed away, and everything appeared to be progressing favourably, when to-day diarrhœa has set in with symptoms of collapse. He never rallied, but died the same night.

On *post mortem* examination (which owing to circumstances was rather hurried), a few decolorized clots were found in the aorta, which, with the exception of a few small spots of atheroma in its descending portion, was healthy, as were also both iliacs. The heart showed no appearance of disease. A preparation of the deligated vessel was exhibited: it presented the usual appearances. A beautiful dissection of the tumour itself was also shown. The aneurism was about the size of a large plum, and, as in its fellow of the opposite side, had the popliteal nerve spread over it. A remarkable fusiform enlargement of the aneurism extended upwards in the femoral direction. Mr Miller said that it was difficult to account for the existence of the strongly marked aneurismal diathesis in this case, and the absence of vascular degeneration (some slight appearance of it in the aorta excepted), and the fact of both ligatures having held so well, and the non-occurrence of hæmorrhage. The man when he died was covered with a tertiary syphilitic eruption, and for years prior to death had for five or six hours every day worked upon his knees.

After a few remarks from *Dr Andrew Wood*, the Vice-president declared the session closed.

THE CHOLERA IN DUMFRIES.

(*To the Editor of the Monthly Journal of Medical Science.*)

Sir,—I am surprised to find by Dr Grieve's letter to you of the 13th ult., that he "can now fearlessly say that none of his assertions were unfortunate

mistakes, and that in none of them was he altogether wrong," although he positively asserted in your February Number, when he ventured to contradict what had been stated by Dr W. T. Gairdner, at a meeting of the Edinburgh Medico-Chirurgical Society, regarding the outbreak of cholera here in 1832, "*That the next well authenticated cases did not occur*, as would naturally be supposed, if contagious influences were in operation, *in the same house or tenement, or in the same district or locality*, or amongst the attendants and relatives, *but in different parts of the town, with which there had been no personal intercourse or communication whatsoever.*" And after reading my statement of facts, extracted from the minutes of our Local Board of Health, which appeared in your March Number, and examining the official list of cholera patients, so carefully kept by Mr Gemmill, admitted in your April Number, that "*the first five or six cases occurred,*" as I had stated, "*in English Street,*" or, in other words, *in the same district or locality.*

As I have had no wish, however, on this occasion, to assume anything like an antagonistic position, but, on the contrary, merely to maintain what I know to be the truth in the gentlest and least offensive way; and as Dr Grieve's last letter is little more than a repetition of what was fully answered in the letter which you did me the favour to publish in the May Number of your interesting Journal, I shall not trouble you and your intelligent readers, with any farther observations on the origin of cholera here in 1832, nor on the arrangements which were so well organised by the medical practitioners of this place—including Dr Grieve—on the reappearance of the pestilence in November 1848, and so carefully *superintended* by them—and the cases so regularly reported—from the 24th until the 30th of that month, when all was unfortunately cancelled by the Parochial Board, and put upon the shelf till the arrival of Dr Sutherland.—I am, etc.,

ARCHD. BLACKLOCK.

Dumfries, June 10, 1854.

THE UNIVERSITY AND TOWN COUNCIL OF EDINBURGH.

SINCE the publication of the last number of this Journal, an event has occurred of great importance to academic education in general, and in which the medical profession are especially concerned. By a decision of the House of Lords, confirming a judgment of the Court of Session in Scotland, the Town Council of the city of Edinburgh has been found to possess the sole, absolute, and uncontrolled power of regulating the education necessary for the degree of Doctor of Medicine in the university. And the terms and principles of the judgment of the peers, as well as of the Scotch court, are such as to show that, in every other respect also, the Town Council possesses over the university the same unlimited jurisdiction.

Fortunately, indeed, for theological and legal education, an indirect control is exercised in these branches by the Church of Scotland, the Faculty of Advocates, and the Writers to the Signet, who fix their several standards of education, irrespective of the universities; so that the university authorities, whoever they may be, must exercise their high privilege with wariness, and only in conjunction with competent professional bodies. But medicine—in this as in all its relations, neglected and degraded by the State in Britain alone of all European nations—is henceforth, in the most frequented medical school of the country, to lie at the mercy of a provost, four bailies, and eight-and-twenty town-councillors.

This may be right, or it may be wrong. But twenty-five years ago, a Royal Commission, composed of the most eminent of the nobility, judges and lawyers of Scotland, came to the resolution, after a laborious and impartial inquiry, that the government of the University ought to be placed in far other hands. And about eight years later, a Parliamentary Committee, consisting of the leading members of the Scotch Bar, and exclusively of what is called the Liberal

party in politics—not very likely, therefore, to decide against popular power, or burgh prejudices, without ample cause—came to the resolution, that the Town-Council ought to be deprived both of the government and the patronage of the University. The former Commission was appointed in consequence of the Senate of the University, the governing body in all other universities in the nation, having been involved in an expensive law-suit, and threatened with annihilation by the Town Council. Since the report of the Committee, the Senate has been involved in another action-at-law just concluded; and the result is that the annihilation of its powers is complete.

The interests of learning and science, so far as they depend on education in this University, are thus made to depend on a body of men, who, however well chosen as to political activity, or skill in city affairs, have no affinity with either literature or science, or with the professions dependent on them. It is for the public and the State to say whether the present "anomaly" is to be permitted to continue. In a subsequent number, we shall endeavour to give a correct narrative of the events which have led to the present state of things. In the meantime, we reprint two documents to which we shall have frequent occasion to allude: 1st, The judgment given by the House of Peers, which it seems to us must appear very remarkable to the intelligent reader, if it be remembered, that in the act constituting the University, King James granted "to the Burgh of Edinburgh, patron of the said College, AND to the *said College, and to the rectors, regents, bursars, and students in the same,* all liberties, freedoms, immunities, and privileges appertaining to a free college; and that in as ample form, and large manner AS ANY COLLEGE HAS OR BROOKS WITHIN THIS HIS MAJESTY'S REALM." 2d, We give some extracts from the able opinion of the late Lord Fullerton, one of the judges in the previous action, from which it will appear, that in the special question of regulating the curriculum for medical degrees, the members of the Senate have not proceeded unadvisedly, or without encouragement, in maintaining what they conceive to be the privileges of this, as one of the free colleges of the realm.

THE SENATUS ACADEMICUS OF THE UNIVERSITY OF EDINBURGH, *Appellants; v.* THE LORD PROVOST AND MAGISTRATES OF EDINBURGH, *Respondents.*

JUDGMENT OF THE HOUSE OF LORDS.

THE LORD CHANCELLOR said this was an action of declarator brought by the University of Edinburgh against the Town Council of Edinburgh, and the object was to have it found and declared, in the first place, generally, that the Town Council had no right to interfere with the pursuers in the exercise of their right to lay down regulations as to the qualifications of students for obtaining degrees; and, secondly, in particular, that the Town Council had no right to insist on the pursuers adopting a statute they had passed, whereby the attendance of students at certain extra-academical lectures was to be counted to a certain extent as equivalent to an attendance within the walls of the University for the purpose of qualifying for the degree of M.D. The question had arisen in the following way: It appeared that in 1833 the medical faculty had obtained a relaxation of the rules which previously existed with reference to the degree of M.D.; for, while purely university teaching had been formerly indispensable, it was then settled that hospital practice was to be deemed to a limited extent an equivalent to it. So the matter rested till 1845, when it was proposed by the medical faculty to go still further, and allow some of the classes constituting the probationary curriculum to be attended in the hospital schools of London and Dublin. On this proposed arrangement being intimated to the Town Council they approved of the alteration, but they insisted on an amendment, which was to the effect that some of the classes should be allowed to be taken, not only in London and Dublin, but in the extra-academical schools of medicine in Edinburgh. The Senatus Academicus objected to this amendment, and ultimately, after a long dispute, the Senatus raised the present action, in which they sought a declarator to the effect

already stated. The Town Council relied on their title, which consisted of a charter of James VI. in 1582, and a subsequent statute passed in 1621, and which, as they alleged, fully secured to them the absolute right and title to regulate all matters affecting the University and the course of education, and they relied on an action which had been decided in 1829, in which this right in the most unqualified terms had been recognized by the Court of Session, and thus that the matter was *res judicata;* but even that, independently of that decision, the terms of their statute and charter were sufficient to establish their right. The First Division of the Court of Session had come to the conclusion that the Town Council possessed the right which they claimed, though one learned judge (Lord Fullerton) differed to the extent of holding that, on the particular instance complained of, the Town Council had exceeded their powers. The case had been elaborately argued at their lordships' bar, and it now rested with them to decide which party was in the right. There were two questions, as he had already stated—the general point, whether the Town Council had a right to prescribe regulations as to the course of studies and other matters affecting education in the University; and the particular point, whether if the Town Council had not the general right, they still had a right to interfere in the present instance. As to the first point, it seemed to him to be entirely concluded by what took place in 1829. At that period the Professorship of Midwifery, held by Dr James Hamilton, which was not then included in the curriculum for the graduation of M.D., was proposed to be added to the medical faculty by the Town Council; and the other professors having resisted this step, the Town Council raised an action for the purpose of declaring their right to insist on the admittance into the faculty of the professor in question. In that action, the Second Division of the Court of Session had unanimously found that "the pursuers (the Town Council) had the right of making regulations or statutes for the college, and that in respect of the studies to be pursued in the college, and course of study for obtaining degrees, as well as in other respects; also that the professors had no right to make regulations, statutes, or laws for the college in contradiction to the Town Council, or which might not be rescinded or altered by the Town Council." He confessed that that decision was in his view conclusive on the second point of the present case also, and that it fully established the right of the Town Council to make the statute which they had done. But even if he had not thought the whole question here had been settled by the action of 1829, still he would have come to the same conclusion on independent grounds. The language of the statute of 1621 was somewhat obscure; still it certainly did not give any independent existence to the College. The object of that statute was plainly to give further benefits to the town of Edinburgh, and, in consideration of its former services, certain privileges were thereby conferred on the Town Council, and the effect of that statute clearly was to make the College an appendix of the Town Council. This view of the case was strongly confirmed by attending to the course of conduct of the Town Council. Soon after the charter first granted to them in 1582, they had lost no time in exercising absolute authority, for it appeared they had in 1583 set down and devised the order of teaching, with the discipline to be kept in "the College now erected." So, in 1604, they appointed one Mr Munro regent, and gave special directions as to what he should teach. And so the same course of minute supervision was to be traced down to recent times, which showed conclusively that they had been in the habit of prescribing the regulations. It was said on the part of the appellants, that the power to regulate the course of study requisite for granting a degree was inherent in the very notion of a university. Now, he did not think that was the case. At all events, this was not a question of an abstract nature at all; but it turned exclusively on the special terms of the charter and statute erecting the University. And he had already stated that he considered the University to be a mere appendix of the Town Council, and entirely subject to the authority of that body.

Lord BROUGHAM said he quite agreed in the opinion which had been formed by his noble and learned friend. They had come to the conclusion, after a full

and able argument, and he regretted that there was no other course for them than to affirm the interlocutor of the Court of Session. In doing so, they had the satisfaction of going along with a great number of the judges below, eight of whom entertained the same views, though one of the judges, and one whose opinion was always entitled to the highest respect—viz., Lord Fullerton, did not quite agree. The opinions of the law officers of the Crown, and of the learned commissioners who inquired into the subject in 1839, were also the same; who all came to the conclusion that the University of Edinburgh stood on a different footing from the other Universities in Scotland. Nevertheless, after looking into the subject, and well weighing all the arguments they had heard, if their lordships had found that the conclusions of the judges below were erroneous, the House would have hesitated to set those judges right, as it had done on former occasions. However, after examining all the arguments and the evidence in the present case, he had arrived at the conclusion that the House was not bound by the *res judicata* of 1829; still the result would have been the same, independently of that decision. He had forgot to mention in enumerating the authorities in the law of Scotland in favour of the view he took, another authority and one of the greatest weight—he meant Professor Hume; who himself a Professor, and therefore having a natural bias the other way, had yet, on examining the question, come also to the clear conclusion that the College was entirely dependent on the Town Council, and that the latter body had the sole power to make laws. With regard to the title of the Town Council, he thought that the charter of 1582 was not a foundation charter at all, but that it merely gave a power to the Town Council to found a College or not, as they might think fit; and it so happened they did not act on that authority, but merely established a seminary for teaching, and did not incorporate another body, investing it with separate and distinct privileges. Then came the statute of 1621, which it was said, at least, gave this separate constitution to the College. But he thought that was a most extravagant construction to put upon it. The statute of 1621 was passed at a time when the Town Council had been already endowed by the charter with the most absolute and extensive powers of appointing professors and making regulations, and the statute proceeded on the express ground of princely favour, and for the further encouragement of the Town Council, and a consideration of their valuable services to the Crown and to the College. Can it be said, therefore, that that statute took away the supreme power from the Town Council, and made the College entirely independent if not superior to them? That would be a most absurd construction of the act, and one which was not warranted when compared with the charter, and with what had occurred since it had been granted. But if there had been any doubt on the subject, and if it had been capable of argument at all, that the act had given this power to the learned corporation at the expense of the municipal corporation, how could this doubt be better decided than by resorting to the course of usage that had occurred under the charter and the act? It might, no doubt, be very absurd for the Senatus to be entirely subjected in this manner to the control of the Town Council; it might be very inconvenient, though he believed no actual inconvenience had ever occurred; still it was a possible thing, and it was not a question of what was best in the abstract, but what was the law and the construction of the charter and statute; and these were best explained by what had been the practice of the Town Council subsequently. Now, no time had been lost by that body after the year 1582, in showing the rights which they claimed. In 1626, it appeared they framed a set of rules and regulations for the government of the College in the most minute details; there was provision for disputations being held, there were severe denunciations against acts of immorality, positive regulations for the students attending at certain sacramental ordinances of the church, and even a provision as to the manner in which degrees were to be conferred, and as to proper respect being shown to the Town Council in the terms which were to be used in the diploma. Then from time to time professors received their appointments on the express condition of their observing the rules laid down, and being subject to the authority

of the Town Council. Thus, even the very greatest man—the highest name in modern chemistry, and one which rendered the University so illustrious, even Dr Black took his appointment on such a condition. In 1780, the Town Council framed a general statute regulating the system of teaching; and notice of that statute was served, through that great man, Dr Robertson, the then Principal, upon the College. Attempts had, no doubt, been made at different times by the University to resist the Town Council, and a remarkable instance occurred in 1703, in reference to laureations; but that attempt to interfere with the legislative power of the Town Council was successfully repelled. It did happen, that forty or fifty years afterwards, steps had been taken by the University to do something which seemed an encroachment on the province of the Town Council, on which occasion the Council yielded to the University, but those were matters which were peculiarly within the cognisance of the professors, and they were contemporaneous with other more important acts in which they, in their turn, yielded to the Town Council, and which clearly showed that the jurisdiction of the Town Council was supreme. Thus, for instance, about 1763, Principal Robertson, that great man, applied to them for power to enable the Professor of Moral Philosophy to take fees instead of being paid by salary. Now, that had been done with full consideration, and no doubt because the Senatus had no power to alter the mode of payment in that way without the aid of the patrons. There was only one point more which he had to say. It had been attempted to press into the service of the appellants a dictum which had fallen from Lord Jeffrey in the case of Macdouall *v.* the Magistrates of Edinburgh, and from which it was attempted to be inferred that that learned judge had held that the moment professors received their appointments they thereby of necessity and incidentally acquired the sole and exclusive power of conferring degrees. There were some of the other judges who, no doubt, seemed to entertain that opinion also. He greatly doubted that point, especially as in the case of St Andrews, and other universities, that power was given by the express terms of their charter. It was, however, not necessary to entertain that question here, for the sole question now was, whether the professors had the exclusive right of regulating the course of study which was to qualify for obtaining a degree. Much had been said as to the difference which existed between the interference of the Town Council in regulating the course of extramural studies and in regulating intramural studies. But he thought that it was only an instance all the stronger of an exercise of power in their attempting to regulate the course of the intramural studies; so that that objection really went in favour of the respondents. In conclusion, he had only to repeat, that he was of opinion, for the reasons already stated, that the College of Edinburgh stood in a position quite different from the other colleges of Scotland, and that it was entirely subject to the control of the Town Council, who had the sole power of regulating the course of studies to be pursued within its walls. The interlocutor must, therefore, be affirmed with costs.

Affirmed with costs accordingly.

EXTRACTS FROM THE OPINION OF THE LATE LORD FULLERTON, AS TO WHETHER THE TOWN-COUNCIL OF THE CITY OF EDINBURGH, AS PATRONS OF THE UNIVERSITY, CAN LEGALLY INSIST ON EXTRA-ACADEMICAL LECTURES QUALIFYING FOR THE DEGREE OF M.D., WITHOUT THE CONSENT OF THE SENATUS ACADEMICUS.

"BOTH parties agree as to the particular course of study to be pursued within the College, as the condition precedent of any student being admitted to examination for a degree in medicine. That is the subject of the two leading sections (I. II.) of the proposed statutes of the University. But these are followed by section III., which enacts, 'that attendance on the lectures of teachers of medicine in the hospital schools of London, or school of the College of Surgeons in Dublin, or of teachers of medicine in Edinburgh, recognised as such by the Royal Colleges of Physicians and Surgeons of Edinburgh (in accordance with regulations to be adopted by these colleges jointly, and approved of by

the patrons of the University), shall, to the extent of one-third of the whole departments required by section II., clause 1, to be studied by candidates, be held equivalent to attendance under professors in this or in some other university, as already defined.' And the only part of this section to which, as I understand, the pursuers object, is that which substitutes, to a certain extent, the attendance on 'lectures by teachers of medicine in Edinburgh,' for the attendance on lectures by the medical professors of the College; so that the question comes to be, whether or not it is imperative on the pursuers, the *Senatus Academicus*, to admit to examination for a degree students who have not followed the prescribed course of education within the College, but who have to a certain extent pursued the course under non-academical teachers.

"The question is one of very considerable importance. I do not think it was decided in the former action; and in determining it now I am not much moved by the consideration which was powerfully pressed upon us in argument upon the part of the defenders, that the pursuers, who have admitted extra-academical teaching, viz., that at different universities, and even the teaching of the hospital shcools of London and Dublin, necessarily admit the legality of that to which they now object, in regard to the private teachers of medicine in Edinburgh. I do not see the relevancy of the observation. There is here no question of legality or illegality. It is entirely one of the conflict of powers. I see nothing absolutely illegal in what is now proposed by the defenders, if both parties agree. The only question is, whether the third section is, in all its points, imperative on the pursuers, without their consent, or rather against their declared objection. They, the pursuers, are willing to allow the teaching at the London and Dublin schools of medicine, as part of a course of study qualifying for examination for a degree in medicine at the University of Edinburgh; but it is only because they are so willing, that they give effect to it; and they deny effect to that part of the third section now under consideration, precisely because it has not received their consent. The only question then is, whether their consent be indispensable or not?

"Upon this point, the defenders, the patrons, certainly take high ground. It was maintained in argument, that the power of granting degrees flowed entirely from the defenders, the founders of the University—and from this it was inferred, that there was no absolute necessity for any previous course of study, and that it was within the powers of the patrons 'to make degrees entirely dependent on examinations.'

"I am certainly not prepared to admit any of these propositions.

"The foundation of the College no doubt flowed from the town of Edinburgh, in virtue of the powers conferred on them by the Crown. But when they did exercise that power, and did found the College, the right of granting degrees, *i. e.* of conferring on persons proficient in certain departments of knowledge, the recognised stamp of such proficiency—did not 'flow from the founders,' if by that is meant that it previously existed in the persons of the founders—but was an inherent quality of the institution, according to the public law of this and every other European country possessing similar institutions. Accordingly, the Charter of Ratification 1621 bears to be granted, not only in favour of the burgh of Edinburgh, but in favour of the 'said College, and of the rectors, *regentis*, *bursaris*, and students within the same,' on whom it confers 'all liberties, freedoms, and immunities and privileges, appertaining to ane free College, and that in as ample form and large manner as any College has or bruikes within his Majesty's realm.'

"I should have great doubts indeed, then, whether the founders, the patrons, were, on this ground, all-powerful in the granting of degrees; and as the greater number of such institutions, in which education is conducted either by lectures or private instruction, have required some evidence of attendance on those intra-academical opportunities of instruction as a preliminary to being taken on examination for degrees, I should require a good deal more argument

to satisfy me, that the defenders, the patrons, might dispense with all previous attendance at lectures, and require, as a matter of right, the College to take on examination for a degree, any individual whom they might choose to name.

"Most certainly that was not determined in the former action, which related entirely to a previous course of study; and it would be difficult to infer from the power recognised by the former judgment, to 'regulate the course of study for obtaining degrees,' a power of enabling a party to take a degree without any previous course of study whatever.

"But it is really unnecessary to go into this; because most certainly no such pretension is advanced by the defenders in this case. They admit that a previous course of study is indispensable for each candidate for graduation, and, in the discharge of their duty as patrons, they have united with the pursuers in determining what that course of study shall be. But, holding such course of study to be requisite, they propose to enact in the disputed part of the third section of the intended statute, that to the extent of one-third of the whole medical course required within the College, attendance on non-academical teachers of medicine in Edinburgh shall be received as equivalent.

"This evidently involves the proposition, that they, as patrons and founders of a College having the power of conferring degrees, and having, in the discharge of their duty in that character, fixed a course of study as requisite for graduation, can of their own authority place a part of that course followed without the College, on the same footing as if it had been intra-academical, and oblige the pursuers, the Senatus, to take parties on examination who had not completed the requisite course of academical instruction.

"And in principle, there is evidently no distinction between a partial and a total substitution of non-academical for academical teaching. If the defenders have the power to dispense with the attendance at College, to the extent of one-third of the departments specified in the proposed statute, they must have the same power as to the whole. So that the question comes to be, whether the patrons can compel the Senatus Academicus to take on trial for graduation, students who have followed, without the College, that very course of study which the patrons held indispensable within it.

"This is a question certainly of very considerable importance to the dignity as well as the interests of the pursuers; and I am not surprised that they resist this pretension of the patrons.

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"The ground mainly relied on by the defenders is, that they are the founders and patrons of the College—from which it seems to be inferred, that every power vested in the College, including that of conferring degrees, flows from the defenders, and therefore must be exercised at their discretion. I cannot adopt this view. It is true the Town-Council founded the College; but that institution, when created by the foundation, possessed, by the force of public law, powers, and, among others, that of conferring degrees, which never did or could exist in the founders themselves. The foundation of the College was the act of the founders; the emergence of the power of conferring degrees was the act of the public law, attaching it to the particular corporation or association which the act of the founders had called into existence. To infer from poe foundation of the College by the Town-Council, that the founders were allbewerful in the matter of graduation, is only a little less illogical than it would de to infer from the power of the founders to institute a College conferring thgrees, that, before the act of foundation, the Town-Council had themselves the power of conferring that distinction.

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"But it appears to me, that the power of regulating the course of study, on which the interference of the defenders in this matter depends, must necessarily be construed as meaning only the course of study within the College. In regard to all studies beyond that, the defenders have no power whatever. Those studies are as entirely foreign to their character of founders of the Col-

lege of Edinburgh, as the studies at the Universities of St Andrews or Glasgow, or any other institution of the kind. Indeed, the only power which, by their charter, they do seem to possess in regard to non-academical teachers in Edinburgh, is not that of regulating studies under such teachers, but of putting down such teaching altogether. For I observe, that in the charter of 1582, quoted in their fourth statement in the record, the magistrates are not only to have the power of appointing professors within the College, but the power of inhibiting all others 'ne dictas scientias intra dicti nostri burgi libertatem profiteantur aut doceant nisi per prefatos præpositum ballivos et consules eorumque successores admissi fuerint.'

"Whether the defenders are termed patrons, or founders, or visitors, their patronage, or foundation, or visitation, has no conceivable reference to any teachers but academical teachers, or any course of teaching but that which is pursued within the College. As the interference in the granting of degrees seems to me to depend on the power of regulating the course of study, and as that is confined to the course of study within the College, it would follow that the interference with the College in the conferring of degrees must be equally limited, and must be commensurate with the course of study afforded within the College.

"I think this view receives material support from some of the proceedings brought forward by the defenders themselves. Then it would appear, by that statement, it was only in 1726 that a proper faculty of medicine was instituted, and the means of a properly academical education afforded within the College in that department.

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"Thus, while no proper medical course of study was afforded by the patrons within the University, the conferring of medical degrees was the act of the Senatus alone, they choosing the examinators, being certain members of the Royal College of Physicians. And it was only after the institution of the medical faculty that the *intra*-academical examinations, and the establishment of a curriculum, or course of study appointed by the patrons took place.

"This is all very intelligible. So long as there were no adequate means of instruction in medicine furnished within the College, the Senatus Academicus were left at liberty to take their own mode of satisfying themselves of the qualifications of the applicants for graduation. In so doing they did not interfere with any interests in the College which the founders and patrons were called upon to protect.

"But after the faculty of medicine was established and endowed, the patrons, as the protectors of the new interests thus established, may have been entitled to regulate the course of study which they considered as indispensable to graduation; and it is quite possible that the patrons may, consistently with this principle, have the power to interfere with the communication of the benefit of a degree to any but those who have received the proper training within the College.

"But there is no such question raised here. On the contrary, the question is, whether, after a medical faculty has been established and endowed, and after the due course of study in that department has been determined on by the patrons, they, the patrons, can of their own authority require the College to take on examination applicants for graduation who have followed part, or, it may be, the whole of that course of study, under teachers in Edinburgh who are not professors in the College.

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"The College, as composed of a body of individuals, consists of the students and professors—of those who are taught, and those who teach. Now, according to the constitution of the College and the terms of the grant to the founders, the power now claimed, of conferring on medical teachers not belonging to the College, the privilege of giving instruction qualifying for a degree, cannot be necessary for the good of the students. By the very terms of the charter, the

patrons are to choose as professors of the different sciences taught within the College, 'personas ad dictas professiones edocendas maxime idoneas.'

"The best instruction to be had is to be furnished within the College itself; so that no extraneous teaching can be required for the students.

"But can it be said with any show of reason, that the exercise of such a power is beneficial to the professors? Is it not, on the contrary, undeniable that the admission of the other teachers in the city to an equality with them, in the matter of studies qualifying for graduation, is not only derogatory to their pre-eminence, but highly injurious to their pecuniary interests? Or, even taking a more general view of the matter, and supposing the welfare and dignity of the College is to be measured by the resort of students to the means of education supplied by it, it is manifest that the tendency of the regulation in dispute, is most materially to diminish the amount of that resort.

"And in this particular the contemplated regulation operates very differently from those modifications of the privileges of the professors, to which they have given their consent. In the case of students who have followed a partial course of study at the hospitals of London, or at the school of the College of surgeons in Dublin, the admission of such students, to qualify for a degree at the College of Edinburgh, by completing the course of study there, may possibly, and will probably, have the effect of attracting to the College a class of students who would otherwise have no inducement to enter.

"But when, in terms of the proposed regulations, the attendance on teachers in Edinburgh, the very seat of the College, is to have the same advantage, to any extent, as the intra-academical instruction, the effect necessarily is, to withdraw from the College a set of students actually on the spot, and who, but for the regulation, would certainly have attended the academical course, if they wished to graduate. This effect can be viewed, as regards the College, only in one light, viz., as a sacrifice of the rights of the professors, for the benefit of the extra-academical teachers. Whether this may or may not tend to the general improvement of the medical education afforded within the city of Edinburgh, is a question into which we cannot enter.

"It is quite possible that the learned persons approved of as private teachers of medicine by the magistrates, may be as competent to the instruction of students as the gentlemen on whom they have conferred professorships. It is also possible that, by a more liberal constitution, a college having the power of granting degrees might be called on to take, on examination, applicants, whatever may have been the locality of their previous studies: And I believe there are institutions in which that forms a condition of their establishment.

"But most certainly it is no part of the constitution of the College of Edinburgh. That College is created in the terms of the charter, with all the powers and privileges enjoyed by any other free College: And one of these certainly is, in the general case, to limit the examination for degrees to parties who have qualified themselves by attendance on the academical course: And the only question before us regards the power of the founders and patrons to dispense with that condition.

"I can see no authority for their claim to such a power. It is not to be found in the letter of the constitution of the College; and, according to the spirit of that and every similar establishment, so far from being implied in the office of patrons, it seems directly adverse to the duties of such an office. It would be a strange act of patronage indeed to rear up a body in direct rivalry with those of whom they are the patrons and protectors. Yet that is what is attempted here. The extra-academical teachers of medicine are not only to be recognised by the Colleges of Physicians and Surgeons, 'but approved of by the patrons of the University.' To this as a general regulation securing the respectability of teachers, the College has no right to make any objection. But when it is proposed to put that teaching on a par with the proper academical teaching, as preliminary to graduation, the interference with the rights of the College is most flagrant. And the patrons, under the colour of their rights

of patronage, are just creating a most anomalous species of extra-academic College, which, though it of itself cannot grant degrees, has the power of communicating to the students resorting to it, the means of compelling the University of Edinburgh to exercise its own power of conferring degrees on parties who, by the rules of the College, hold no proper qualification.

"I think it would be nearly a contradiction in terms to sanction such a claim, as inherent in the office of patrons—who, while their duty is manifest, to protect both the dignity and the pecuniary interests of the professors of the College, have most certainly no right to extinguish or impair either the one or the other."

POSTSCRIPT TO DR BARRY'S PAPER IN THE PRESENT NUMBER.

Kölliker mentions Remak as the first who observed reproduction of the blood-corpuscles by division—an observation first published by Remak in 1841. "Yet," says Kölliker, "he subsequently retracted his opinion; and only now, since I have confirmed it, and declared it to be true . . . . has he again advocated it."—*Lc.* p. 29.

When Kölliker wrote this, he was evidently not aware of observations recorded in the "Medical Gazette," Nov. 13, 1839; for these show that the first intimation of reproduction of the blood-corpuscles by division was given by Professor Owen. And it may be added, that the next record of such observations was by myself, in the Phil. Trans. for 1840. See also a paper of mine in the Phil. Trans. for 1841.

POSTSCRIPT TO DR J. ARNOTT'S PAPER IN THE PRESENT NUMBER.

In a discussion on local anæsthesia from cold, in the Surgical Society of Paris—reported in the French Medical Journals of the early part of this month—several speakers alluded to the alteration of the tissues produced by congelation as a disadvantage of the proceeding. Experience will soon correct this erroneous opinion, and show that the alteration objected to is even a more valuable effect than the anæsthesia. Might not intense cold, properly applied to that part of the face of a stump which cannot be reached from the surface, tend, by its antiphlogistic power, still farther to prevent the dreadful mortality from purulent deposit, which occurs after amputation in the Paris hospitals? The alteration of tissues is only produced by the severer applications of cold: where it is not an object to prevent inflammation, and only a very superficial anæsthesia is desired, these are not required.

VARIETIES.

How Families are sometimes Poisoned.—Dr Alfred Taylor, one morning, when about to cut the loaf on his table for breakfast, observed green patches and streaks on the crust. He also observed it in the toast. He scraped from the bottom of one loaf about two grains of this substance, which on the application of Rensch's process, he proved to consist of Scheele's green, a pigment which contains about 50 per cent. of white arsenic. On entering the shop of the baker from whence the bread had been procured, it was ascertained that the shelves on which the loaves rested had recently been painted of a bright grass green colour. These when warm had imbibed a quantity of the poisonous pigment; and on one loaf there was enough to have killed an infant. A nursemaid about to quit a service for misconduct, might have been seriously implicated by such an occurrence as this, unless the cause had been traced. The baker at Dr Taylor's request had the tops of his shelves planed off, so that the loaves should rest upon nothing but unpainted deal.—*Pharmaceutical Journal*

Instruction of the Deaf and Dumb.—The first systematic attempt to instruct the deaf and dumb was made by Petro de Ponce, a Benedictine monk, at Sagahun, about the middle of the sixteenth century, during that bright period

when the literature of Spain was graced, and its conquests extended by men of the highest genius. It is related by the historian of that celebrated ecclesiastic, that he taught the two mute sons of a Castilian nobleman, and a young Aragonese, to read and also to write with elegance, not only the language of their own country, but likewise the Latin tongue; and it is moreover stated, that these persons could understand by sight the motions and expressions of the lips, and that they also spoke as do those who have been taught in a modern deaf and dumb institution. De Ponce died in 1584; but his system appears to have been followed up by his countryman, the monk, John Paul Bonet, who published at Madrid, in 1620, a work upon the mode of teaching the deaf and dumb.—*The Census of Ireland for* 1851.

DEATH OF DR STANGER.—This gentleman who held the appointment of Government Surveyor General of the Port Natal district, died on the 14th of last March, at 42, a victim to hydropathy. He had travelled from Maritzburgh to Port Natal on horseback, and in order to relieve the fatigue he felt, was induced to submit to the application of the "wet-sheet." The next day inflammation of the lungs took place, which carried him off in one week.—*Literary Gazette.* Dr Stanger will be well remembered by his fellow students in Edinburgh from 1833-37, as one distinguished in many branches of natural history. His enthusiasm for his favourite studies induced him to join the disastrous Niger expedition, and it was mainly owing to his energy in conjunction with Dr Macwilliam that one of the steamers was brought down the river. Had he cultivated medicine more, his country and science might still be deriving advantage from his labours.

THE BULWER LYTTON GOLD MEDAL.—The gold medal offered by Sir Edward Bulwer Lytton to the students of the University of Edinburgh, will be awarded by the Senatus Academicus to the author of the best essay—"ON THE INFLUENCE EXERTED BY THE MIND OVER THE BODY, IN THE PRODUCTION AND REMOVAL OF MORBID AND ANOMALOUS CONDITIONS OF THE ANIMAL ECONOMY." The competition is open to all students matriculated at the University of Edinburgh during the Session of 1853-4, and the essays are to be given in at the university library on or before the 25th of December next. Each essay must bear a motto, corresponding with one on a sealed envelope, including the author's name, and address.

INDUSTRIAL PATHOLOGY.—On the 7th ult. Dr T. K. Chambers of London, read a very interesting paper under this title to the Society of Arts, embracing a general resumé of the diseases produced by different kinds of labour. Amidst many excellent illustrations of his subject, he said, "I show you here a rotten jaw-bone, which Mr Simon was obliged to cut out of a man's head, because it was corroded by the noxious fumes evolved in the manufacture of lucifer-matches. It is to be hoped that there is some mode of making them without rotting men's jaws, and this mode it is the business of Industrial Pathology to find out."—*Society of Art's Journal.*

SIR JOHN FORBES'S NEW MEDICAL BILL.—Sir John Forbes has not forwarded to us a copy of the new bill he has drawn up, but we have carefully perused it in some of the medical journals. What especial qualifications are possessed by Sir John for the task he has undertaken, does not appear. But we can safely say, this new attempt at medical legislation, contains so much objectionable matter, as to stand very little chance of ever becoming the law of the land. We shall allude to some of its clauses in a future article on the subject of medical reform.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.—The next meeting of this Association is fixed for Wednesday, September 20th, at Liverpool, under the presidency of Lord Harrowby. The discouragement of late years thrown upon the subjects of anatomy, physiology, and pathology, as sciences connected with medicine, has greatly diminished the number of professional visitors to

these pleasant meetings. We should be glad to learn that in Liverpool, as in Edinburgh, a physiological section could be established.

Leaves of the Olive a Febrifuge.—Mr Sidney H. Maltap of Smyrna, discovered, when in the Island of Mytelene in 1843, at a time when fever was raging there, and all the quinine was exhausted, that a decoction of these leaves was strongly febrifuge. Knowing that biberine and salicine were often used for fever and ague, he thought of all the bitters which might prove effectual—many were poisonous, but he at length tried Olive leaves in the form of decoction, two handfuls boiled in a quart of water, till evaporation had reduced it to a pint. This he gave in doses of a wine glassful every three or four hours. Many obstinate cases of fever were cured, and for many years he has found it more effectual than quinine.—*Pharmaceutical Journal.*

Circulation of the Weekly Medical Journals.—An innocent writer in the *Gazette des Hopitaux*, for June 10 (A. Bernard), has inserted a *feuillton* on the circulation of the English Medical Journals, founded on the statements of one of them. This writer is not aware that according to their own accounts, each of these Journals possesses a circulation far greater than any of its rivals. Mr Churchill says the Lancet has the greatest, and his own Journal the Medical Times, the second circulation. But how should Mr Churchill know more on this subject than the individual Journals themselves?

Medical Officers of the Army and Navy.—The army can obtain medical candidates of a high order at will, and the Director-General has accordingly been enabled to send forth to the East the most splendid medical staff that ever belonged to any campaign, while the Medical Director-General of the Navy finds himself unable to supply more than five candidates to fill thirty-one vacancies, and Surgeons' and Druggists' Assistants are called upon to do the duties that ought to devolve upon Assistant-Surgeons.

Strangling in the Army.—At the Templemore military barracks, a coroner's inquest found the following verdict:—"That on Tuesday morning the 6th inst. (June) Samuel Henfield came by his death from effusion of blood into the pericardium, which proceeded from an aperture in the aorta, having been induced by the pressure of the stock around his neck during the period of his service, and not otherwise." It may be hoped that no further time will be lost in carrying out those reforms in the dress and equipments of the soldier, to which the Government is now fairly pledged.—*Times.*

Professor E. Forbes and the Crystal Palace.—Professor E. Forbes is stated by many of the London papers, to have presented with Dr Latham the handbook of zoology and botany to Her Majesty, on the opening of the Crystal Palace. To our certain knowledge, he was that very day and hour demonstrating to a class of 150 young geologists, the structure of the Craig-Leith Quarry, and neighbouring rocks in the vicinity of Edinburgh.

The M.D. of Durham.—The University of Durham have also applied to Parliament that the holder of its degree of M.D. shall possess all the privileges granted to the M.D. of Oxford and Cambridge. The number of the MD.'s of Durham up to this moment, amounts to the sum total of 0.—*Lancet.*

Ventilaton of the House of Commons.—According to Mr W. Williams' statement, the House, after a prodigious investment in schemes of scientific ventilation, has now discovered that health and comfort in this respect can be secured for the very moderate sum of L.80.—*Times.*

Marine and Fresh-Water Vivaria.—The establishment of these receptacles for animals in the London Zoological Gardens, has given rise to a new taste among naturalists. It is contemplated to exhibit a large vivarium of aquatic animals in St George's Hall, Liverpool, at the forthcoming meeting of the British Association.—*Literary Gazette.*

THE CHOLERA, following its usual progress from east to west, is now devastating many districts in America, especially the localities on the Upper Mississipi.

THE LONDON HAHNEMANN HOSPITAL.—This institution has ceased to exist; the furniture and effects were sold by auction on the 14th ultimo, by Messrs Debenham and Storr.—*Lancet.*

PUBLICATIONS RECEIVED.

Surgical Anatomy. By Joseph Maclise, Esq. Second Edition. Parts IV. and V. London. Folio. 1854.

The Chemistry of Common Life. By James F. W. Johnston, F.R.S., L. and E. Vol. 1. Small 8vo. Edinburgh, 1854. Pp. 352.

A Treatise on Hooping Cough; its complications, pathology and terminations. with its successful treatment by a new remedy. By George D. Gibb, M.D., etc. London. 12mo. 1854. Pp. 395.

The Micrographic Dictionary; a guide to the examination of the structure and nature of Microscopic Objects. By J. W. Griffiths, M.D., and Arthur Henfrey, F.R.S., etc. London. 8vo. 1854. Parts I. and II.

Gymnastics an essential branch of National Education, both public and private, etc. By Captain Chiosso, Professor of Gymnastics, etc. London. 8vo. 1854. Pp. 72.

Result of an Inquiry into the invariable existence of a premonitory diarrhœa in Cholera, in a series of communications to the Registrar-General. By David Macloughlin, M.D., etc. London. 8vo. 1854. Pp. 61.

Six Lectures on the Pathology of Strabismus and its treatment by operation. Delivered at the Westminster Hospital. By C. Holthouse, F.R.C.S.E., etc. London. 8vo. 1854. Pp. 115.

Anatomical and Physiological Observations. By John Struthers, M.D., F.R.C.S.E., etc. Part I. Edinburgh. 8vo. 1854. Pp. 539.

The Baths of France, Central Germany and Switzerland. By Edwin Lee. Third Edition. London. Small 8vo. 1854. Pp. 207.

Suggestions for the future provision of Criminal Lunatics. By W. Charles Hood, M.D., etc. London. 8vo. 1854. Pp. 174.

On the special treatment of Pulmonary Consumption and Hooping Cough. By John Hastings. M.D. London. Small 8vo. 1854. Pp. 171.

Report of the Pennsylvania Hospital, for the year 1353. By Thomas S. Kirkbride, M.D., etc. Philadelphia. 8vo. 1854. Pp. 58.

Annual Report of the Royal Edinburgh Asylum for the year 1853. Edinburgh. 8vo. 1854. Pp. 50.

The art of good and cheap Cookery. By the Editors of the Family Herald. London. 12mo. 1854. Pp. 40.

Clinical Lecture on Stricture of the pyloric orifice of the Stomach, simulating ileus. By D. O'Ferrall. Reprint. 16mo.

Notes on certain discoloured appearances met with on the surface of the sea in warm latitudes. By Dr G. Buist, Bombay. Reprint. 8vo.

Clinical Reports and Observations on Medical Cases. By J. T. Banks. M.D., etc. Reprint. 8vo.

Case of Strangulation of the Jejunum, released by Gastrotomy, etc. By Joseph Ridge, M.D. Reprint. 12mo.

Journal of the Statistical Society of London for June 1854.

CORRESPONDENCE.

Dr Begbie has requested us to correct an error which was reported in the discussion on cholera, in our last January number, concerning the late Mr George White. He has since learnt that, at the time Mr W. was seized with his fatal illness, he was in attendance on a gentleman labouring under cholera. Mr White had also previously seen other cases.

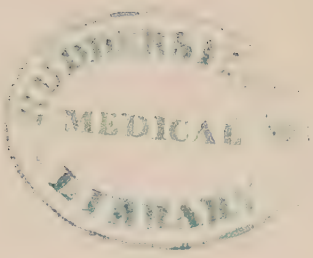
We have received a paper from Mr Daniel March, proposing carbazotic as a test for nitric acid. We regret we have no room for its insertion. The desideratum not supplied by Mr March is to identify a stain on cloth, where there is no excess of acid, or where it has been purposely washed away. We are not aware, however, that any fallacy stands in the way of our present mode of identifying such dry stains, which consists in moistening them with potash. This restores the colour of the cloth, if the acid be other than nitric, but changes the yellow stain to an orange, if that acid produced it.

We have received from Mr Ramsay, of Broughty-Ferry, an ingenious proposal for the construction of a urinal, after the model of certain ink bottles, with an inverted tube, which will not allow the fluid which passes in to be spilt. This, he says, is a great comfort to the sick-bed.

The Proceedings of the Physiological Society will be continued in our next number.

Our reply to Professor Virchow's letter, and the papers of Drs Christison, Lindsay, and Mr Bickersteth, in our next.

Part First.



ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Causes of Colour-Blindness.* By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.

(*Continued from page* 10.)

V.—THEORIES OF COLOUR-BLINDNESS.

THE seat or cause of colour-blindness probably lies altogether beyond our reach; but whatever we can learn concerning it, is certain to be of service in determining the extent to which we may hope to cure or alleviate this affection of vision.

Amidst many minor diversities there are but two prominent physical theories of colour-blindness.

The one of these refers the false perception of colours to the *chromatic condition* of certain portions of the optical apparatus of the eye; the other to the *peculiar organization of its nervous apparatus*, including so much of the brain as is essential to vision. This latter view is now nearly universally adopted, although it is necessarily greatly modified in its mode of statement by the metaphysical or psychological views of those who embrace it.

Chromatic Theory.

The first, or CHROMATIC THEORY (as it may be named), of colour-blindness, has been proposed under one form by Dalton, and under another by Brewster. Both are ingenious and interesting speculations; after considering which I shall offer some suggestions of my own, in connection with a chromatic theory differing from either.

Dalton was not convinced of the peculiarity of his own vision, although he had suspected it before, till, as he tells us, "I accidentally observed the colour of the flower of the *geranium zonale* by candlelight, in the autumn of 1792. The flower was pink, but it appeared to me almost an exact sky-blue by day; in candlelight, however, it was astonishingly changed, not having then any blue in it, but being what I called red, a colour which forms a striking contrast to blue."[1]

At a later period he observed "that a sky-blue transparent liquid

[1] Manchester Mem., 1798, p. 29.

modified the light of a candle so as to make it similar to daylight; and, of course, restored to pink its proper colour by day, namely, light blue."[1] From this observation he came to a conclusion which he thus states: "It appears therefore almost beyond a doubt that one of the humours of my eye, and of the eyes of my fellows, is a *coloured* medium, probably some modification of blue. I suppose it must be the vitreous humour; otherwise I apprehend it might be discovered by inspection, which has not been done."[2]

Prevost appears to have been the only convert whom Dalton gained to his doctrine, during his lifetime,[3] and an examination of his eyes after death, put an end to any prospect of his theory acquiring new supporters. Mr Ransome, who examined Dalton's eyes, states that "the aqueous humour of one of them was found to be perfectly pellucid and free from colour. *The vitreous humour and its envelope* (the hyaloid membrane) *were also perfectly colourless.* The crystalline lens was slightly amber-coloured, as usual in persons of advanced age. The tunics, retina, choroid and sclerotic, with their subdivisions presented no peculiarity."[4]

Further, on looking through the humours of the other eye (after the posterior portion of the outer coats had been removed), "objects of different colours, both by transmitted and reflected light, were examined without any appreciable difference."[5] Among the colours thus examined, Mr Ransome particularises scarlet and green.

This report of course disposes of Dalton's theory so far as his own case was held to support it; but it is worth a brief consideration with a view to guiding future physiological and pathological researches, whether colour-blindness *ever* depends on alterations in the colour of the optical apparatus of the eye.

Physiologists have not recorded as familiar to them, the occurrence of a blue colour in the transparent, internal portions of the eye. Sir David Brewster, indeed, says, "During the dissection of many hundred eyes, I observed in several cases that the *vitreous humour* was of a decided *greenish-blue* colour."[6] In the human subject, however, this must be rare, for Dr W. Mackenzie of Glasgow (Surgeon Oculist in Scotland to the Queen), whose experience as an eye-surgeon I need not enlarge upon, informs me that he has never witnessed this colour in the vitreous humour of the human eye, which in early and middle life has appeared to him "colourless, and in old age yellowish." As colour-blindness, however, is not regarded, either by its subjects or their medical attendants, as a disease, it is not included within the province of pathology, and in truth, so far as I know, Dalton's case is the only one on record of a post-mortem examination of colour-blind eyes. It must still, therefore,

[1] Op. Cit. p. 41.
[2] Ibid. p. 43.
[3] Taylor's Scientific Memoirs, 1846, p. 132.
[4] Henry's Life of Dalton, p. 202.
[5] Op. et Loc. Cit.
[6] Phil. Mag. August 1844, p. 137.

be regarded as desirable to embrace all opportunities of examining such cases in reference to the point before us.[1]

Wartmann, it is true, affirms that even if the transparent media of the eye were blue, colour-blindness would not be the result: "If," says he, "the passage of the luminous rays through a blue medium sufficed to produce Daltonism, the habitual use of blue glasses for spectacles would have long ago confirmed this hypothesis, against which it forms on the contrary a very strong argument."[2]

But this reasoning is of little value. *Daltonism*, or colour-blindness of the kind which characterised Dalton, might not be induced by a blue vitreous humour, but colour-blindness of some kind certainly would be. The experience of wearers of blue spectacles does not furnish an *experimentum crucis* in reference to Dalton's theory. These are pale in colour, are only worn occasionally, and are generally, in virtue of their unavoidable distance from the orbit, and their form and size, inadequate to exclude entirely white light from the eye. Moreover, most wearers of ordinary spectacles, peep at short intervals over and under them, and then receive the impression of uncoloured light, so that their congenital and acquired perception of hues can only be slightly and transiently disturbed by the coloured media through which they *occasionally* gaze. But if any one will make the experiment of covering the eyes with a sheet of deep red, or green, or blue glass, so pressed to the face as to prevent light reaching them otherwise than through it, and then ask another to show him coloured bodies, he will find himself making as many and as flagrant mistakes as any colour-blind person ever did. I have found even glass-stainers who were accustomed to make allowance for the effect of coloured transparent media in altering the tints of opaque bodies seen through them, err very widely in their conclusions; and others were generally wrong in every colour except that which was identical with the glass through which they were looking. In truth, it needs no proof, that to fill the greater part of the eye with a blue liquid, through which alone light is permitted to reach the retina, cannot fail to induce a peculiar vision of colours, and that this cause of *chromatopseudopsis* (properly so called), is worth the attention of physiologists and pathologists.

At the same time, I entirely agree with Wartmann, in thinking

[1] The infrequency of such opportunities, and the corresponding desirableness of improving them, is strikingly shown by the following statement with which Dr Mackenzie has favoured me:—

"A curious fact about colour-blindness is the extreme rarity with which the subjects of it apply for advice to oculists regarding it.

"During the last thirty years I have treated at the eye infirmary and in private, upwards of 4000 cases of eye disease; and out of this number I do not recollect more than two of colour-blindness. One of these was a calico-printer, distinguished for his taste and success in his own line, and very unwilling to let his defect be known. I suspect this is generally the feeling of colour-blind people."

[2] Taylor's Scient. Mem. 1846, p. 182.

that Dalton's theory "could not explain the diversity of names given to the same colour by different Daltonians."[1] A colour-blindness, indeed, dependent on the humours of the eye being coloured, would be of a much simpler and more uniform kind than that characterising the cases which I have described. As exhibited by an eye otherwise perfect, it would show itself as a "constant error," the extent of which could, by comparison with the vision of normal eyes, be ascertained and allowed for; and as it could not occur in any marked degree, unless the humours were deeply coloured, it would be accompanied (especially if they were red or blue) with a faint perception, in virtue of their imperfect illumination, of the forms and outlines, as well as of the tints of bodies; whereas true colour-blindness does not necessarily, or even generally, involve in doubt the discrimination of aught but their colours.

A colour-blindness of this kind should also be characterised by that colour being seen best (*i.e.*, most normally), which tinged the transparent media of the eye. A blue vitreous humour, manifestly, would not prevent *blue*, nor a red one *red*, from being seen (a difference of shade only excepted) as normal eyes see them; and if true colour-blindness were thus chromatic in its origin, it is *yellow*, not blue, which should be found predominating in the eyes of those who shared in Dalton's peculiarity, for yellow is the colour which they see best. But there is no reason to think that the slight tinge of this colour which was found after death in the lens of Dalton's own eyes, was at all more intense than that which appears in the eyes of those who, with a perfect perception of colours, live to so advanced an age as he did. Moreover, Mr Ransome's observations on the unchanged aspect to his eye of red and green, *ex. gr.*, when seen through the transparent media of the deceased philosopher's eyes, render needless any further proof that colour-blindness, such as Dalton's, could not be induced by the change in colour which the crystalline body had undergone. This change, which, as occurring in advanced life in the lens, goes the length of developing in it an amber colour, in addition to a paler yellow tint in the vitreous humour, does not, according to the universal testimony of eye-surgeons, lead to any alteration in the judgments of colours. In truth, the aged are, in one respect, gainers by this coloration of the humours, for there is good reason to believe that Sir D. Brewster is right in his opinion, that "the retina receives a more powerful luminous impression from yellow light, than from the pure white light of which this yellow forms but a part;"[2] and if this conclusion be just, the diminished sensibility of the retina in advanced life is, to some extent, atoned for by the altered colour of the light which falls upon it. There must, unquestionably, be some slight change in the perception of tints, as a drawback to the gain from increased illumination, but this altera-

[1] Op. et Loc. Cit.

[2] Lond. and Ed. Phil. Mag., 1844, p. 139.

tion has, no doubt, occurred too gradually to be sensible to the subject of it.

Sir David Brewster's chromatic theory of colour-blindness is referred to by himself, simply as a conjecture, and is best given in his own words. He is replying to a criticism of Wartmann's, and after some remarks continues :—"During the dissection of many hundred eyes, I observed in several cases . . . . that the *retina* had a marked *French grey* or *pale blue* tint, which decidedly absorbed red light. I knew that in cases of colour-blindness the vitreous humour was not *blue*, or even *greenish-blue*, as Dr Dalton conjectured; but I could not assert that in the same cases the retina might not be blue, and hence I was led to hazard the idea of a blue retina as one which might be admissible as a cause of colour-blindness, but *only on the supposition* that the choroid coat should prove to be the seat of vision."[1]

I am not aware that Sir David has sought, in later publications, to extend this supposition into a full conclusion. He is at one with the majority of philosophers in regarding the retina as the seat of vision; but his conjecture deserves notice as calling attention to the *colour* of the retina as a possible cause of colour-blindness. I do not know by what arguments it could be rendered probable, that the choroid coat performs the function usually ascribed to the expansion of the optic nerve; but excluding altogether the supposition that it can, it appears to me that a coloured retina could not fail to alter the perception of colours, even on the received view that it is itself the seat of vision. To this, and to certain other speculations concerning the extent to which the colours of the *membranes* within the eye may alter its chromatic perceptions, I now turn.

The subject may be conveniently discussed under three heads,

1. Relation of the general colour of the retina to colour-vision.
2. Relation of the *yellow spot* of the retina to colour-vision.
3. Relation of the colour of the choroid to colour-vision.

1. *Relation of the General Colour of the Retina to Colour-Vision.*

It would be putting a question which no one can answer, to inquire, Can the retina transmit to the brain, or determine in the mind, the sensation of its own colour? But it is more within our power to answer the allied question, Would the possessor of *ex. gr.* a blue retina have a different perception of blue, from the possessor of the normal pinkish grey one? I think he would, whatever answer be given to the previous query.

A man of genius has declared that the "eye sees all things but itself," but this fine thought is only partially true, for physiologists have long been aware that by many devices (such as the very simple one of moving the eye tremulously, whilst looking through a very

[1] Lond. and Ed. Phil. Mag., 1844, p. 137.

small hole in a card at the clear sky), the retina may be made visible to itself, or at least a perfect spectral copy of it, becomes an object of consciousness. Such being the case, it does not seem to admit of doubt, that were an individual retina to have a marked colour developed in it, this also would be perceived; and that when the possessor of such a retina made the familiar experiment of looking, till the eye was fatigued, at one colour, the complementary colour which then appeared would be tinged by an added hue from the retina.

But apart altogether from this, a coloured retina, though transparent, would certainly reflect some coloured light, and this light, falling on an opposite portion of the membrane, would produce the same impression, as light of the same quality from any other source would do. It seems to me, therefore, certain that, admitting the retina to be physically the seat of vision, its colour, if at all considerable, would render the perception of colours abnormal.

Now, accordingly, that oculists, by means of their ingenious *specula*, can perceive the condition of the retina during life, it would be important to know whether it exhibits a different colour in the colour-blind, than it does in those whose vision is normal; and similar observations could be made in *post mortem* examinations.

Since writing the above, through the kindness of Dr W. R. Sanders, Curator of the Museum of the College of Surgeons, Edinburgh, who first made his countrymen acquainted with the eye-speculum of that accomplished physiologist and natural philosopher, Professor Helmholtz of Koenigsberg,[1] I have had an opportunity of observing, how strikingly the innermost recesses of the living eye can be rendered visible by no more complex apparatus than a gas-flame and a slip of glass. It is very startling to see what for ages has been named the *camera obscura* of the eye light up in a moment into a dazzling *camera lucida*, sending through the pupil, brilliant white, brassy, or crimson-coloured rays, according as the movements of the eye-ball make the optic nerve, the "yellow spot," or the blood-vessels of the retina, the more immediate reflectors of the light which issues. No one who witnesses this very beautiful phenomenon will need further proof that the retina reflects in its normal state much coloured light, which, when the pupil is contracted, must, in considerable part, return from the back of the iris to the retina, and impress it as coloured light from without would do. And if this be the case, *à fortiori* the development in any marked degree of a tint such as blue or red on the whole retina must alter the visual perception of colours.[2]

It does not appear that, hitherto, variations in the general colour of the retina have been observed, at least, in the human subject. Dr

[1] Edinburgh Monthly Journal of Medical Science, July 1852, p. 40.

[2] Helmholtz's description of the apparatus referred to in the text is contained in his work entitled "Beschreibung eines Augen-Spiegels zur untersuchung der Netzhaut im lebenden Auge. Berlin, 1851."

W. Mackenzie informs me that he has never seen it in healthy eyes, other then "reddish or pinkish grey," and such appears to be the general opinion of physiologists who compare it in tint to the grey substance of the brain. I do not anticipate that the tint of the retina will be found to vary much in different persons, but it is one of the points deserving attention in the examination of the eyes of the colour blind.

2. *Relation of the* Yellow Spot *of the Retina to Colour-Vision.*

The remarks previously made apply strictly to the *general* colour of the entire retina, but it appears to me singular, that no physiologist has connected the existence of the normal *yellow spot*, (the *macula lutea*, or *limbus luteus* of its discoverer, *Sœmmerring*) on the human retina, with the perception of colour; but in truth, so far as I am aware, no physiologist has suggested any use for it. The spot in question is thus described by a recent authority:—

"If we cut across a fresh human eye, so as to look at the hyaloid surface of the retina, or if we carefully remove the sclerotic and choroid coats, so as to expose to view its choroidal aspect, we are struck with the rich yellow colour of one small spot about one-twelfth of an inch in diameter, situated at the very bottom of the eye, in the exact axis of the humours, *i.e.* at about one-tenth of an inch from the optic nerve. The colour is deepest in the young adult, much fainter in the old. It exists in some monkeys, and according to Dr Knox, in certain reptiles; but in general it is wanting among the lower animals, while it is quite constant in man. The colour shades gradually off, and is deepest when seen from the inside." [1]

The same author, writing in conjunction with Dr Todd, refers further to the yellow spot in these terms: "It has been described by some as a fold, by others as a foramen in the retina, and after several examinations, we should speak of it as a small mound, or projection of the retina towards the vitreous humour, with a minute aperture in the summit. . . . . As for the colouring matter, it is not in grains of pigment, but stains the several tissues, and soon disappears in water. *The use of the yellow spot is unknown.* It is interesting to observe in connection with the perfection of vision over the spot, that the principal branches of the artery and vein of the retina, above and below, curve round it at a distance, going, as it were, out of their course to avoid it, so that only capillary vessels are found in its immediate vicinity." [2]

There are differences of opinion among physiologists (into which I do not enter), as to the exact structure of the retina at the *limbus luteus*, but all are agreed that from its position at the bottom of the eye in the very axis of vision, it must in man receive the most perfect image of an object which the eye, considered as an optical instrument, can produce; and the production of which we instinctively

[1] Lectures on the Eye, by W. Bowman, 1849, p. 89.

[2] Todd and Bowman's Physiological Anatomy, chap. xvii. p. 30.

secure by turning the centre of each pupil towards those bodies which we wish to perceive most accurately. "Perhaps," observe Todd and Bowman, "it is only in, or very near, the axis of vision, that sight can be said to be *perfect*. The existence of the yellow spot of Sœmmerring at that point, continues a riddle which the most attentive examination of its anatomy has not yet solved. And from the absence of this spot in almost all the lower animals, we are led to doubt its importance to perfect vision." [1]

The almost exclusive limitation of the yellow spot to the eye of man, points to some special service which it renders him. And I hazard the following suggestions in reference to it. According to the testimony of all who have described it, it is exceedingly transparent, and as the retina is of sensible and even measurable thickness, the light which falls upon its vertex, and passes through the *limbus luteus*, must lose most of its blue and red rays before it traverses its thickness and fully impresses it. *It is truly thus by yellow, not by white light, that ordinary perfect vision is exercised by man*, and I cannot help connecting this fact with the special sensitiveness of the retina to yellow light.

The yellow rays of the spectrum are confessedly more luminous than the others, and even seem to make a stronger luminous impression on the eye, than the composite white light (of which the yellow is but a part) does. It has further been amply shown in the preceding pages, that yellow is the colour seen best by all human eyes. I suggest, therefore, that the spot of Sœmmerring is a provision for securing to the human retina the conversion of white light into the more exciting yellow light, which makes the maximum impression upon it.

Nor must it be forgotten that the light which traverses the yellow spot will be deprived, not only of its red and blue, but also of its *actinic* or *chemical rays*. What influence these rays exert upon the vision, either of form or of colour, we do not know; but they cannot fail to affect it, for the retina, like the other portions of the nervous system, is throughout life continually undergoing chemical changes, which are essential to the performance of its function, and these will be quickened, retarded, or altered in their character by the actinic agencies. Now, the yellow spot which absorbs, and retains within its thickness a much larger proportion of the actinic rays which reach it, than other parts of the transparent retina can, must be the seat of chemical changes, differing in kind or degree from those which they undergo; implying a different amount of molecular disturbance; and, as can scarcely be doubted, occasioning a different sensational impression. It may reasonably be inferred, that this impression enhances the perfection of vision, and that one object of the yellow spot is to secure its production. And if such be the case, we can understand why the retina was made yellow, rather

[1] Op. Cit. chap. xvii., p. 55.

than the humours of the eye; for had they been of this colour, the light which traversed them would have lost its actinic rays (besides parting with its red and blue, and heat rays), before it reached the retina; and the impression produced by white light containing a large proportion of its actinic, chromatic, and thermic forces, till the moment of its traversing the retina and changing into yellow light within its substance, must be very different from that occasioned by light which has been filtered through yellow media before it falls upon the retina.[1]

But whatever be the value of these suggestions, it does not, I think, admit of doubt, that the colour of objects must seem different to us, than it does to the great majority of animals in whose eyes there is no yellow spot. Their *white* must be *whiter* than ours, their yellow *less* yellow; and all tints must wear for us "a *jaundiced*" look which they do not show to them. It is already ascertained also, that the colour of the *limbus luteus* grows fainter as life advances, and we may well believe that it varies in area, as well as in intensity of tint in different individuals, whose judgments of colours must in a corresponding degree vary. I do not think that such variations can occasion colour-blindness, but it must occasion diversities of judgment among those who, unlike the truly colour-blind, are prompt and unhesitating in their chromatic decisions.

Examples of these diversities occur every day, among those who have spent the greater part of their lives in dealing with colours. Every one must be familiar with the differences which appear among colourists, otherwise equally practised, in their decisions regarding the less-marked tints and shades. Who has not heard painters, and critics of their works, discuss by the hour "flesh-colour," or "sea-green," without any change of opinion among the disputants as to the true quality of these colours? This is not the result, on the part at least of the majority of those differing in opinion, of colour-blindness, for its subjects are not pertinacious in their conclusions regarding colour, and are not given to dispute regarding it.

I have been much struck with the same phenomenon as appearing among dealers in coloured goods, and dyers. The remark has been spontaneously made to me (and on that account impressed me the more) on four different occasions, by the heads of carpet manufactories and dye-works in Edinburgh and Paisley, that they were quite accustomed to find workmen who would "match" unerringly or dye to a pattern, reds, blues, yellows, greens, purples, browns, and most of the other mixed colours, but who failed to satisfy others with such tints as "drab," "fawn," or "stone-colour," although as

[1] The diathermancy of the humours of the eye is very small, so that few of the heat-rays which fall upon the cornea reach the retina: the actinic rays are said also to be stopped by the transparent media of the eye, but the evidence of this is not satisfactory.

well pleased themselves with their adjustments of these as with those of other colours.

Astronomers who seek to give to their estimate of the colours of the heavenly bodies, the same precision as to that of their other characters, supply still more unequivocal proofs of the existence of what they happily term a "chromatic personal equation" in most individuals.

The learned and ingenious Captain (now Admiral) W. H. Smyth, referring to the different colour which appears in the two components of many of the binary stars, observes, in allusion to the extensive researches of Struve on this point:—"Professor Struve's chromatic designations are, *obscurissima*, *obscura*, *pallida*, *livida*, *sub-flava*, *flava*, *sub-caerulea*, *caerulea*, *rubicunda*, and *rubra;* he supposes the ninth magnitude to be the outside boundary in which he recognises colour, but I have been much struck with the beautiful blue tint of the smallest stars visible in my telescope. This, however, may be attributed to some optical peculiarity. The Professor found, what I have also experienced, that Sir William Herschel saw most objects with a redder tinge than they have since proved to bear. This may be owing to the effect of his metallic mirror, or to some peculiarity of vision, or perhaps both.[1]

Here are three unusually skilful observers, with (as there is every reason to believe), in each case, what must be called a normal sense of colour, differing by a *constant* amount regarding certain tints. And as Admiral Smyth observes elsewhere, in commenting on this and similar facts, "a personal [chromatic] equation, of greater or less amount, occurs in every case, and the reason of the faulty colouring of so many artists, is, that they really are not aware of many of the refinements of colour; there eyes not perceiving them, their fingers cannot render them."[2]

One probable cause of such chromatic idiosyncracies in the normal-eyed, and one source of aggravation of those occurring in the colour-blind, is the condition of the yellow spot as to area, thickness, intensity of tint, and the like; for both by the change which it induces on the light which traverses it, and by the light of its own colour which it reflects across the chamber of the eye, it must influence the impression which coloured objects make upon the entire retina.

I would suggest, accordingly, the inspection by means of such an instrument as the retina-speculum of Helmholtz, of the yellow spot in the living eye, in connection with its powers of vision as regards form, outline, and, not least, colour, as well worth the attention of observers. We may reasonably hope to find a close connection subsisting between the extent and tint of the coloured spot and the visual powers of the eye, and it would be important to

[1] Cycle of Celestial Objects, vol. i., p. 302.

[2] Aedes Hartwellianæ, p. 311, where the whole passage is worth study.

know what differences it presents in those whose colour-vision is normal, and those in whom it is the reverse.

There are difficulties, doubtless, in the way of such observations, which can be made to purpose only by experienced oculists. The appearance of the yellow spot in a perfect eye, as seen by Helmholtz's speculum is thus described by Dr Sanders :—"The retina surrounding the white optic nerve is of a bright red colour, which becomes deeper towards the periphery, and is caused probably by the capillary blood-vessels, too small and too faintly illuminated to be distinguished from the grey substance of the retina. . . . . The yellow spot of Sœmmerring or spot of direct vision, is of a dimmer yellowish-grey colour, without trace of capillary vessels; its observation is rendered difficult by reflection from the cornea; and the luminous image is much less bright than on the adjoining parts of the retina."[1]

These remarks apply, of course, solely to the vision of man; but although the yellow spot is not found in the retina of the majority of the lower animals, certain remarkable coloured objects occur in the "rods and bulbs" of that membrane in these creatures, which, like the yellow spot of the human eye, may affect the vision of colour. The objects in question show themselves in the outer or choroidal ends of the rods and bulbs as "globules resembling oil, either colourless or possessing most brilliant tints of yellow or crimson. In the Chelonian reptiles and in birds these are the most beautiful."[2]

A portion of the retina of the tortoise, Mr Bowman tells us, shows on its outer aspect, when seen through the microscope, "a most elegant array of pale, of yellow, and of crimson globules, scattered with regularity over the surface, the first being the smallest, and the last the largest, ...

"In birds we have even a more beautiful pattern of colours. In the sturgeon, among fishes, I have found the globules large but colourless. In the mammalia they are either very small or wanting."[3]

I am informed that the skilful histologist, Hannover, has made these singular bodies objects of special observation, and has figured them in his "Recherches Microscopiques sur le Systeme Nerveux, 1844." The subject is one lying beyond the compass of my familiar knowledge; but these remarks may perhaps induce competent inquirers to study the globules in the retina of many of the lower animals in relation to their possible influence on the vision of colour.

[1] Edinburgh Monthly Medical Journal, July 1852, p. 44.
[2] Bowman on Vision, p. 88. [3] Op. et Loc. Cit.

ARTICLE II.—*On the Relative Influence of the Male and Female Parents in the Reproduction of the Animal Species.* By ALEXANDER HARVEY, M.D., Edin., late Physician to the Aberdeen Royal Infirmary, and formerly Lecturer on the Practice of Medicine in the University and King's College of Aberdeen.

AT a meeting of the Newcastle-upon-Tyne Farmer's Club, held on the 4th March last, Sir Matthew White Ridley, Bart., in the chair, Mr Orton of Sunderland read a paper on the "Physiology of Breeding," which excited great interest among the members present, and has since been published in the *Newcastle Chronicle* (March 10th).

The views therein set forth, supported as they are by a large body of evidence, seem well deserving the attention of the physiologist. Moreover, as they have important practical bearings in relation as well to our own species as to the lower animals, they possess a general interest, and concern not merely the physiologist and the farmer, but the physician also, the vital statist, the medical jurist, and others.

Mr Orton's argument is, that in the reproduction of the animal species there is no casual blending of the parts and qualities of the two parents, but that each parent contributes to the formation of certain structures and to the development of certain qualities. And advancing a step further, he maintains that the male parent chiefly determines the external characters, the general appearance, in fact, the outward structures and locomotive powers of the offspring (*e.g.*, the brain, nerves, organs of sense, and skin, and likewise the bones and muscles more particularly of the limbs), while the female parent chiefly determines the internal structures and the general size and quality, mainly furnishing the vital organs (*e.g.*, the heart, lungs, glands, and digestive organs), and giving tone and character to the vital functions of growth, nutrition, and secretion.

Not, however, that the male is wholly without influence on the internal organs and vital functions, or the female wholly without influence on the external organs and locomotive powers of their offspring. The law holds only "within certain restrictions." These may be said to constitute a secondary law—the law of limitations, equally important to be known as the fundamental law itself, and to be hereafter considered.

Such, according to Mr Orton, is a general fact or law of Nature. Claiming the merit of having worked it out by independent observations of his own, Mr Orton, nevertheless, gives the credit of its discovery to the late Mr Walker, and refers to this author's work on *Intermarriage*, as embodying views which, with certain qualifications, are identical with his own. It is scarcely necessary to remark, that this circumstance—of two independent observers in the same field of inquiry arriving unknown to each other at the same point—imparts to their common conclusion a strong *à priori* probability.

In proof and illustration of his general proposition, Mr Orton adduces a large collection of what appear to be strikingly apposite examples, taken from three several divisions of the animal kingdom, mammals, birds, and fishes.

Premising, what will afterwards more fully appear, that crosses or hybrids furnish the most remarkable examples of the proposition, and serve best to test it, we will now endeavour fairly to exhibit the kind and value of the evidence brought forward in support of it.

The *mule* is the produce of the male ass and the mare; the *hinny* (or as it is also called the *mute*), that of the horse and the she ass. Both hybrids are the progeny of the same set of animals. They differ widely, however, in their respective characters—the mule in all that relates to its external characters having the distinctive features of the ass,—the hinny, in the same respects, having all the distinctive features of the horse; while, in all that relates to the internal organs and vital qualities, the mule partakes of the characters of the horse, and the hinny of those of the ass.

"The mule, the produce of the male ass and mare, is essentially a *modified ass:* the ears are those of an ass somewhat shortened; the mane is that of the ass, erect; the tail is that of an ass; the skin and colour are those of an ass somewhat modified; the legs are slender and the hoofs high, narrow, and contracted like those of an ass. In fact, in all these respects it is an ass somewhat modified. The body and barrel (however) of the mule are round and full, in which it differs from the ass and resembles the mare.

"The hinny (or mute), on the other hand, the produce of the stallion and she ass, is essentially a *modified horse:* the ears are those of a horse somewhat lengthened; the mane flowing; the tail is bushy like that of the horse; the skin is finer like that of the horse, and the colour varies also like the horse; the legs are stronger, and the hoofs broad and expanded, like those of the horse. In fact, in all these respects, it is a horse somewhat modified. The body and barrel (however) of the hinny are flat and narrow, in which it differs from the horse and resembles its mother the ass."

In connection with these examples, Mr Orton refers to a special feature seen equally in the two instances, and which seems at first sight a departure from the principle laid down by him. It is this: Both hybrids, the mule and the hinny, take after the male parents in all their external characters, save one, which is *size*. In this respect they both follow the female parents, the mule being in all respects a larger and finer animal than its sire the ass; the hinny in all respects a smaller and inferior animal to its sire the horse; the body and barrel of the mule being large and round; the body and barrel of the hinny flat and narrow; both animals being in these particulars the reverse of their respective sires, but both resembling their female parents.

In explanation of this seeming exception, Mr Orton adduces a well-known principle in physiology. The principle is, that the whole osseous framework is moulded in adaptation to the softer structures immediately related to it, to the muscles covering it in the case of the limbs, and to the viscera in that of the great cavities which it assists in forming. Accordingly, in perfect harmony with Mr

Orton's views, the *general* size and form which must be mainly that of the *trunk*, will be determined by the size and character of the viscera of the chest and abdomen, and will therefore accord with that of the female parents by whom the viscera in question are chiefly furnished.

On the important but difficult subject of the restrictions within which the general law holds, or what may be called the *law of limitations*, Mr Orton makes some judicious observations :—

"I do not mean it to be inferred" he says, "that either parent gives either set of organs uninfluenced by the other parent; but merely that the leading characteristics and qualities of both sets of organs are due to the male on the one side and the female on the other, the opposite parent modifying them only. Thus, I do not infer that the ass has alone been the agent in conferring the external characteristics of the mule, but merely that he has principally conferred the developments, while the mare has been in regard to the external organs a secondary agent, an instrument not of conferring, but only of modifying those organs. It is just the reverse, however, with the vital organs. The female is the agent in conferring them, the male only an agent in modifying them. Hence I conceive that though the male and female parents in all cases give—the former the external, and the latter the internal organs, yet they each mutually exercise an influence in modifying to a greater or lesser extent the organs given by the other."

The instances given are in fact a proof of this. The mule, though essentially a modified ass, and the hinny, though essentially a modified horse, are neither of them anything else. In neither hybrid have we the perfect head and limbs of the ass or the horse grafted as it were on the proper body and barrel of the horse or the ass. The animals are both of them *composite*, though so specifically distinct the one from the other, and taking each of them to such an extent after their respective sires as to warrant the distinctive appellations given them by Mr Orton.

It was formerly remarked that crosses or hybrids serve best both to test and illustrate Mr Orton's law. It will now appear that in the first instance the law in question can only be satisfactorily investigated by observations confined to animals of this sort. For, if even in crosses or hybrids there is more or less of a blending, in every part, of the organs and qualities of the two parents, it is plain that in animals the produce of parents of the same species, and of the same breed or variety, this blending, although not necessarily greater than in crosses, must yet be such as often effectually to obscure to the ordinary observer the distinctive features of either parent. It is owing, doubtless, to this, that there is such diversity of opinion even among practical men on the subject, as any one will find by looking into any of the many treatises on cattle. The law, however, once clearly established in the way suggested, and the conditions or limits within which it holds ascertained, may afterwards be more easily and confidently applied to all cases.

Besides the influence which either parent exerts on the parts or qualities furnished by the other, and which may be regarded as

innate and essential, various other influences control and modify the general law. One of these, and that on which Mr Orton chiefly dwells is, the effect on the breeding powers of the female produced by the male that *first* had *fruitful* intercourse with her, as seen in the impress of that male on the progeny she may subsequently have by other males. Of this we have a notable instance in Lord Morton's Arabian mare, which, after having a hybrid by a Quagga, bore successively three foals, every one of which had distinct traces of the Quagga. Having formerly[1] given a large collection of cases

[1] "Monthly Journal of Medical Science" for October 1849, and September 1850; and pamphlet by the author "On a Remarkable Effect of Cross-breeding," 1851. As supplementary to the examples of this influence there given, the following additional examples of it are extracted from Mr Orton's paper.

Referring to Lord Morton's mare, and to the influence exerted on her reproductive powers by the Quagga, Mr Orton says :—

"I myself made an experiment in illustration of it. Having procured a white bull-and-terrier bitch, which had previously been breeding with coloured dogs, I caused her to breed with a bull-and-terrier as like herself as well could be, and also pure white, reasoning that if this male influence existed, then the bitch, although breeding with a white dog and herself white, should throw coloured puppies. In her first litter of nine, there was not one which was not more or less coloured. In her second also of nine, four were white and five coloured. In her third (also to the same dog) all were white. The bitch had evidently been influenced by the previous impregnation, and it was also evident that by continuation of the influence of the white dog, the *stain*, if we may so call it, was gradually obliterated. Her fourth litter were all pure white. It was my intention to have carried her back to the coloured dog, reasoning that she would then breed one or two litters of white dogs; but unfortunately she ceased to breed, although lined by a coloured dog. I regret this the more, as I shall be years in bringing another animal to the same condition for experiment.

"I have, however, tried the same with pigs, breeding from a white sow and an Essex boar, and with the same result. The white sow had three litters from the Essex boar, and was then served by a white boar; three out of seven of the litter were coloured.

"About two years since I engaged to take two pigs from a litter. The boar and sow were of the improved Essex breed, and had both carried premiums. I of course looked for good bred pigs from them; but on going to make my selection I found the largest and best of the litter with more or less white on the feet and fetlocks. I protested against the purity of the breed, but being assured of it, it occurred to me that this might be a case of influence from previous impregnation. I asked the question without stating my reason for it; and at once it was admitted that previously to the owner procuring his Essex boar, the sow had been breeding with a white boar.

"Mr Storey of Hartwarren, had a brown mare which for its first foal was put to what he called a 'large, long-legged, Cleveland horse.' The produce was a brown foal. The mare was next twice served by a *black, short-legged* horse, and had two foals to him, both of which were bay, and, in Mr Storey's own words, 'large, tall, long-legged, Cleveland-looking horses.'

"Having myself a mare which I prized, I determined on breeding from her. She was grey, and was put to Wizzard, a grey horse, the produce was a brown colt. She was next served by Sheffield, a bay horse; the produce was a grey filly. She was then served by Clinty, a grey horse, and the produce was a bay colt. It is curious to observe that though grey herself, and twice served by grey horses, she only once threw the grey, and, moreover, that her foals

of this sort, and endeavoured to show that the influence thus exerted by the male on the reproductive powers of the female is a *constitutional* one, and that it is brought about by the *fœtus in utero* INOCULATING the mother's system with the constitutional qualities of its sire inherent in it by necessary derivation from him, it may suffice to observe under this head, that among the lower animals, by reason of the promiscuous intercourse that obtains, the agency of the cause in question in modifying Mr Orton's law must be frequently in operation, and a source of error in conducting observations as to that law.

The foregoing comprises most at least of the general statements bearing on the subject referred to by Mr Orton. We may now proceed to the other illustrations given by him in support of his theory :—

(1). "The mule and hinny," says Mr Orton, "have been selected and placed first, because they afford the most conclusive evidence, and are the most familiar. Equally conclusive, though perhaps less striking instances may be drawn from other sources. Thus, it has been observed that when the ancona or other sheep are allowed to breed with common ewes, the cross is not a medium between the two breeds, but that the offspring retains in a great measure the short and twisted legs of the sire.

"Buffon made a cross between the male goat and ewe ; the resulting hybrid in all the instances, which were many, were strongly characteristic of the male parent, more particularly so in the hair and length of leg. Curious enough the number of teats in some of the cases corresponded with those of the goat.

"A cross between the male wolf and a bitch illustrates the same law ; the offspring having a markedly wolfish aspect ; skin, colour, ears, and tail. On the other hand, a cross between the dog and female wolf afforded animals much more dog-like in aspect—slouched ears and even pied in colour. If you look to the descriptions and illustrations of these two hybrids, you will perceive at a glance that the doubt arises to the mind in the case of the first, 'What genus of *wolf* is this ?' whereas in the case of the second, 'What a curious *mongrel dog !*'

(2.) "Amongst *birds* we have the same results, and they afford the like illustrations to our subject. Those who have had much to do with pigeons must have perceived that a cross between a *carrier* cock and *dragoon* hen is always a fine bird, and very nearly equal to the carrier ; whereas, a cross between a dragoon cock and carrier hen results in nothing better than a dragoon. Precisely the same may be observed in the cross between the *tumbler* and *pouter*.

"It is curious to observe, that the proposition I make regarding male influence should not only have been observed, but distinctly stated in so many words. Mr Lloyd says, 'The *capercailli* occasionally breed with the *black grouse*, and the produce are in Sweden called *racklehanen*. These partake of the leading characters of both parents, but their size and colour greatly depend upon whether they have been produced between the capercailli cock and grey

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never were the colour of the horse by which it was got, but always of the one which preceded him.

"It is remarkable that though the knowledge of this curious influence has only lately dawned upon Europe, the Asiatic seems to have been in possession of it. In Abdel Kader's celebrated letter on the Arab horse, the principle is touched upon. He states that a mare having bred with the ass is no longer fit to breed from—why, he does not say, perhaps does not himself know, but we can now easily see the reason."

hen, or *vice versa*.' (Yarrel, p. 298.) The hybrid between the pheasant and grouse is a striking illustration, showing so clearly its male parent: in almost all respects it is a pheasant, only the tail slightly shortened. It may be observed too, that the feathered feet of the grouse have disappeared in the offspring (Ibid. p. 309). Another instance of the same cross is given (p. 311), in which the general characteristics are those of the pheasant; and this would have been still more striking if the tail had not been spread—a liberty, I suspect, either of the artist or the stuffer of the specimen. The legs in this instance are slightly feathered. Another hybrid is given (p. 313), between the *ptarmigan* and the grouse. Although the precise parentage of the bird is not stated, I am perfectly satisfied that in this case the grouse has been the male parent, and the tail indicated this, being somewhat forked and divergent. In your museum there is an interesting specimen illustrating the same law—a hybrid between the pheasant and grey hen. In this case the produce is pheasant-like in aspect, tail like the pheasant, but somewhat spread, no appearance of forking of the tail.

(3.) "Even in the breeding of *fish* the same law has been observed. Sir Anthony Carlisle produced mule fish by impregnating the spawn of the *salmon* by means of the male *trout*. The result I give in his own words:—'These mules partook of the character of the trout more than of salmon. They had bright red spots on their sides, but the black colour was shaded downward in bars like those of the perch. The tails were not forked like those of the salmon, as I have seen them in the Thames *skeggers* (from which I infer the male salmon in that case to have been the impregnators.') We thus see in the case of fish, as in that of animals, the male parent giving the external characteristics; those produced by the male trout had not forked tails. The skeggers, on the other hand, produced by the male salmon had forked tails."

In the course of his paper, Mr Orton adduces some special evidence in support of that part of his argument which relates to the office of the female in the work of reproduction. Admitting, as he does, that on this side of the subject the evidence is not so certain or conclusive, he gives several instances bearing chiefly on the milking, and on the nutrient and fattening qualities of the offspring, which qualities, he alleges, pass chiefly on the side of the female parent. He dwells at some length on the history of the *Short-horns*, the origin of which he conceives furnishes evidence in this direction. He refers to Mr Bakewell's Dishley sheep, as deriving and maintaining their celebrity through the ewes. And he adduces an observation which, if well founded, is of great pathological importance, and of especial interest to members of the medical profession, as well as to the directors of associations for life assurance. It is this: that diseases of the vital organs (and it may be presumed, therefore, diseases primarily involving the vital functions of nutrition and secretion) are transmitted oftener, and in a more intense form and degree on the side of the mother than that of the father. And he adds, that in the matter of life assurance, he has long been in the habit of judging of the value of a life by the family history on the female side. In perfect accordance with this position, and if well founded in their degree confirmatory of it, two observations may be cited; the *first*, that the daughters of a woman who has herself borne a large family, are often equally prolific as their mother; a fact, if it be one, not

without interest to those to whom an heir-male of their own body is an object; the *second*, that the daughters of mothers that have borne twins oftener than other females give birth to twins.

In connection with this branch of his subject, Mr Orton draws a distinction between a part or organ, including its vital endowments, and the *quality* of the organ and its endowments. And, while maintaining that the "outer" structures are chiefly furnished by the male parent, he equally holds that the quality of these, as of all the organs, comes mainly of the female. By quality, he obviously means what the older physiologists included under the term *vis vitæ*, or what in ordinary language is called *stamina*. It is not, for example, the special endowments of the nervous and muscular systems, the powers of contractility and nervous agency, considered *per se*, which he says are given by the female, but the quality of these as now defined, and which may be either good or bad. This allegation he illustrates by a reference to the "Short-horns," and Mr Bakewell's breed of sheep. But it will perhaps be best understood by a reference to the horse. "The Arab," he says, "will let you have his stallion; but his mare at no price. He cultivates *endurance* and *bottom*, and the female gives them. He does not know the law we are promulgating; but he acts as if he did, for experience has taught him. The English breeder, on the other hand, values the stallion. He cultivates *speed*, and he finds that the sire gives the locomotive organs; consequently his value, just the reverse of the Arab; his mare is easily got at, but his stallion is priceless."

This distinction, if a real one, properly comes within the law of limitations, formerly spoken of, and forms an additional article of it. It is plain that it will be an additional source of difficulty and embarrassment in conducting observations as to the main law, but a means also of explaining seeming anomalies or discrepancies. Declining to give an opinion regarding it, it may be remarked in passing, that while *talent* is notoriously often hereditary in the male line, it has often been observed of individuals that have risen to distinction among their fellow-men, either by their *power* of intellect or *force* of character, that they have owed their pre-eminence to their mother.

The foregoing is a tolerably full abstract, and it is hoped a fair representation of Mr Orton's views. It would have been desirable to compare them with Mr Walker's, and to conjoin a like abstract of the facts and arguments of the latter. This we are at present unable to do. Mr Orton's own paper, however, is so well fitted to direct attention to the subject, and to give a definite bearing to scientific inquiry into it, that we would fain believe that the account now given of that paper will be not unacceptable to the profession. And submitting for their consideration some further remarks on the general subject, we will only here observe, that whether Mr Walker and Mr Orton's theory of it be correct or not, the process which they have undertaken to explain, and every minute detail of it, must be

regulated by fixed law, and that the discovery of this law, if at all possible, must be reached through such observations as those founded upon by Mr Orton.

Connected with the subject of Mr Orton's paper, there are some facts and considerations not referred to by him, but in entire harmony with his theory, and which may serve both to lend support to it, and to impart to it a deeper practical interest.

1. While the fœtus is developed from that part of the ovum called by physiologists the *germinal* membrane, this membrane itself consists of *two* layers, an outer and an inner, called respectively the *serous* and the *mucous*. Of these layers, each gives origin to a special set or system of organs; the outer (or serous) to the brain, nerves, organs of sense, and integuments, and likewise to the bones and muscles; the inner (or mucous) to the lungs, glands, digestive organs, etc.

That is to say, the outer layer gives rise to the whole set of organs concerned in the strictly *animal* functions, while the inner layer gives origin to those concerned in the strictly *vital* functions. It is scarcely necessary to remark how the very keel and foundation (so to speak) of every animal is laid down by nature in a manner which very exactly tallies with Mr Orton's position; or how the observations of the pure physiologist and those of the mere naturalist coincide!

With regard to the heart and blood-vessels, they would appear either to be the joint production of the two layers or to originate in an intermediate layer subsequently developed and called the *vascular* layer. But, however this may be, it may be remarked that the whole vascular system stands in the same relation to the vital organs and their functions that the osseous does to the muscular and to the locomotive powers,—that is, is *subordinate* to them. The vessels are mere carriers of the nutrient fluid, the blood,—mere channels of irrigation to the vital organs; not taking any other or more direct share in the vital processes; the activity and whole character of which are determined by their own organs and the specific endowments of these.

2. It is now clearly made out as a matter of fact, by the researches of Wagner, Bischoff, and others, that in the impregnation of the mature female ovum, the essential element of the male semen (a spermatozoon) is brought into actual *contact* with the ovum; and there can be no reasonable doubt that such contact is an indispensable condition in the process.

But there is another fact of even greater importance in relation to our present subject, for which we are indebted to the late Mr Newport. The spermatozoon is not simply brought into contact with the ovum, but into the closest possible *union* and *incorporation* with it. When that contact occurs, according to Mr Newport, the spermatozoon embraces the ovum (ovule) on every side, grasping it firmly, but quickly undergoes solution and disappears; being, in

fact, either absorbed by or becoming intimately blended with some part of the ovum.

Further than this, we cannot at present go. We are still ignorant of the relation which, in this act of union, obtains between the spermatozoon and the two layers of the germinal membrane that are subsequently evolved. And as the spermatozoon so quickly disappears by solution, it seems impossible that we shall ever be able to determine as to this. But as the spermatozoon comes first into contact with the *exterior* of the ovum and disappears while lying there, it seems not unreasonable to suppose that its influence while extending to the outer layer may be in a great measure confined to it. And Mr Orton's law (if a real one) would go far to establish this supposition. That layer gives origin to the "outer" structures of the animal, *i.e.*, to the nervous, osseous, and muscular systems; while, according to Mr Orton, it is the male parent that chiefly furnishes these.

So much for the facts. The following considerations seem not unconnected both with those now stated and those adduced by Mr Orton.

First, Of the several parts composing the animal body, and of the several functions performed by it, those called by physiologists the *animal* in contradistinction to the organic or vital, are all that can properly be regarded as essential to any animal and distinctive of it. The vital organs and their functions have no other objects in the economy than the preservation and maintenance of the animal organs; their growth in the first instance, and, contingent on the tear and wear which their exercise involves (and that in a very peculiar degree), their renewal during the whole period of life. But for this, there would be no need of the vital organs and their functions —which do not differ, it may be added, in their essential nature from those of vegetables.

In short, brain, nerves, and organs of sense, bones, and muscles,— the organs and instruments of a *sentient* being, and bestowed for the purpose of establishing the relations of this being to external objects, and of investing it with more or less of voluntary power over the surrounding world—are all that properly enter into our idea of an animal. And possibly in our future state of being, in which, as Scripture teaches, we shall hunger no more, nor thirst any more, in which there will be no decay, neither disease, nor death, and in which likewise there will be neither marrying nor giving in marriage, the "incorruptible" and "glorified" body, suitably modified doubtless in adaptation as well to the absence of parts no longer needed, as to the enlarged capacities and powers of its immaterial inhabitant, may consist of nothing more.

Secondly, Keeping in view what has been already said as to the origin of the external structures in the outer layer of the germinal membrane, and likewise as to these being the structures which are

specifically distinctive of the animal; it may next be observed, that of each species of animal, the *male* must be regarded as the proper representative. Our great progenitor, *Adam*, was not only formed first, but lived sometime—longer perhaps than we are wont to imagine, single and alone, the sole and only representative of our race. "The head of the woman is the man;" "neither was the man created for the woman, but the woman for the man." In the reproduction of the species, accordingly, it might *à priori* be expected that the male parent should impart of his own qualities precisely those which are characteristic, as well of the animal in general as of the species to which he belongs; that is, his bones and muscles, his brain and nerves, and it may be added, his own mental peculiarities.

And that the female should furnish the vital organs and give tone and character to the vital functions,—the feeding and fattening powers of the offspring, its secreting powers (*e. g.* in respect of milk), and likewise its general quality in so far as influenced or determined by the nutrient powers, we may reasonably enough suppose. Such a notion would be in keeping with what is obviously the subordinate character of these functions, subordinate not certainly in the eye of the breeder, but in relation to the order of nature in the animal economy. Moreover, it would harmonize with what is palpably the part assigned to the female in the rearing and tending of her offspring.

Thirdly, We read in Scripture of *Levi*, yet unbegotten, being "*in the loins*" of his father. We speak of a father being *reproduced* in his son. And it is the boast of some of our nobility and old families, that they have passed through a long course of ages, in the *direct male* line.

Formerly, when the whole influence exerted by the male in the act of reproduction was supposed to be nothing more than a mere "*vivifying*" of the female ovum; when the whole virtue of the seminal fluid was thought to be a mere "*aura*," the ideas in question had no proper value. The female was presumed to bear the chief part and the essential part in the production of the offspring. In the facts before us, however, those ideas have their full significancy given to them. The male not only takes a direct share in the production of his offspring, but actually contributes that part which constitutes his own proper self; stamping his offspring at the same time with his own proper image and superscription.

What gives special value to the notion of descent in the male line is, that though the daughter equally with the son, represents their common father, her children do not. Her brother's children do. But her's must be looked upon as representing mainly her husband's house and line, whose name accordingly they bear. Yet in "leaving" her father's house, and "joining" herself to another, while she is but following the order and appointment of nature, she has her reward. Not only are her interests identified with her husband's, and her affections transferred to him, but she becomes "bone of his bone, and flesh of his flesh,"—the two being "no more twain but one flesh."

And what if these expressions have a deeper meaning than at first appears? What if they be physiologically as well as figuratively true? For, according to Mr M'Gillavray,[1] through the offspring, and by them while *in utero*, the woman comes to be *inoculated* with those of her husband's qualities that are inherent in them, and were imparted to them in the act of impregnation,—so inoculated, that is to say, with those qualities as to be able herself to impart them to offspring she may subsequently have by another husband; while, according to Mr Orton, the qualities which the husband thus imparts, are his locomotive, *i. e.* his *bones* and his *flesh*. And to carry this speculation only one step further,—what if, as unquestionably he can to his offspring, what if the husband can thus and through them, impart to his better-self of his own *mental* attributes, and thus there be engendered a blending of heart and mind, in a sense altogether special and in a way otherwise impossible. It is a common observation at least, that husband and wife come to resemble each other not only in face and feature, but in sentiment and feeling.

As for these speculations, however, *valeant quantum valeant*. If they cannot be received as established truths, they may, peradventure, be taken as "guesses at truth," bearing in mind as we ever ought, that "there are more things in heaven and earth" than either our microscopes, or our balances, or our re-agents can give account of; or even than is yet "dreamt of in our philosophy."

ARTICLE III.—*Reports and Observations in Surgical Practice.* By E. R. BICKERSTETH, Surgeon, Liverpool.

THE term neuroma is used generally to signify a tumour originating in or upon a nerve; but, like many other terms in medical nomenclature, the use of the word has exceeded its proper limitation. Thus it is not uncommonly applied to express the character of any tumour in connection with a peculiar train of nervous symptoms, although such tumour has nothing whatever to do with the nerves or their ramifications. For instance, the "painful subcutaneous tubercle" is commonly called a neuroma, whereas it has been plainly demonstrated by repeated dissections and microscopic examinations, that in the great majority of cases no nerve fibrils can be found, either in or upon the tumour. A neuroma, properly so called, is always developed on the interior of the nerve sheath, in the substance of the nerve itself, or on the extremity of a divided nerve. It consists of a fibrous or a fibro-cellular structure, which, by its gradual increase, stretches and separates in a greater or less degree

[1] See papers by the author, in the "Monthly Journal of Medical Science" for Oct. 1849, and September 1850, "On the Fœtus in Utero, as inoculating the maternal with the peculiarities of the paternal organism."

the fibrils of which the nerve is composed. Sometimes one or more cysts are found in their interior, and a few cases are on record where the expansion was entirely due to a simple serous cyst formed in the midst of the nerve. The latter, however, cannot be correctly called a neuromatous tumour.

Neuromatous growths may be either single or multiple. Two remarkable cases, where nearly every nerve in the body was affected with these tumours, are recorded by Dr Robert Smith.[1] They may be either painful or give rise to no inconvenience. A very interesting fact regarding them is, that sometimes they continue for many months or years without pain, and then suddenly become the seat of the most agonising tortures; while at other times, but more rarely, a painful neuroma loses its peculiar sensibility. They may affect all the nerves in common, but are more frequently seated on those of the extremities than elsewhere.

Tumours of this nature never disappear spontaneously. If the seat of severe pain, and not more than one exists, their removal by the knife is necessary, for there is no other means of relieving the terrible suffering they almost always occasion. The following case is a good illustration of the character and severity of the pain to which they give rise.

Neuroma of the Great Sciatic Nerve; Excision, without Division of the Nerve; Complete Recovery.

Alfred King, æt. 9 years, a delicate looking and extremely emaciated boy, was brought to me about the middle of January 1853, suffering from excessive and constant pain, of a peculiar character, in the left leg and foot, and from a tumour on the posterior part of the thigh, about three inches above the centre of the popliteal space.

His mother gave me the following history: About twelve months before, the boy, who was always considered delicate, began to complain of frequent cramp in the sole of the left foot. It disabled him from walking, for whenever he attempted to extend the leg, so as to place the foot flat on the ground, it brought on violent pain. After a while the pain became more severe, and extended up the posterior part of the leg, as high as the knee. It troubled him night and day, coming on in sudden paroxysms of "violent cramping pain," lasting for a minute or more, and then ceasing with equal abruptness. It made him cry out sharply, and the only thing that gave the least relief was rubbing the leg firmly, or pressing the sole of the foot between his hands. About four months before he came under my care, his mother discovered a hard swelling at the back of the thigh, and she thinks it has been slowly increasing since. It was exceedingly sensitive, and when touched brought on the pain in the foot and leg. For the last three months the poor lad has had no rest. His mother states she is sure he has not slept five minutes together for many weeks. He has sat up in bed all night holding and pressing the foot with his hands, and crying out with sudden pain every few minutes. He has refused to take food, and has become so emaciated and worn out, as to be hardly able to support himself, without assistance, in the sitting posture. The knee joint has gradually become flexed, and now he is unable to extend it beyond a right angle.

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[1] Pathology.—Diagnosis and Treatment of Neuroma. By Robt. Smith, M.D., Dublin. 1849.

On examination the tumour was found so excessively sensitive that it was not possible to ascertain its exact characters. It formed a slight prominence in the middle of the back of the thigh, and appeared to be deeply lodged between the external and internal flexors of the leg, just at the part where they begin to separate to form the upper boundaries of the popliteal space.

The peculiar character of the pain, the situation of the tumour, and the history, confirmed me in the impression that the case was one of neuroma. Several applications were tried in the hope of affording relief, but in vain. The friends were earnestly desirous that the tumour should be removed, and at their request I undertook the operation, having first fully explained the possible consequences.

Feb. 8, 1853.—The boy being under the influence of chloroform, I made an incision over the surface of the tumour, from about four inches below the fold of the nates to the middle of the popliteal space. Having divided the fascia to the same extent, the tumour was immediately brought into view, and a little dissection at its upper and lower borders plainly revealed the great sciatic nerve expanding and enveloping the tumour, and passing off again at its lower part, in the form of two distinct nerves, separate from each other about three-fourths of an inch, and gradually diverging towards the inner and outer margins of the popliteal space. The tumour was the size and shape of a hen's egg; its surface was rather irregular, being slightly lobulated and fissured in the longitudinal direction by the passage of the nerve fibrils, which lay along the grooves. They were very sparingly scattered over its posterior surface, but laterally were more abundant. I made an incision through the capsule of the tumour, and then, partly with a blunt hook, and partly with a bistoury, I carefully reflected the nerve fibrils, together with their connecting membrane, which constituted also the capsule of the tumour. In this manner the tumour was very easily turned out from its bed. A good deal of troublesome bleeding from minute vessels in the interior of the nerve followed this proceeding, and after other means of checking it had been ineffectually tried, it was found necessary to place a ligature on three or four points. In doing so the greatest care was taken not to include any of the nerve structure. Several stitches of the interrupted suture were introduced, and the wound dressed with the water dressing.

The boy was quite comfortable a couple of hours after the operation, and slept better the next night than for many weeks before. Sensation was perfect in the limb. It retained its natural heat, and he could move the foot and toes. The day following, *Feb. 9th*, the skin looked red, and the thigh was slightly swelled, so that it was necessary to remove the sutures and put on a poultice. The inflammation increased, the foot and leg became anasarcous, and after a few days, diffuse erysipelatous inflammation attacked the whole extremity, and spread over the scrotum and lower part of the back. Extensive incisions through the scrotum and upon the front of the leg and foot were required, for the parts threatened to become gangrenous. As it was, a part of the scrotum sloughed away. The constitutional symptoms were very severe, and for several days I almost despaired of his life. Under the free use of wine and bark, with frequent small doses of morphia, he slowly rallied, and the inflammation subsided, but his recovery was greatly retarded by several relapses, and the formation of abscesses about the leg and foot. It was between three and four months after the operation before I could consider him cured, and he was able to walk about without lameness. Since then, and up to the present time, he has continued well; he has had no return of the pain, and can run about as well as any other child.

The tumour on examination proved to be formed of fibro-cellular structure. It was of a pale grey colour, and rather soft and friable, so that it easily broke when bent or squeezed between the fingers. Its consistence was uniform, and it had no appearance of degeneration.

Three different methods have been adopted for the removal of

this disease: they are amputation of the limb, excision of the tumour with the nerve, and excision without removal of the nerve. The two first are sufficiently simple and satisfactory in their results. The last is always dangerous, and has frequently proved fatal from the intense inflammation which is set up in the nerve, and the parts which it supplies. It has been repeatedly shown by operation that an inch or more of the main trunk of a nerve may be cut out, and yet only temporary paralysis be the consequence. In a marvellously short time reunion takes place, and the paralysis is recovered from. This is more remarkable in the lower animals than in man. In the horse, for instance, there is an operation still practised for the cure of lameness, which consists in the excision of three inches of the nerve supplying the foot with sensation. It cures for a time, but in a very few months the animal goes lame again, and when it is killed the nerve is found complete, as if no part of it had been removed. In the case which I have narrated above, it would have required at least three inches of the nerve to have been excised, including the commencement of both the internal and external popliteal nerves, and this I was afraid to do, lest permanent paralysis of the leg and foot should result—a condition less desirable than loss of the limb by amputation. I determined, therefore, under these circumstances, to run the risk of leaving the nerve entire, and the result has fortunately proved satisfactory, although, I confess, the proceeding was accompanied with great danger.

Should I again meet with a similar case, I think I should adopt the same plan, but should afterwards take the precaution of cutting across the trunk of the nerve, either above or below the tumour; by doing so, I am inclined to think the danger of this operation would be greatly lessened, for the limb having ceased to receive its nervous supply through the injured trunk, inflammation of it might afterwards take place with comparative impunity, and little or no risk be run of the paralysis continuing for more than a short period.

The diagnosis of neuroma is generally simple enough; the excessive sensibility of the tumour, the peculiar character of the pain and cramp in those parts supplied by the nerve affected, and the situation of the enlargement, are alone sufficient to render the nature of the complaint pretty evident. If to these symptoms is added that the tumour is freely moveable in a lateral direction, but not so in the direction of the long axis of the nerve, the existence of neuroma will be in the highest degree probable. With every care, however, it is sometimes impossible to establish beforehand a correct diagnosis; but this is of the less consequence, as the same treatment is required, whatever be the nature of the tumour, if it is found to give rise to painful sensations. The case following affords an example of this, and is, besides, interesting in a pathological point of view.

Painful Tumour over the Head of the Radius, suspected to be Neuroma; Excision; Cure.

Miss A. H., æt. 23, a governess, came under my care in the beginning of December 1853, complaining of severe pain in the left elbow, and inability to extend the joint. She stated that two years before she was much engaged in knitting, and then first experienced a kind of aching pain in the elbow, and difficulty in extending the arm fully. At that time she was residing in one of the back States of America, and adopted the country mode of crossing gates, etc., *i.e.*, by vaulting over them, but soon this gave her so much pain that she was obliged to take a more decorous method of passing from one field to another. About nine months afterwards, the pain became constant, but was much increased when anything touched the outer side of the joint, or when she tried to straighten the arm. She then discovered a small lump on the external side of the joint, and found that when this was pressed the pain shot down the arm into the fingers. She described it as a dull, constant aching pain, always fixed over this spot, and frequently extending down the back of the forearm to between the thumb and first finger, and sometimes along the fore and middle fingers. At times it was very severe, and prevented her altogether from using the hand. She was unable to extend the arm beyond a right angle, and even this was painful, so that when she slept she kept it flexed as much as possible, placing the hand of the affecting limb over the opposite shoulder. When once asleep she slept soundly, but was often kept awake many hours from occasional starting of the arm before she could place it in a comfortable position. The position in which she held the hand was also remarkable; for, in order to extend the fingers sufficiently for useful purposes, as sewing, etc., she was obliged to keep the wrist bent backwards to its utmost. When she straightened the wrist, she could not open the fingers.

On examination of the elbow, there was nothing unusual to be seen. There was no swelling. Extension to the limited degree mentioned was effected without pain, and she could rotate the forearm freely. Pressure over the head of the radius caused so much suffering that it was impossible to ascertain the exact condition of the parts. It felt thickened and enlarged. Supposing that some periosteal inflammation existed, I prescribed blistering and iodine of potassium. She had previously been under several medical men, and had tried all sorts of local applications, including chloroform liniment, and belladonna, but without any relief. The house surgeon at one of the hospitals said he could feel a tumour, and introduced her to his superior, who dismissed her with the recommendation that "she should forget she had an arm, as it was only a nervous affection."

As no benefit followed the treatment I had recommended, I felt the importance of making a more thorough examination, and accordingly gave her chloroform. When she was completely insensible to pain, I was able to feel distinctly a hard tumour about the size of a horse bean, deeply embedded in the muscular mass immediately in front of, and close upon the head of the radius, and I was able to move it about in such a way as to convince me it had no connection, although in close proximity, with the bone. I also found that during insensibility I could extend the forearm nearly to the straight position, and that the joint was in a perfectly healthy condition.

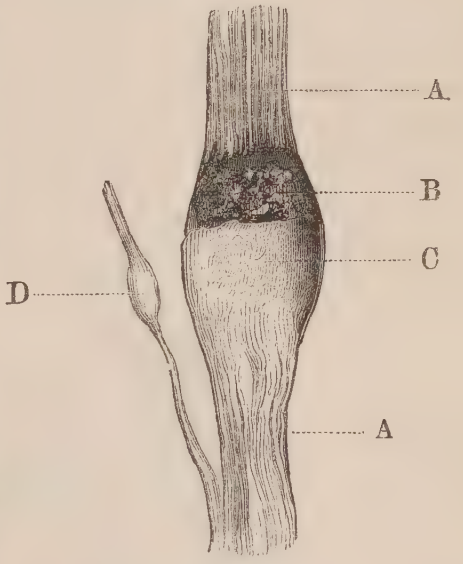
It was at once determined to remove the tumour, and the patient thankfully assenting, I performed the operation in Erskine Street Hospital, on the 21st of December. She was placed under the influence of chloroform, and I made an incision about three inches long in a curved direction over the front of the head of the radius. Having divided the fascia to the same extent, I cut through the muscles in the direction of their fibres till I came down upon the tumour. When fully exposed it proved to be of an oval shape of the size I had previously supposed. Its lower end was continuous with what looked like a large nerve, but which was afterwards found to be a process of tendinous structure. At its upper end, and between it and a similar tendinous process was a small vacuity

filled with blood, produced, I have no doubt, by the rupture of the tumour from its superior attachment during the extension of the arm in the examination a few days before. The whole was carefully removed, and the wound brought together by sutures, and dressed with water dressing.

Immediately after the operation she felt relief, and found she could put the arm in any position excepting perfectly straight without pain. She slept well. The wound healed kindly; and on the tenth day after the operation she left the hospital cured. About four months afterwards she again consulted me, stating that if she used the arm much in playing on the pianoforte she felt an aching pain in the old place, and was afraid it was coming again. Her general health was not good, and she was evidently very nervous about herself. A grain of quinine three times a day quickly removed the pain, and the disagreeable impression about the return of the complaint.

On examining carefully the substance removed, and taking away the muscular and tendinous structures that adhered to, and surrounded, the lateral parts of the tumour, I found it did not possess any investing capsule, but was everywhere intimately attached to the neighbouring textures.

The accompanying wood-cut represents the tumour seen in section A.A. Tendinous structure attached to each extremity of the tumour. B. Vacuity at upper extremity of tumour filled with clotted blood. C. The tumour itself in which the white tendinous fibres can be traced for a short distance at both ends. D. A small detached portion which was on the surface of the tumour, and was separated from it by dissection. This is seen entire, no section being made.



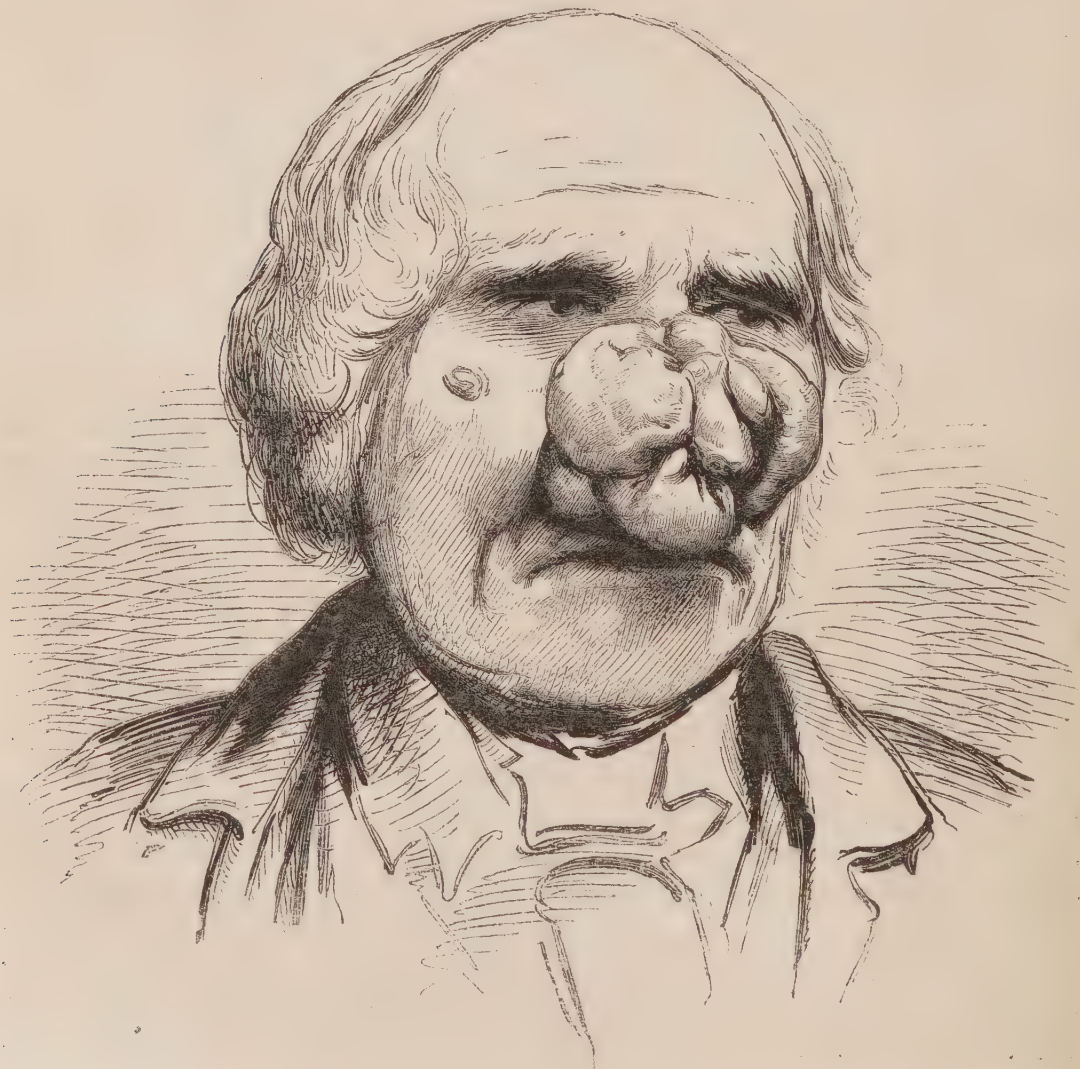
On making a section lengthways through the tumour, together with the musculo-tendinous structure incorporated with its two extremities, it was evident that the growth had formed between the ruptured ends of some tendinous fibres, or else had arisen in this texture, disintegrating and destroying its structure to the extent occupied by the tumour. I could trace the white tendinous fibres entering each extremity of the tumour gradually becoming thinner and more delicate, and then abruptly ceasing. The tumour itself was of firm fibrous consistence. Its surface was irregular, being minutely lobulated. Its section presented a well marked concavity of partly white and partly rose-coloured aspect. Microscopically, it consisted throughout of fibro-nucleated tissue. No trace of nerve structure was found though carefully sought for.

The symptoms in this case were identical in every respect with those of neuroma, and the physical examination of the arm rather confirmed than dispelled this diagnosis, yet the actual dissection, after removal, clearly showed the tumour to have no connection with any nerve. To what pathological condition the tumour had its origin I do not venture to say, never having heard of or met with a case presenting similar appearances on examination. On the whole, although there is no positive history of mechanical injury, I am inclined to suppose that in "vaulting," or some other muscular exercise, the tendinous fibres of one of the muscles had been ruptured, and

that in the attempt to repair the injury, a diseased or perverted action had given rise to the formation of the tumour.

Cutaneous Hypertrophy of the Nose; Removal; Cure.

One of these shocking cases of deformity came under my care in June 1853. The subject was a fine hale man, 68 years of age, who said his nose had been the ruin of him, and begged I would "cut it all off together, for he could stand it no longer." It was certainly of a very large size, covering a considerable portion of both cheeks, and hanging over his mouth, so that he could not eat or drink without first lifting it out of the way with his hand. The poor fellow had formerly been in a merchant's office, but was dismissed nearly twelve years ago, as his nose was then beginning to assume a disreputable appearance. Having saved some money, he opened a public-house, trusting his customers would be less fastidious than his late employers, and for a few years his affairs prospered; but at length, as his nose got larger and larger, his shop was gradually deserted, and he was obliged to give it up. Although a very sober man, he was regarded as a warning of the consequences of free drinking, and his oldest customers preferred to spend their time elsewhere. After this he tried to get all sorts of employment, but without success. One look at him was enough. His nose condemned him. The children cried after him wherever he went. Broken-hearted and disgusted, he latterly remained altogether indoors, existing on the charity of his neighbours. The day before I saw him



he had ventured out a short distance from home, but was "hooted at" by the children, and was obliged to retreat. In a fit of desperation, he then "determined to have his nose cut off, even if it should cost him his life."

The tumour consisted of a blue-coloured mass as large as a closed fist, growing from the point and both alæ of the nose. It was divided into three principal lobes, and each consisted of numerous smaller lobules, which were rough and marked with deep pits and eminences, the orifices of subaceous ducts.

He was admitted to the Erskine Street Hospital, and on the 15th of June I cut the whole of the tumour off with a bistoury, taking care, with my fingers in the nostrils, to go as near as possible to them without wounding the cartilages of the nose. Afterwards I clipped off with a pair of curved scissors a few projections that remained, and made both sides as symmetrical as possible. The bleeding was rather considerable at the time, but soon stopped under the influence of cold and pressure. Three or four of the larger vessels were secured by ligature.

In the course of three or four days suppuration was established, and, the dressings being removed, zinc lotion was applied over the raw surface. Cicatrization quickly took place, and in a fortnight he left the hospital with wonderfully little deformity.

A year has now elapsed, and there is no appearance of a return of the disease. The nose is perfect in its shape, and the new cuticle covering it is quite as smooth as the skin on any part of the face. But for previous knowledge, it would be difficult to suppose anything had been the matter with his nose. With the loss of the deformity, the man has regained his character, and has now no trouble in obtaining constant occupation.

ARTICLE IV.—*Cases of Cholera; illustrative of the Presence of Fat in the Fæces, and of certain Alterations of the Blood.* By W. LAUDER LINDSAY, M.D., Assistant Physician, Crichton Royal Institution, Dumfries.

THE papers of Drs Reeves and Cowan, in the March number of the Journal, on "The Presence of Fat in the Excretions," and on "Certain Alterations of the Blood Corpuscles in Cholera," respectively, induce me to believe that the narrative of the four following cases may not be altogether devoid of interest, as a small contribution to the natural history of cholera. In all these cases fatty or oily matters existed to a notable extent in some of the fluid fæces of the fever stage and convalescence; in one, *post-mortem* examination revealed a morbid structural alteration of the blood corpuscles; while all serve to illustrate various of the more interesting features connected with the pathology and histology of the disease. The cases referred to occurred in the Surgeon Square Cholera Hospital of Edinburgh, while I was resident physician thereof, during the months of October, November, and December 1853. It would occupy too much space to enter upon the details of the history of the progress, etc., of all, or any, of these cases systematically or *ad longum;* I content myself with giving a very brief sketch of their general features, drawn from full notes, taken at the bedside, in my private journal.

CASE I.—M. F., a woman æt. 40. Admitted in *collapse* 11½ hours after seizure; *fatal* in the "*consecutive fever*" [or typhoid stage] on the third day. She was healthy, sober, and industrious, but lived in a filthy, crowded close in one of the worst parts of the Old Town. She could assign no reason for her seizure; but it was ascertained that a few days previously she had washed the clothes of a person who had died after two days' illness [of cholera?]

Her condition, noted on admission, was as follows:—

In extreme collapse. Unable to maintain a sitting posture on a low stool before the ward fire, while a bath was being prepared; sank helpless and prostrate on the floor. Voice low, whispering, almost amounting to aphonia; when roused, she spoke huskily and roughly, as if she laboured under laryngitis. Face pale; features pinched; expression haggard; eyes deeply sunk in their orbit. Slight conjunctival injection surrounding corneæ. Lips partially livid. Hands of a dingy purple colour. Pulseless. Body quite cold. Vomited immediately on admission ℥iv. of a whey-like fluid, resembling the thinner "rice water" stools: it was not ejected in a forcible gush, nor was the vomiting induced by drinking water or other fluids.

On the afternoon of the same day, she complained of dyspnœa, sensation of choking, faintness, constriction of the throat, general *malaise*, and insatiable thirst; the respiration became laborious; the pulse gradually became perceptible, and rose to 100, and the body acquired a comfortable degree of heat. The vomiting continued for a few hours only, and the cramps were very slight and confined to the calves of the legs, feet, and intercostal muscles of the left side. She was frequently in a dozing or apathetic condition, from which she awoke with a sudden yell of agony from cramp, or from a feeling of impending suffocation. There was great mental despondency. She showed a decided disinclination to speak; asked questions in whispers, and answered them by a shake of the head. Her body exhaled a most peculiar [choleraic] odour, which was at once detectable in any part of the ward.

On the 2*d day*, *reaction* was passing into distinct *fever*, judging from the condition of the tongue, pulse, and skin. She still complained of an overpowering feeling of suffocation, and of intense heat, to mitigate which, she requested the removal of a large portion of her bed-clothes.

On the 3*d day*, the typhoid stage was well marked by hiccup; slow and difficult protrusion of a dry, brown tongue; great debility; pinching of the features, and laborious respiration. Her stool was chocolate-coloured and fœtid: on allowing it to stand a few hours, there appeared on the surface of the fluid portion a number of masses of *fat*, resembling pieces of tallow or suet, and varying in size from that of a pea to that of an almond. About a quarter of an hour before death—she having passed no urine since admission, and being quite unconscious of, and passive under, my interference—I drew off ℥iss urine by the catheter. The tracheal râle was distinct, and fine and coarse moist râles were prevalent throughout the chest for some time prior to dissolution. After death convulsive tremors of the jaws, throat, and hands, were noticed.

Chemistry and Histology of the Evacuations.—1. *Stools.*—Those first passed after admission, had the usual characters of the "rice water" discharges, were alkaline, albuminous, and of sp. gr. 1010-12. On most of them a few phosphates appeared, among an oily scum, on the surface, on allowing them to stand for a night; in one, the scum contained a quantity of globular urates (the origin of which had probably been some accidentally intermixed urine). All gave a *biliary* reaction with heat and nitric acid; that is to say—in some, a pink; in others, a reddish, brownish, or greenish-yellow colour was developed: the three former colours being readily convertible, by prolongation of heat and excess of acid, into the latter.[1]

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[1] For a statement of my views on the presence of bile in the cholera evacuations, *vide* "Clinical Notes on Cholera," in the *Association Medical Journal* for March 10th, 1854; and for a detailed account of the chemistry and histo-

Those passed during the fever stage on the 3d day, were all chocolate-coloured, or resembled the "*café au lait*" of the French,—alkaline, albuminous, sp. gr. 1010-12, and very fœtid. In the earlier stools the sediment preponderated; in the later, it amounted to one-third of their bulk. On one, a phosphatic scum speedily formed on the surface; while, on another, occurred the almond-shaped masses of *fat* already mentioned. Under the microscope, these exhibited the appearance of fat vesicles, each containing a radiating crystalline mass of margarine, and the interspaces being filled with oil globules.

2. *Vomit.*—The fluid first vomited resembled the ordinary "rice water" stools in chemical and histological characters. The scanty flocculent sediment had the usual characters of cholera-mucus under the microscope, viz., the appearance of hyaline, finely striated bands, entangling a number of granular corpuscles, which at one time resembled pus corpuscles, and at other times white-blood, lymph, and mucus corpuscles, or free epithelial nuclei; there was, moreover, a quantity of pavement epithelium, perfect or atrophied, a few "annular bodies" (so-called) [which are, generally speaking, merely vegetable cells disintegrated or isolated from the tissue of some of the substances consumed as food],—starch grains and compound granular bodies. The fluid had a pale straw colour, sp. gr. 1008, was alkaline, highly albuminous, and gave a biliary reaction (greenish-yellow) with heat and nitric acid. It had a strong smell of whisky, a glass of which she acknowledged to have swallowed before admission.

The later vomits were of a dark brown colour, resembling ale or porter,—alkaline, albuminous; the sediment, which was scanty, contained epithelium scales, more perfect and larger than in the sediment of the earlier vomits, besides starch granules, "annular bodies," and molecular debris.

3. *Urine* removed before death, by catheter, was alkaline, of a deep amber colour, albuminous; had a slight flocculent sediment, consisting entirely of pavement epithelium; struck a blood red colour with heat and nitric acid; gave no evidence of urea, but showed great effervescence on the addition of acid.

Pathology.—Sectio two days after death.—Body that of a stout, muscular woman in the prime of life; good covering of subcutaneous fat; muscles of good colour, not notably dry; livor of various parts of body considerable; livid mottling of arms; incipient decomposition of abdominal contents; corneæ hazy; no injection nor ecchymosis of conjunctivæ.

Pericardium contained 5i. of tolerably clear serum; marked by ecchymosis, both punctiform, and in small irregular patches of a dark venous colour.

Heart weighed 11 oz.; dilated, flaccid; muscular substance of dull brownish tint. The posterior surface was covered by ecchymosis of a dark purple colour, punctiform and in patches. Punctuate ecchymosis also occurred on the right auricle, and, to a very slight extent, on the right ventricle; it covered the origin of the great vessels posteriorly. *Left* ventricle firmly contracted; contained little or no blood. *Right* ventricle and large vessels were gorged with blood, which was dark, grumous, of a dull colour, and showed scarcely any tendency to coagulation; valves normal, with the exception of the mitral, which was slightly thickened at its free edge.

Blood.—The red corpuscles, under the microscope, were not recognizable as such, but were granular, and in their general aspect, both before and after the addition of water or acetic acid, closely resembled pus.

Lungs weighed 2 lbs. 4 oz. The *pleuræ* were studded over with small ecchymotic patches of a dark tint similar to those of the pericardium and heart; they were also well marked on its diaphragmatic surface. *Lungs* emphysematous anteriorly and superiorly, with intervening portions of collapsed tissue, giving an irregular character to their free edge. Posteriorly and inferiorly the tissue was non-crepitant, congested; exuded, on pressure, a copious, dark, bloody juice;

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logy of the cholera evacuations, the pathology of the disease, and various other subjects connected with its natural history, *vide* the same Journal for April 14, 21, *et seq.*

tough and resembled "carnified" lung; this was most marked on the left side, where the finger could scarcely penetrate or lacerate the pulmonary tissue; greater part sank in water. *Bronchi* congested, full of rusty coloured mucus. The juice already referred to, under the microscope, consisted almost wholly of *altered blood corpuscles*, with an intermixture of cylindrical ciliated epithelium and compound granular bodies. Some of the blood corpuscles resembled pus; others free epithelial nuclei; in some, again, a delicate cell wall surrounded a well marked single or double nucleus, while in others the cell wall was developed in such a direction round the nucleus as to give a caudate appearance to the cell; a few were irregular in shape, and more resembled the bodies usually figured as "tubercle corpuscles." Water rendered these characters more distinct, but the subsequent addition of acetic acid caused little or no change in their essential features.

Liver weighed 3 lb. 4 oz.; of large size, much congested; epithelium slightly fatty. *Gall-bladder* distended. *Bile* natural in colour and consistence; gave ordinary reaction with heat and nitric acid. In the bile, which had been removed from the gall-bladder, and allowed to stand a night, a small *gall stone*, about the size of a boy's marble, was found. Surface irregular, tuberculated; felt soapy; on section, had the appearance of a crystalline mass radiating from a centre; nucleus was of a bright orange red, friable, granular, amorphous: surrounding this was a zone or circle of a brownish-red (biliary resin), while the remainder or circumferential zone was of a bright orange-yellow. Its basis was *cholestrine*.

Kidneys weighed 10 oz.; were small in size, and irregular in shape; congested; surface was marked by numerous small depressions resembling the cicatrices of minute cysts; stellate vascularity well marked. Cortical substance cream-coloured; striæ obscure; no distinct "granulations of Bright." Epithelium slightly fatty.

Spleen weighed 3 oz.; small, congested, and of dark purple colour.

Stomach.—Rugæ very prominent, mucous membrane soft and pulpy (from the effects, partly or wholly, of post-mortem maceration?); of a brownish-green colour (result of incipient post-mortem decomposition?); surface marked by ecchymosis, punctiform and in patches, of a much brighter red than in other parts of the body.

Duodenum, *Jejunum*, and greater part of *Ileum* normal in appearance; lined with a gelatinous mucus, which, however, was not "ricy" nor flocculent in its character. Valvulæ conniventes of duodenum deeply stained orange by bile. There was a very slight amount of glutinous matter on the peritoneal surface of the small intestines, which was also pale in colour; but interiorly several considerable patches of roseate hyperæmia were visible. The lower part of the ileum was the seat of a considerable amount of ecchymosis. Peyer's patches distinct, but there was no surrounding congestion nor ecchymosis. No "psorenterie," but the mucous membrane was rough and granular. Whole gut was somewhat distended with gas (result of post-mortem changes?)

Cœcum and ascending *Colon* normal in appearance. In the interior of the transverse and descending colon, there were several patches of thickened mucous membrane of a greenish colour, apparently in process of sloughing; they had thick inverted edges, there was no surrounding congestion, and their base was only partially marked by punctuate ecchymosis; they resembled patches of dysenteric lymph. When the process of separation was less advanced, the edges were only slightly raised above the surrounding membrane, and the surface of the patch was dotted over with hæmorrhagic spots; at other points a line of demarcation was just beginning to form, the portion of mucous membrane about to be thrown off having merely a brownish-green colour to distinguish it. Some of these patches extended nearly round the gut, and were several inches broad; the larger ones were very irregular in shape, being usually ragged in their outline; the smaller were circular or oval, and had a more entire margin. Disintegrated or lacerated portions of these were found in the canal, and resembled

the mucous flocculi of the "rice-water" stools; though to the naked eye, undistinguishable from lymph, they had all the microscopic characters of exuviated mucous membrane, consisting chiefly of nude intestinal villi, cylindrical epithelium cells,—isolated and in patches, connected at their apices by a delicate membrane,—and mucus, entangling a few prismatic phosphates.

Rectum.—Greater part of mucous membrane had begun to soften and separate, and there was only a narrow ribbon of healthy tissue running down the interior of the free border of the gut.

Uterus.—There was a good deal of hæmorrhagic extravasation in the broad ligament and right ovary, which were small and flaccid. Left ovary exhibited a well-marked corpus luteum, with a hæmorrhagic centre.

Brain weighed 2 lb. 10 oz.; calvarium and membranes congested; sinuses of dura mater very turgid; ventricles contained 3i. serum; choroid plexus not notably congested. Cerebral matter healthy in appearance.

Bladder thickened, contracted, and corrugated; quite empty; no ecchymosis.

Congestion of all the abdominal viscera was chiefly venous.

Contents of *small* intestines resembled pea-soup in colour and consistence; were fœtid, sp. gr. 1012-15, alkaline, albuminous; sediment amounted to five-sixths of the bulk. The fluid removed from the *large* intestines had a slight bloody tinge, sp. gr. 1010; was also alkaline, albuminous, and fœtid; sediment amounted to four-sevenths of its bulk. Under the microscope this fluid matter consisted chiefly of cylindrical epithelium; in very small quantity in the small, and in much smaller amount than in deaths during the collapse stage in the large (in great measure due to post-mortem changes?); some of the cells were deeply stained with bile; there were, besides, a few nude villi, fragments of vegetable tissues (debris of food), and a quantity of dark granular matter.

Case II.—A. W., a girl, æt. 7½. Admitted in advanced, almost hopeless, *collapse*, passed through all the stages of the disease, *recovered*, and was discharged well in three weeks. She was a healthy, lively, active child, but poorly fed and clad. Her father's dwelling was crowded, dirty, small, and ill-ventilated. In the court in which it was situated several other cases of cholera occurred about the same time, the principal of which were:—

1. Her father, who was first attacked, and *recovered* after a mild, but protracted, illness, of seventeen days.
2. Her mother, who was attacked two days subsequently, and also *recovered*. These two cases are immediately hereafter referred to.
3. A younger sister, who *died*, after a short and severe attack.
4. A playmate, a girl of 6¼, was attacked with premonitory symptoms, but recovered.
5. A younger brother and sister had suspicious diarrhœa, but recovered.

Within an hour after the seizure, she presented all the ordinary features of the collapse of cholera, with the exception of cramps, which were absent throughout her illness.

On admission she was in such a state of collapse, and with symptoms so like those which were fatal in her sister's case, that her parents had given up all hope:—and certainly her condition did not lead to a favourable prognosis. Pulse 130, scarcely perceptible; gradually rose to 160; eyes deeply sunk; surrounding areola distinct and dark; tongue moist, cold; breath cool; body very cold, pale; hands of a dull purple; apathetic and drowsy, peevish and irritable.

In the afternoon she became very restless, at times incoherent or delirious, made repeated attempts to get out of bed, and urgent demands for cold water; the respiration became laborious, the pulse so rapid and small that it could not be counted, and the *vox cholerica* was well marked. The heart sometimes beat upwards of 200 times per minute. She lapsed into a drowsy condition, from which she frequently awoke with screams of distress or alarm.

2*d day*. The collapse appearances were fast disappearing, and giving place to those more especially characteristic of the typhoid stage.

3*d day*. She passed urine for the first time since admission, after the use of the warm hip bath. Diarrhœa had quite ceased; there was now a tendency to constipation.

4*th day*. She was in a lethargic, semi-comatose condition, which was frequently interrupted by delirium and restlessness.

5*th day*. She passed urine spontaneously. Fever still marked by delirium and restlessness.

6*th day*. More serious febrile symptoms had abated. In the evening she expressed a desire for food.

8*th day*. A smart febrile attack preceded the appearance of the so-called "cholera exanthem," which was, in this case, an urticario-rubeoloid eruption, well marked on the hips, face, and hands, and more sparingly distributed over the chest, back, abdomen, and legs. Its predominant character was *urticarioid*.

9*th day*. The eruption was found to have extended over the chest, back, and abdomen. The larger circular or crescentic patches which, on the previous day, were slightly elevated and pale, were now flat, of a dull purple colour, and surrounded by a distinct scarlet zone, separated by a circle of pale skin. The predominant character was now *rubeoloid*.

11*th day*. Convalescence was fully established; the eruption was fast fading, and the evacuations were comparatively healthy.

Chemistry and Histology of the Evacuations.—1. *Stools*.—1*st day*. A semi-fæculent stool, passed in the bath, contained a large quantity of certain forms of the so-called "annular bodies" or "cholera corpuscles;" under the microscope, the flocculi consisted of hyaline bands of mucus, covered by granular corpuscles, intermixed with a few phosphates, and a quantity of greenish-yellow granular débris. A stool passed shortly after was "ricy," but scanty.

The third and fourth stools were beginning to acquire a brownish-yellow colour, and, under the microscope, fragments of vegetable tissue (débris of the food) were observed.

5*th day*. Quantity ʒviij.; colour an intense greenish-yellow; sp. gr. 1015, alkaline, non-albuminous; under the microscope, appeared a large quantity of *oil* globules, several helicine or convolute cellular bodies, and a few very large mother cells, enclosing four or five smaller cells also of considerable size.

9*th day*. The stool was of pea-soup colour and consistence, fœtid, and contained several flocculent masses of dark fæculent matter. Floating on its surface were numbers of almond-shaped pieces of *fat*. These *fatty masses* were comparatively soft, and of a bright greenish-yellow colour, which were readily removable by immersion in water. Before being washed, they resembled the *appendices epiploicæ* of the intestines, or clusters of hydatids; after the biliary colouring matter was removed, they were like pieces of tallow. Under the microscope, nothing could be noticed but oil globules and a few compound granular bodies. They were converted into fluid oil on the application of heat: in aqua potassæ, aided by a gentle heat, they dissolved and formed a white opaque solution. Ammonia had a similar effect; acids had no effect.

12*th day*. Fluid portion of semi-fæculent stool was alkaline, non-albuminous; under the microscope, sediment contained potato cells and other fragments of vegetable tissue, a few phosphates, and compound granular bodies.

2. *Vomit*.—The sediment of the first matter vomited contained abundance of pavement epithelium, mostly atrophied and collapsed, or altered in form (elongated and fusiform), and at the same time dark and granular,—a few "annular" and fatty bodies, and a quantity of molecular débris.

The second vomit contained an enormous quantity of the same epithelium, loaded with dark granular matter and a few compound granular bodies.

The matter next vomited contained,—besides pavement epithelium, "annular" bodies and fragments of vegetable tissues,—a large quantity of *vibrios*.

3. *Urine*.—3*d day*. First urine passed was of sp. gr. 1015, very acid, turbid,

albuminous; struck, with heat and nitric acid, a deep orange-red colour. On standing, became loaded with urates. Under the microscope, the scanty sediment contained a few casts of the renal tubuli,—transparent, granular, and oily,—dumb-bell oxalates, free epithelial nuclei, "annular" bodies, similar to those occurring in the stools and vomit, and compound granular bodies. Contained little or no urea.

7*th day.* Urine acid, albuminous, sp. gr. 1010; had a sparing flocculo-granular whitish sediment. On the sides of the urine glass there was a considerable deposit of uric acid in large, striated, irregular, and dark-coloured crystals. The sediment consisted of a little mucus entangling numerous transparent or light-coloured, lozenge-shaped crystals of the same substance. Contained abundance of urea. On allowing the urine to stand for two days, the uric acid was wholly converted into dark globular masses of urate of ammonia.

8*th day.* Urine passed in morning acid, sp. gr. 1010, non-albuminous, sparing flocculo-granular sediment of mucus, entangling a few irregular crystals of uric acid.

That passed in the afternoon was acid, sp. gr. 1006, of a pale straw-colour, and had no sediment.

10*th day.* Urine sp. gr. 1014, acid, clear, scanty sediment, which still contained a few lozenge-shaped crystals of uric acid. A few irregular crystals of the same acid also adhered to the sides of the glass jar in which the urine was collected.

11*th day.* Urine sp. gr. 1012, acid, clear. Contained abundance of urea.

13*th day.* Urine sp. gr. 1012, neutral, non-albuminous; sediment contained a few uric acid crystals and pavement epithelium scales, chiefly atrophied and shrivelled.

The two following cases were members of the same family, being the mother and father of A. W. [Case II.] It is unnecessary to give in detail the history of the progress of these cases, inasmuch as they resemble, in essential features, the two already described :—

Case III.—I. W., a woman æt. 28. Admitted in *collapse*, *recovered*, and discharged well in three weeks.

State on Admission.—Collapse marked chiefly by incessant and severe retching and vomiting, which continued unremittingly for two days; the cramps were few and comparatively mild.

Progress of Case.—All the phases of cholera were well marked. On the 2*d day*, *reaction* was established, as was shown by the symptoms and stools. On the 3*d day*, *fever* set in. The case resembled her daughter's in its termination in recovery; the presence of the "cholera-exanthem" during the transition of the consecutive fever into convalescence; the character of the phases of the disease; its duration; and the histological and chemical features of the evacuations.

Chemistry and Histology of the Evacuations.—Stools.—1*st day.* 1st stool immediately after warm bath,—℥iv., "rice watery."

2*d day.* Several stools passed, about ℥v. each,—all "rice watery," alkaline, albuminous, and of sp. gr. 1015. The stools passed in the afternoon began to acquire a light brownish-yellow colour, and the sediment resembled oatmeal and water; sp. gr. 1010-15; they contained much ammonia.

3*d day.* In the morning, stool and urine passed in bed; the former was of pea-soup character. One stool in the afternoon was again "ricy" in its appearance, ℥ss., alkaline; sediment contained "annular" bodies, potato cells, fragments of epidermis, and portions of other tissues of plants, which had been consumed as food.

9*th day.* Castor-oil produced three copious evacuations, all dark brown, semi-fluid, and fœtid. On the last of these stools was found floating a number of flocculent masses of *fat*, of a bright greenish-yellowish colour, from the size of a pea

to that of an almond. Under the microscope, they consisted entirely of oil globules.

11*th day*. Stool dark brown, half-formed, very fœtid. On the surface of the fluid portion the same flocculent *fatty* masses occurred. The fluid was alkaline and albuminous. This phenomenon was co-existent with the presence of the "cholera-exanthem."

12*th day*. One stool of light brownish-yellow. No fatty masses on its surface.

14*th day*. *Convalescence*.—One copious semi-fluid stool, of a greenish-yellow colour, fœtid. On the surface of the fluid portion was a scum or layer of *oil*, having the same bright tint.

CASE IV.—J. W., a man, æt. 34. Admitted in mild *collapse*, *recovered*, and was discharged well in eighteen days.

Progress of Case.—On the afternoon of 1*st day*, *reaction* was fairly established, and thenceforth he progressed favourably. On 2*d day*, *fever* set in; it continued mild throughout, and exhibited very few and slight typhoid symptoms.

Chemistry and Histology of the Stools.—1*st day*. Quantity ʒvi., apparently mixed with urine; alkaline; somewhat fœtid; of pea-soup character. Sediment, under microscope, consisted of little else than a mass of molecular débris of a greenish-yellow colour; there were a very few phosphates and fragments of vegetable tissue.

10*th day*. Fæces of consistence of porridge; of a light brownish-yellow colour; fœtid; on the surface there was a scum of fluid *oil* of a yellow *colour*.

It thus appears that, in the four preceding cases, fatty matters were found in the fæces on seven different occasions. In five of these they occurred in a solid form, resembling pieces of tallow or suet; in two as a fluid oil. It is noteworthy that three of the patients were members of the same family. I have frequently found a greasy scum to collect on the surface of the collapse, as well as the fever and reaction, stools, on allowing these to stand for various periods. Under the microscope, this usually consists of oil globules, entangling a few prismatic phosphates. It often happens, moreover, where no appreciable scum or sediment exists, that the microscope points out the presence of abundance of minute oil globules. In the *vomit* of cholera, fatty bodies of various forms and oil globules are very common, evidently, however, having their origin from the food; and in the *urine* occasionally, and the *bile* very frequently, I have met with considerable quantities of oil globules. In several cases the hepatic epithelium has been loaded with oleo-albuminous granules; in a less proportion of cases the renal, and in a still smaller number the epithelium of various parts of the respiratory and digestive systems, has been rendered dark and granular from infiltration by the same material. Of the cause or source of the presence of these oily matters in the fæces, I shall not here speak; but it is right to mention that, in most, if not all, of these cases, castor-oil had been administered, one or more days previously, to obviate a tendency to constipation. That this substance, however, is not its true source is probable, because—

1. The fatty or oily matter did not appear in the *first* stools produced by the action of the drug.

2. The phenomenon occurred sometimes two or three days after the drug was administered, several stools devoid of any fatty intermixture having been passed in the interval.

3. It did not occur in many other patients, who laboured under precisely similar symptoms, and to whom castor-oil was given.

4. It was exhibited in three members of the same family.

5. It is improbable that castor-oil could be the origin of fatty matters having such opposite characters, viz., hard concrete fat, soft yellow fat, and liquid oil.

In Case I. it will be observed that the blood discs are noted to have been remarkably altered in appearance. I have met with a similar appearance, and with other changes of the blood in other cases of cholera. In one instance, the white corpuscles were present in marked excess, the red being normal in appearance; in some cases the red corpuscles seemed to have a strong tendency to adhere in rouleaux, in others the very reverse obtained; frequently they became hazy or granular.

Without at present making any comments, I would merely call attention, *en passant*, to the following points, which the foregoing cases also serve to illustrate:—

1. Albuminosity is not a distinctive feature of the *collapse* evacuations, as it occurs frequently also in the reaction and fever stools. I have found it occasionally in healthy fæces, and in the stools of typhus, phthisis, dysentery, etc.

2. Non-correspondence, in date or degree, of the comatose tendency of the "consecutive" fever with the suppression or re-establishment of the urinary secretion.

3. The distinctive histological and chemical features of the collapse, reaction and fever stools, vomit, and urine.

4. The following pathological phenomena in Case I.,
   - *a.* Distension of the gall-bladder and comparatively *healthy* condition of the bile. Presence of a *gall-stone.*
   - *b.* Thickening, softening, and exuviation of the mucous membrane of the large intestines.
   - *c.* The marked difference in the histological character of the post-mortem contents of the intestines and the stools passed during life.

5. The presence of the so-called "annular" bodies, vibrios, etc., is merely accidental and immaterial, so far as regards the causation of the disease.

6. The occurrence of the so-called "cholera-exanthem" in cases II. and III.

7. Insufficient police and parochial sanitary regulations a fertile predisposing cause of cholera, etc.

ARTICLE V.—*Report of some Cases of Articular Disease occurring in Mr Syme's Practice, illustrating the Advantages of the Actual Cautery.* By JOSEPH LISTER, M.B., London, F.R.C.S., Resident Surgeon to the Clinical Wards of the Royal Infirmary.

CASE I.—*Omalgia; Application of the Actual Cautery; Cure.*

MARGARET ASHTON, æt. 25, admitted October 25, 1853; a servant; has generally enjoyed good health, and has a very robust appearance. Four months ago, after exposure to wet and cold in washing, she had a severe fit of shivering, and was seized a few days after with pain in the right shoulder, just below the acromion, so severe that she could scarcely lift the arm; this lasted about twelve hours, and was followed in the course of the next day by intense pain in the left shoulder, below the back part of the acromion. From that day till her admission she was unable to raise the arm; the pain was for the first two months extreme, keeping her as if "in the fire all night," and banishing sleep almost entirely. During the last two months she has rested from work, and has suffered less. On admission she complained of constant gnawing pain in the left shoulder, and extending down the limb as far as the elbow, and sometimes to the fingers; when in the sitting posture she held the affected limb with the other hand, to ease the pain; the arm was also affected with a feeling of numbness and weakness; and although the shoulder was not very tender on pressure, and very gentle passive motion of the arm could be performed, through a considerable angle, without pain, yet any attempts on her own part to move it, produced great aggravation of her sufferings. As a result, no doubt, of habitual disuse, the muscles about the shoulder were much atrophied, and this caused a remarkable apparent prominence of the bony points, viz., the spine of the scapula, the acromion, the anterior border of the outer part of the clavicle, and the head of the humerus. The shoulder had an appearance that suggested at first sight the idea of dislocation.

On the 3d November, the patient being under the influence of chloroform, Mr Syme cauterized thoroughly the skin over the anterior and posterior aspects of the joint, rubbing a red-hot cautery iron freely backwards and forwards four or five times over each part. It had the effect of raising and rubbing off the cuticle, but did not char the skin. An hour afterwards the patient was suffering but little pain.

Nov. 4.—Said, with a smiling countenance, that she slept well last night, the first time for four months, and feels now no pain save that of the burns.

Nov. 5.—A poultice was applied yesterday; the pain of the burn is now gone, and she feels *no pain at all.* Says that she has not only lost all pain, but also that the feeling of numbness is gone from the limb, and that she seems to have more power in it. The burned parts present a white sloughy appearance.

The poultice was continued till the sloughs separated, when simple cerate was substituted for it, with the view of retarding, rather than promoting cicatrization.

Nov. 12.—To-day she has been trying to lift the arm, and felt none of the old pain in the attempt.

Jan. 31, 1854.—She has to-day left the Infirmary. She has for some time past been gradually acquiring more and more power in the limb; she can move the arm backwards and forwards for a considerable extent, and even raise it slightly. The movements of the forearm are free; there is no tenderness whatever about the shoulder. The return of the use of the limb has been accompanied with a restoration of the fulness of the muscles, so that there is now no difference between the contour of the two shoulders. She continues quite free from spontaneous pain.

I saw her again towards the end of May. She was still quite free from pain,

and there remained only some stiffness about the joint that prevented her from raising the arm to the full extent.

CASE II.—*Disease of Shoulder Joint; Actual Cautery; Cure.*

Lily Kay, æt. 50, admitted March 23, 1854. Has generally enjoyed good health, except that for the last twelve years she has suffered inconvenience from what she supposed to be rheumatism in the right shoulder, characterized by shooting pain, occurring more especially when she attempted to lift anything. In January last the limb became completely disabled from increase of the pain, which now assumed a gnawing as well as a shooting character, and also began to be felt in the elbow joint, and in the arm, forearm, and hand. At this time she first observed the existence of swelling about the shoulder joint.

The pain continued to increase till the time of her admission into the Infirmary, when it was exceedingly severe; not constant, but frequently keeping her awake at night. She was unable to raise the arm from the side, and had a sense of weakness in the limb, and some stiffness of the hand. There was considerable swelling about the shoulder-joint, which was tender on pressure, particularly at the anterior and posterior aspects. On the day of admission Mr Syme applied the actual cautery freely over the anterior and posterior parts of the joint, the patient being under chloroform. From this time she lost the old pain entirely, or at least was uncertain whether that which she still felt was not altogether that of the burn; and though the pain of the burn was considerable till the sloughs separated, yet it was much less distressing than the old pain, for which it was substituted, so that she slept much better than before the application of the cautery. The sloughs came away on the 1st April, on which day she had a slight return of the old pain near the wrist, but it has not occurred again, and she is now (4th April) quite easy. The swelling about the shoulder has almost entirely disappeared, and there is little, if any, tenderness; the sores are granulating healthily.

April 14.—Continues quite easy.

She was discharged on the 27th April; I saw her about a month after, and she still continued free from pain.

CASE III.—*Disease of Wrist Joint; Actual Cautery; Cure.*

Janet Archibald, æt. 32, admitted November 2, 1853. Rather a weakly subject. In October last she "took a shivering," without any particular exposure to cold, and a pricking pain came on in the left wrist, which increased for a time, and was accompanied with swelling. She applied poultices medicated with acetate of lead, and under their use a great improvement had taken place at the end of five weeks, when she got fresh cold in it, as she says, and it became excessively painful; the pain continued ever after till her admission, and although its extreme severity was then somewhat mitigated, yet it kept her awake a good deal at night; it was partly dull and heavy, and partly of a shooting character, and extended down through the hand and fingers. There was also an occasional tingling sensation in the fingers, and a sense of unnatural weight in the limb. A great degree of swelling existed about the wrist-joint, particularly on the dorsal aspect, and this part when manipulated gave a feeling very like that of fluctuation, so that her medical attendant had been desirous to open what he had supposed a collection of matter there.

Mr Syme regarded the condition of the wrist as almost hopeless, but as he thought suppuration had not yet occurred, he determined to give the limb a chance with the actual cautery, which he accordingly applied on the dorsal aspect in two lines, crossing one another over the articulation. The pain and swelling both diminished greatly during the first four weeks after the cauterization; some aggravation of the symptoms then occurred for a time, but as the sore was still open, Mr Syme thought it unnecessary to interfere further, and a gradual improvement afterwards took place, till at the time of her

leaving the Infirmary (Feb. 14, 1854) there was scarcely any swelling and very little pain.

I saw her again on the 10th of June, there was then no swelling whatever about the wrist, and no uneasiness except a painful feeling of weakness when she exerted it much.

CASE IV.—*Disease between the Atlas and Axis; Actual Cautery applied with great benefit.*

Thomas Smith, æt. 27, admitted June 20, 1854. Generally enjoyed good health till eighteen months ago, when a stiffness of the neck came on without any assignable cause, with pain when he turned round his head on the pillow; the pain increased greatly, and deprived him altogether of sleep for seven weeks, during which time he lost three stone in weight. There was severe pain in the head as well as in the neck, aggravated to an extreme degree by either nodding or turning of the head, particularly the latter, which, indeed, he at last never did without turning the rest of the body also. He applied to numerous medical men in Birmingham, where he lives; and blisters and caustic issues were repeatedly applied to the back of the neck, but never gave more than very slight and very transient relief, and he says that from the commencement of his complaint, he never had one minute's freedom from pain, except during sleep, till he came here.

At this time he was, according to his own account, about as bad as he had been at all. His countenance wore a peculiar expression of mingled suffering and apprehension, as Mr Syme expressed it. He complained of severe pain in the neck and head, aggravated by any sudden movement, so that there was a great constraint about all his actions. He always kept his head bolt upright, except when in bed, and could neither lie down nor get up without supporting his head with his hands; he never turned his head without the rest of the body, but gentle nodding was not very painful. There was great swelling of the upper part of the neck, and he could only open his mouth a little way; deglutition was extremely difficult, and a remarkable prominence of the bodies of the upper cervical vertebræ was to be felt in the pharynx.

On the day after his admission, Mr Syme applied the actual cautery over the spinous processes of the upper cervical vertebræ; the man was not under chloroform, and said he hardly knew whether the pain was greater even at the moment, than what he had experienced from caustic issues, and immediately afterwards he told us that he did not feel the pain of the burn at all. Next day he found less pain in moving the head, and in two or three days his countenance assumed a cheerful aspect. A steady daily improvement has since taken place in his symptoms, and at the present time (July 15) he has no pain whatever when he sits at rest, and can also use strong and active exertion without uneasiness, and no longer requires to support his head in lying down or rising; he can turn his head round pretty freely and look up to the ceiling, and it is only in sudden movements of the neck that he feels any pain at all. The swelling of the neck has greatly subsided, and he can open his jaws wide, and swallow with comparative facility. The sore on the neck is almost healed, and he talks of leaving the hospital in a few days as cured.

Remarks.—The above cases speak for themselves; and I might add several others, that exemplify in an equally striking manner the beneficial effects of the actual cautery in certain forms of articular disease. It will be observed that it is by no means so painful a remedy as is generally supposed, and also that its good effects are more than can be attributed to the mere discharge of pus from the sore which it produces, seeing that a great improvement commonly

occurs within a few hours of its application, and long before suppuration is established.

It is now many years since the use of this means of counter-irritation was introduced into Great Britain by Mr Syme; but although a constant series of successful cases have since continued to demonstrate its value to those who have witnessed his practice, yet I am satisfied that it has not hitherto been sufficiently generally appreciated. Case IV. is an example of its efficacy against a most formidable disease, where caustic issues had been long tried in vain. I believe many limbs and lives have been sacrificed that might have been saved by the actual cautery, and by it alone; and having been myself very strongly impressed with the importance of the subject, I should be truly glad if any surgeon who may have hitherto overlooked it, should be induced by the above report to inquire more closely into its merits.

ARTICLE VI.—*Case of Extra-Uterine Pregnancy.* By JAMES WILL, M.D., Aberdeen.

MRS S., æt. 39, of a thin spare habit. In the year 1844, soon after her marriage, she became pregnant for the first time, experiencing all the usual symptoms; the morning sickness began at an early period, and was severe for two months. About this time she suffered from an inflammatory attack in the abdomen, which confined her to bed for six weeks; she, however, gradually recovered, but was never altogether free from uneasiness in the bowels. The breasts were full and contained milk, and the abdomen gradually enlarged until it attained its usual size in the ninth month. Her medical attendant who had ample opportunities of ascertaining her condition, never doubted the existence of pregnancy; his only surprise was that she did not abort at the beginning or during the continuance of the inflammatory attack.

At the expected time labour pains came on with such frequency and severity that the accoucheur was induced to hazard an opinion that the case would soon terminate. The pains, however, gradually became less frequent, and finally ceased, together with the movement of the child. A discharge took place from the vagina of bloody mucus, and for some time after she continued to complain of pain in the hypogastrium, particularly on making water. The size of the abdomen diminished, and her health was completely re-established, every organ performing its proper functions.

Becoming pregnant again in 1850, I was requested to attend her in her confinement in the beginning of the year, and on the 2d January 1851, she was delivered of a healthy female child, the labour being in every respect natural, and the recovery quite satisfactory.

I was now made acquainted with her previous history, which led me to make a careful examination. The only unusual appearance presented was the existence on the left side and approaching towards the mesial line of a tumour of the size and shape of the head of a full grown fœtus; it was hard, partly moveable, but gave no pain or uneasiness.

After her delivery she seems to have enjoyed perfectly good health, but I saw little of her till she called to engage my attendance in her next confinement. This took place on the 12th February last, and on my arrival I found that she had been in labour for some hours, and that the os uteri was dilated, and the membranes protruding, and in little more than an hour she gave birth

to a healthy male child. On placing my hand on the abdomen, I found that I could insinuate it between its contracting fundus and the tumour, so that there could have been little or no connection between them.

For two days she had not an untoward symptom, but on the third she had a rigor, and complained of acute pain and tenderness across the belly, particularly in the situation of the tumour the pulse was frequent and hard; skin hot; tongue dry and coated; bowels obstinately confined.

All the usual remedies were had recourse to, and for a time with marked benefit, but a hectic state coming on accompanied by a diarrhœa, she gradually sunk and died on the 9th May.

During the whole course of her illness she had occasionally recurrences of abdominal pain and vomiting of a green bilious matter, which seemed to give her relief.

Dr Dyce, lecturer on midwifery in the Marischal College, who had seen the patient with me in April, was present at the inspection, which took place fifty hours after death.

On external examination the abdomen felt as if it contained both liquid and gaseous matter. A solid, hard, nodulated and circumscribed tumour was felt occupying the umbilical and extending into all the adjacent regions. The skin over the tumour was whiter than the rest. On opening the abdomen a quantity of gas of intolerable fœtor escaped, and on freely dividing the abdominal parietes, a mature fœtus, of a yellowish white colour, was brought into view, occupying the greater part of the space between the ensiform cartilage and the pubis. It lay with its head upwards and its back towards the right side of the mother. It was lifted out *en masse* without resistance, as it was lying unattached in a cyst which adhered so closely to the abdominal parietes as to be cut open along with them. The deep surface of the cyst had very extensive attachments to the viscera, by which it was glued down to the vertebral column,—its internal surface was of a dark colour, and contained fœces which had escaped from the bowels by three openings; but neither placenta nor membranes could be detected. The uterus lay immediately below the cyst, but had no connection with it, and was in all respects normal as well as the Fallopian tubes and right ovary, but the left ovary was not recognized.

ARTICLE VII.—*Case of Poisoning by Sulphuric Acid.* By THOMAS STEWART TRAILL, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh.

MRS CATHERINE BURNET, of Kirkliston, near Edinburgh, a most industrious and respectable widow, who earned her livelihood as a washerwoman, after a severe morning's work in her vocation, went to a small grocery shop in Kirkliston to buy a pint of ale to drink with her dinner. The grocer, it seems, sold also *oil of vitriol;* and most reprehensibly kept that article in common pint bottles on the same shelf with the ale, which was in bottles of the same size and appearance. During his temporary absence another member of his family, unaware of that circumstance, most unfortunately handed to Mrs Burnet a pint of oil of vitriol, instead of the ale.

The poor woman went to dinner, and seems to have filled a wine glass with the acid, which she hastily swallowed.

On the alarm being given, the grocer ran to the woman's house, and instantly administered to her carbonate of soda, dissolved in milk, and also gave her magnesia diffused in the same vehicle, with a little olive oil. But all was unavailing; and the unfortunate woman expired in great agony, in about an hour after swallowing the poison.

The body was judicially examined; and the stomach, with a portion of the

fluid which had escaped from it into the general cavity of the abdomen, together with the bottle containing the remains of the acid, were sent to me for examination by the Procurator-Fiscal.

The stomach was extensively disorganized. I found a considerable hole with ragged edges at its *fundus;* and the adjacent tissues tore with the slightest touch. Its villous coat was extensively mottled with dark brown patches, produced by the disorganization of the tissues and of the effused blood. Other portions of its interior, and also its peritonæal coat were reddened; the larger vessels were highly congested with dark coloured blood. Membraneous whitish flakes separated from it when rinsed in water.

The fluid from the stomach, as well as that found in the general peritonæal cavity, were strongly impregnated with sulphuric acid. The tissues of the stomach, soaked in distilled water, yielded this acid abundantly to the usual chemical tests. I found also in the stomach much sulphate of magnesia, with sulphate of soda, and some uncombined magnesia, apparently enveloped in the sulphates.

The acid in the bottle was the ordinary commercial sulphuric acid, which had acquired a brownish colour from acting on the cork that closed the bottle. Its specific gravity was 1835.

This most melancholy accident happened from the very improper and most reprehensible practice of keeping a liquid of such dangerous qualities, in the same compartment of the shop, and in similar bottles, with an article of diet; and all persons who deal in substances that may endanger the lives of individuals, if mistaken for food or medicine, would do well to imitate the example of our respectable apothecaries, who inscribe the bottles containing such articles with the word POISON in large characters, together with the usual name of the contained substance. Indeed I do not see why they should not be compelled to adopt some precaution of this kind, to prevent the occurrence of such accidents as that above recorded.

Part Second.

REVIEWS.

Traité Elémentaire des Maladies de la Peau. Par MAURICE CHAUSIT, M.D., etc., d'Apres l'Enseignment Theorique et les Lecons Cliniques de M. le Docteur A. Cazenave. Paris, 1853. 8vo, pp. 448.

Traité Pratique des Maladies de la Peau. Par ALPH. DIVERGIE, Medicin de l'Hopital, Saint Louis, etc. Paris, 1854. 8vo, pp. 736.

Manual of Diseases of the Skin: from the French of Cazenave, with Notes and Additions. By THOMAS H. BURGESS, M.D. 2d Ed. London, 1854. Small 8vo, pp. 432.

ALTHOUGH the Hospital of St Louis at Paris has been the means of furnishing us, through its distinguished physicians, with many

excellent works on skin diseases, we do not perceive that of late years our modern means of investigation have been much cultivated in that establishment. Indeed, with the exception of the recent researches on animal and vegetable parasites, no very great advance has been made in the pathology of this class of disorders, and in consequence their treatment is still conducted on empirical rules. The ideas and practice of M. Cazenave are now widely known, and appear destined to reach still further extension from the new account of them by M. Chausit in Paris, and by the English translation of Dr Burgess, which has now reached a second edition. A new systematic treatise on skin diseases has been put forth by Dr Divergie, the colleague of MM. Gibert and Cazenave at St Louis. We cannot say that it contains anything remarkably novel, but as it furnishes us with the most recent account of Parisian practice in this department of the healing art, we shall endeavour to glean from it, and from the copious notes added by Dr Burgess to this edition of Cazenave, what may be useful to our readers in their treatment of the more common forms of skin eruption.

Passing over the introduction and exanthematous affections, we are arrested at eczema, a disease which we are always anxious to cure as rapidly as possible, and one which, in its chronic form, often produces despair in the practitioner. Dr Burgess says—and we can confirm the statement—that the favourite English remedy of citrine ointment always produces unsatisfactory results. He then adds,—

"The mineral acids, liquor potassæ and bicarbonate of potass; cantharides, in many cases a most efficacious remedy; the iodide of mercury; the arsenical preparations, especially Donovan's solution (liquor arsenici et hydrargyri iodidi); and tonics, as the citrate of iron and quinine, internally; and externally, repeated applications of cloths moistened with tepid water; poppy-head fomentations in acute forms of the eruption upon the limbs; lotions of the bicyanuret of mercury, two grains to the ounce of distilled water, a very beneficial application in the inveterate forms of eczema; the linimentum aquæ calcis, as recommended by the late Dr A. Thomson. Professor Bennett, of Edinburgh, considers eczema of the scalp and adjacent parts to be a purely local affection, and consequently requires only local treatment. For this purpose he recommends a solution of two drachms of carbonate of soda to a pint and a half of water, and that lint saturated in this should be kept over the diseased surface, and covered with oil-skin to prevent evaporation. He believes that keeping the parts moist is a necessary part of the treatment. (*Edinburgh Monthly Journal*, August 1849.) I do not coincide in Dr Bennett's opinion as to the local nature of eczema capitis."—P. 112.

If Dr Burgess will carefully read the paper to which he has referred, he will find that Dr Bennett recommends as a lotion, ʒij. of the sub-carbonate of soda to a pint and a half of water, and not ʒij. of the carbonate to a pint, as is here erroneously stated. Indeed, the quantity of alkali may be diminished with advantage, to ʒss. to the pint of water. Neither ought it to have been stated so decidedly that the Edinburgh professor considers eczema of the scalp and adjacent parts to be a purely local affection; a mistake which a perusal of the first four paragraphs of his paper would have prevented.

But the more chronic eczema becomes—the more the discharges become matted and encrusted among the hair of the scalp—then, undoubtedly, the more local it is, and may at length be regarded as almost purely so. In all cases, attention to the general health is necessary, but combined with this the topical application of a slightly alkaline lotion, *kept constantly moist* with an oil silk, or gutta percha sheeting cap, is invaluable. It should also be remembered, that this treatment is best adapted to chronic eczema, for as is well pointed out by M. Divergie, moist applications in the acute form are more injurious than beneficial, and on the contrary dusting the part with flour or starch powder gives, in conjunction with a general treatment, the most relief.

According to M. Divergie, scabies consists essentially in an eruption which accompanies a peculiar product, the acarus, which in itself is the agent of infection. He considers the eruption, whether papular, vesicular or pustular, may be developed under the sole influence of uncleanliness, and that the acarus itself, under such circumstances, instead of being the cause, is in reality the effect. On the other hand, the positive experiments of M. Bourguinon prove, that the acarus does communicate the disease, and may, he thinks, inoculate the system with a poison, which explains the occasional presence of an eruption where no insect can be discovered. This hypothesis is considered by Divergie as probable. Professor Boeck of Christiana, has described a new form of scabies, which to the naked eye presented somewhat the appearance of rupia. But when the hard crusts were examined with the microscope, they were found to consist of a compact mass of acari, their eggs and excrement.

With regard to the treatment of scabies, M. Divergie points out that there are two things to get rid of, namely, the eruption and the acarus—that whilst the last is destroyed by frictions with Helmerinch's or the Stavesacre ointment, the former is often aggravated by such a treatment. In short, if the eruption be acute and pustular, emollient baths, purgatives, and a calmative treatment is required in the first instance, and that instead of following a routine plan of medication, it should be modified according to the stage of the disease, the delicacy of the skin, sex, age, etc. This chapter on scabies by M. Divergie, is well worthy the perusal of every medical practitioner.

With regard to the treatment of acne, Dr Burgess in a note says :—

"M. Cazenave has recently recommended in his clinical lectures ammoniacal lotions, the action of which, he says, is principally chemical, forming, with the fatty matter contained in the follicles, a soluble soap, with an ammoniacal base. The hydrochlorate, or the acetate of ammonia, will answer equally. The iodide of sulphur ointment is one of the most useful local remedies ; and a lotion of biborate of soda, with glycerine, to parts where the hair grows, will also be found very efficacious. I have seen these remedies act well in a case of three years' standing. The patient was connected with the *Times* newspaper, and the heat and glare of the gas all night had aggravated the disease

considerably. The eruption on the face became gradually extended, until in a few months the face and forehead were completely covered with a red eruption of an unsightly character ; first appearing in blotches, then speedily extending over the whole surface. After a time it would partially die away, but the blotches soon re-appeared as before, and became again diffused. This was frequently repeated. The patient here referred to was completely cured with the preparations of phosphorus, iodine, and sulphur, and a wash of the biborate of soda and glycerine. The creasote ointment is sometimes used in chronic cases of acne, but I have never seen much benefit derived from it. The repeated use of the warm bath, or the vapour bath occasionally, will materially assist the action of the remedies above mentioned. Indeed, baths cannot be too freely used in the treatment of this, and of the great majority of cutaneous diseases, although they are but seldom had recourse to in this country."—P. 219.

Chronic papular eruptions are at all times exceedingly difficult of cure. Dr Burgess considers them to consist essentially of a disorder in the cutaneous nerves, which in its turn is often the result of sympathy with a disordered condition of one or other of the mucous surfaces. He adds,—

" In accordance with this view of the elementary nature of the papular diseases of the skin, I have been lately in the habit of prescribing strychnia and phosphorus in those distressing cases of prurigo which reduce the unhappy patient to the most abject state of human suffering, and too often baffle the usual methods of treatment prescribed in works on cutaneous pathology. In several cases of this kind, and after the acids, alkalies, and arsenic internally ; and hydrocyanic and acetic acids in the form of lotion, had utterly failed to subdue the unceasing pruritus, I have found the phosphorated ether to succeed, given internally, and preceded for a day or two by repeated doses of the tincture of hyoscyamus.

" In the prurigo of old age, I have also seen it succeed in allaying the tormenting itching and tingling, after the usual remedies had failed. Can it be a revivifying effect which phosphorus produces on the nervous centres that causes this alleviation of the pruritus ? As the proportion of phosphorus in the young and healthy human brain is considerable, being from eight to eighteen parts in one thousand of the whole mass, or from one-twentieth to one-thirtieth of the whole solid matter ; and as it is unusually deficient in idiots and in extreme old age, the loss of so necessary an ingredient of the nervous mass may occasion the irritation in the peripheral nerves which constitutes prurigo senilis. This is evidently a lesion of innervation, associated with decrepitude, and a sure indication of the gradual disintegration of those structures and functions through whose combined agency the process of life is carried on.

" But why should there not be *neuroses* of the organ of touch, or of the skin, as well as of the organs of sight, hearing, smell, and taste ? It is not because the " hallucinations of the sense of touch," as the French pathologists call those morbid conditions of the skin which come under the heads of *hyperæsthesia*, and *anæsthesia*, are so difficult of explanation, that we are to deny their existence. There is no doubt but several of these cutaneous disorders which we have been accustomed to regard as the result of inflammation, in consequence of placing an undue reliance on their *secondary* products, as a means of diagnosis, are in reality lesions of the cutaneous nerves, producing exaltation, or other modification in the sensibility of the skin.—P. 278-9.

Although there can be no doubt that the cutaneous nerves are affected in this as in many other skin diseases, it must be remembered that the papulæ are evidences of a lesion of nutrition as well.

In other words, there is a new formation, but on what this depends is unknown, and demands further investigation.

We find nothing new with regard to the treatment of the scaly eruptions. Pitch ointment externally and arsenic internally constitute the chief remedies. Unfortunately they do not prevent the return of the disease, and a method of permanent cure is still a desideratum. Dr Burgess speaks well of Donovan's solution, a medicine which has failed entirely in our hands, and recommends when psoriasis is confined to the legs, fumigation with cinnabar or flowers of sulphur.

For lupus, Divergie recommends cod-liver oil internally, and the oil of cade externally, applied with a camel's hair pencil every other day.

From what we have now said, our readers will see that the work of M. Divergie merits attentive perusal, and that the well known manual of Cazenave has been greatly improved by the valuable and numerous additions made by Dr Burgess to the present edition.

Notices et Extraits des Manuscrits Medicaux Grecs, Latins, et Français des Principales Bibliothèques de l'Europe. Par le Dr Ch. Daremberg. I. Partie, Manuscrits Grecs d'Angleterre. Paris, 1853.

Œuvres d'Oribase. Texte Grec en grande partie inedit. Traduit pour le premier fois en Français avec une Introduction. Tome 2d. Par les Docteurs Bussemaker et Daremberg. Paris, 1854.

The medical profession in this country lies under obligations which, we are sorry to say, it has been very dilatory in acknowledging, to the learned and enterprising Dr Daremberg of Paris, who, with a degree of literary zeal seldom equalled in the present age, has given in the former of these works the results of a thorough exploration of the medical MSS. in Greek, which lie unedited in the public libraries of Great Britain. It cannot but appear remarkable, since England can boast of so many rich scholarships and literary endowments, that she should have been obliged to devolve this task on a foreigner, but when once performed, it seems still more wonderful that none of our classical or professional journals should have proclaimed the feelings of gratitude with which such an undertaking had been regarded in this country. And yet, not only have Dr Daremberg's labours in bringing to light the hidden treasures of our great libraries passed unnoticed by all our quarterly, monthly, and hebdomadal periodicals, but with the exception of a previous notice of the first volume of his Oribasius in this Journal, we are not aware that any one of his literary performances has ever been brought into public notice through the influence of British reviewers. We regret that our narrow limits in this department do not admit of our giving such a

disquisition at present on the merits of these works as they are justly entitled to, but we regard it as a duty to say a few words in their commendation, with the hope of inducing some of our contemporaries to do greater justice to them.

Of the former of these works, indeed, our notice shall be very brief; for, although it contains much curious matter which cannot fail to interest every one who is possessed of a minute acquaintance with ancient and mediæval literature of medicine, we must admit that it is not likely to prove very attractive to the general reader, who is not qualified to appreciate the value of such minutiæ. We may just mention then that it contains an analysis of the more important Greek MSS. connected with ancient medicine, which lie in the libraries of Oxford, Cambridge, Middlehill, the British Museum, and the Medical Society of London. This examination of our neglected storehouses had evidently been conducted with great care, ability, and skill, by Dr Daremberg. At the same time, since from their nature the contents of the volume do not admit of being stripped of detail, and presented to the reader in a popular shape, we shall rest content with having simply drawn the attention of our learned readers to the work in question.

The other work is not only more attractive, but is of an eminently practical stamp; for it is rich in the treasures of great facts and recorded observations on some of the most important questions which can come under the domain of our profession. The second volume of the works of *Oribasius*, now for the first time published in the original, touches fully on bleeding, general evacuations, the influence of air and climate on health, baths, and the materia medica. In a word, the volume contains an elaborate exposition of the opinions of ancient authorities on what must be regarded as some of the most important points of medical practice even at this day. Considering the lamentable changeableness of professional views within the last twenty or thirty years on many points connected with the application of these remedial means, and the difficulty there often is in deciding whether a popular medicine owes its present reputation to its virtues or to fashion, it is surely proper and desirable not to neglect any method of training the mind at all likely to improve its power of separating truth from error; nor can too much importance be easily attached to a work which, like that of Oribasius, embodies the accumulated results of Greek and Roman experience, in an age when the human intellect had not yet sunk into that miserable state of imbecility and barbarism with which the civilized world was destined soon after to be overwhelmed. Here, indeed, we could certainly have wished that our limits had admitted of our giving an abstract of ancient opinions upon some, at least, of these subjects,—such as bleeding, or baths, spatiis; but, "studiis inclusus iniquis," we must not venture on any such attempt in the hopes of doing justice to it.

We may mention, however, that Dr Daremberg has added greatly

to the value of his work, and we should think to its general attractiveness, by bringing the opinions of the highest living authorities in France of the present time to bear on the points of practice mooted by his ancient authorities; so that an intelligent reader is thus enabled on easy terms, and at one view, to compare ancient and modern opinions on some of the most interesting subjects in practical medicine. That this must afford a most wholesome exercise to the mind of the intelligent physician, no one possessed of any liberal spirit will venture to deny. And here we would take the opportunity of saying, we have often regretted that the method of teaching medicine now prevalent in this country is entirely dogmatic, and founded on a strict deference to established authority; the student has such and such opinions inculcated upon him as true, but in general he is not told on what grounds his teacher upholds them to be such, nor whether these dogmas be controverted in any other quarter. One cannot but lament the fatal credulity and ill-grounded reliance with which the newly-initiated in medicine thus begin the exercise of their art, and pity their first patients who are so unfortunate as to be the subjects on whom they attempt to reduce their preconceived opinions to practice. We have often thought, that as in every well appointed College of Divinity, there is a Professor of Ecclesiastical History, whose office it is, or at least ought to be, to give a fair exposition of by-gone doctrines, and to state the origin of those which are prevalent, so in every complete school of medicine there ought to be a Professorship of Medical History; or failing this, that the teacher of every branch in medicine ought to embody in his prelections an exposition of the views of distant ages and countries. In short, it would be highly important that every teacher of medicine and surgery were a cosmopolite in his own sphere. In this country we fear it is far from being the case at the present day that teachers are possessed of these comprehensive and liberal views.

Looking to the importance now attached to baths in the practice of medicine, we would most earnestly call attention to the excellent chapter on them as given by Oribasius, and also to the interesting annotations by the editor. There are few subjects connected with antiquities on which a larger amount of misapprehension has long prevailed, than on the method of using the public baths as practised by the Greeks and Romans. With Dr Daremberg's views we entirely coincide, and shall here briefly state "on our own authority," what we believe to have been the ancient process of using the Thermæ. The person about to be bathed went first into the warm apartment called Apodyterium or vestry, where he left his clothes, and immediately passed into the Laconicum, an apartment filled with vapour or dry heated air, where he was shampooed *secundum artem;* he then went into the Caldarium or hot bath, where he remained for a longer or shorter time according to his taste; and leaving it, he took a plunge into the cold bath, which was close to the vestry, and then

being well rubbed, first with towels, and afterwards with fragrant oils, he resumed his clothes. One need scarcely remark how applicable this process must have been to almost all ages and circumstances. The directions for the use of the cold bath as given in an extract from the works of Agathinus are highly important, and even in an age of hydropathic experience, little further remains to be added regarding its uses. A curious account is given (x. 8) of a remedial process now entirely obsolete, we mean *le bain du sable*, or sand bath. It appears to have been principally practised in hot climates, but with a little contrivance it could of course be easily applied at any time, or in any place. It consisted in covering up the affected limb, or even the whole body, except the head, with sand heated by the sun, a process which is said to have been very efficacious in provoking copious perspiration. It was applied in affections of the chest, gout, anasarca, and other chronic diseases. By the way, we have often wondered that the vapour bath should be so sparingly used in this country for the cure of anasarca, and other cognate diseases. On the passage of Oribasius (p. 469) when the vapour bath is so recommended, Dr Daremberg says in his annotations that "the efficaciousness of vapour baths in anasarca is confirmed by modern observation; that anasarca connected with albuminous nephritis, and which often proves fatal, can only be cured by the use of the vapour bath." The ancients also had recourse in such cases to a process similar to the dry air bath of the moderns (p. 468). Another of the ancient modes of applying the bath for the cure of diseases, consisted in adding about a sixth part of oil to the water in the bath. It was called the bath of oil, and was used to relieve the effects of lassitude, and other chronic pains.

It is not solely, however, nor indeed principally with the view of attracting distinction by restoring forgotten remedies, that we would recommend the pages of Oribasius to the attention of our contemporaries, but because we regard the study of ancient literature in all its departments as a process of mental culture highly calculated to guard its possessor from the dangerous bias of prevalent errors. That there are at the present time many professional doctrines, sanctioned by authority, and recommended by fashion, which will require in the end to be abandoned as erroneous, few physicians of sober judgment will hesitate to admit; and surely he who is thoroughly instructed in the rise, progress, and decay of similar doctrines in former ages, will be more likely to keep his mind aloof from the dangerous contagion of error, than he who has never learned a lesson from the wisdom and follies of our forefathers. Such a one will receive with considerable hesitation the reports industriously circulated and greedily swallowed of the marvellous efficacy of some new remedy, more especially if it be impossible to trace any obvious connection between the physiological action of the vaunted medicine and the remedial powers which it is held to possess. For example: is there any person possessed of a sober judgment and familiar with the history of medicine, who

can doubt for a moment that the medical powers of cod-liver oil have been much over-rated of late years? We do not mean to deny that in constitutional emaciation, especially when connected with tuberculosis, many patients are benefited by it. But the practice now so prevalent of administering it indiscriminately in almost every disease of an obstinate nature, we hold to be the result of mere fashion and popular delusion, and to be capable of raising it in the estimation of the ignorant and credulous to the rank of a *panacea*. In this case a physician skilled in the history of his profession, will season his confidence in the new remedy with a suitable admixture of scepticism, when he recals to mind the high reputation once enjoyed by other medicines now little esteemed, such as the viper's flesh, the Millepedes, and the Bezoar stone of the Greeks, Romans, and Arabians? But we must cut these reflections short, and hasten to a conclusion.

With regard to the editorial merits of this volume, we need only say that they are fully equal to those of the first volume, previously noticed by us. A correct text, beautiful print, good paper, *variæ lectiones* at the foot, and at the conclusion most valuable annotations embracing philological and professional matters of the highest importance, what more could we desire in a perfect edition of an ancient author? We sincerely wish Dr Daremberg and his coadjutor great success in their laborious and useful undertaking, and trust that their example will rouse the profession in this country to similar exertions in the cause of ancient literature. So important do their services appear to us, that in our opinion no physician has a right to regard himself as being properly acquainted with the standard literature of the day, provided he has never directed his attention to the labours of Daremberg and Bussemaker.

Six Lectures on the Pathology of Strabismus, and its Treatment by Operation. Delivered at the Westminster Hospital. By C. Holthouse, F.R.C.S.E., Assistant-Surgeon of the Hospital, and Lecturer on Anatomy in its Medical School. Lond.: Churchill, 1854.

We have perused Mr Holthouse's lectures with much pleasure. They contain not only an accurate exposition of the principles upon which the treatment of the disorder should be conducted, but also various original suggestions which will prove useful to the oculist or surgeon in performing the operation; for, with all its improvements, it has not hitherto proved so invariably satisfactory as might be desired.

From the author's first lecture, upon the anatomy and physiology of the eye-ball, we extract the following jottings: "The breadth of the internal rectus muscle in the adult is 5·428 lines. So broad is the insertion of the tendon of the muscle, that it not unfrequently

exceeds the diameter of the cornea; an important fact to recollect when operating for the convergent strabismus. The tendons of these recti muscles are perfectly separate from each other, and do not coalesce by their margins, as was formerly supposed, to constitute the so-called *tunica albuginea*, but are inserted into the sclerotic, their extremities being embedded in its substance. These muscles, together with the two oblique, and the lavator palpebræ are connected together and are inclosed in the ocular fascia, which inverts the eyeball as far forwards as the cornea, where it is thick, strong, and dense, whilst it is very thin posteriorly. The practical point in connection with this structure to which especial attention should be directed, is, that three layers of the fascia intervene between the conjunctiva and the sclerotic, viz., the sub-conjunctival layer, the sub-muscular, and that which immediately invests the sclerotic; all of which must be divided in the operation for the cure of strabismus.

The author's original investigations, we may also mention, go to confirm the statement made long ago by J. Hunter, and more recently by Hücke and Dr Jacob, that the action of the oblique muscles has the effect of causing the eye to roll round an imaginary access, passing through the centre of the cornea to the posterior part of the sclerotic; whilst his original experiments corroborate those of Mr Bransby Cooper, that the division of the oblique muscles of the eye produce a permanent retraction of the globe within the orbit.

The author's SECOND LECTURE is occupied with the PHENOMENA of strabismus, which it appears to us are judiciously and accurately narrated. The *single convergent* strabismus is of course enumerated as the most common. Sometimes, however, in this category, the inversion is not constantly present in the same eye, but shifts to the opposite one, where it will sometimes remain for a considerable time. Nor is it easy on the moment to decide which is the affected eye, until we compare the vision of the two, when it will be found that the really squinting eye is the most defective in visual power. Another test consists in desiring the patient to close each eye alternately, while he is desired to look at an object at a short distance in front of him with the eye which remains open. If this be the sound one, it will preserve its position in the centre of the orbit when the lid of the closed eye is raised, but if it immediately turn inwards, it is the one at fault. The same experiment should be repeated on the other eye.

Next in frequency to the variety above alluded to is the *Divergent* strabismus, the reverse of the preceding, wherein the affected eye is *everted* instead of inverted; the degree of eversion, however, being seldom so great as the inversion in the former class of cases. A subvariety is what is known under the name of *Lucitas*, and which is distinguished by the greater immobility of the eyeball. The patient is unable in these cases to move the eye from its unnatural position, whether the sound one be open or closed, so that the cornea remains permanently in its abnormal position. This variety, it would

appear, appears most frequently in the category of cases now under review.

Mr Holthouse's treatise is enriched by a number of interesting cases, some of which we should have presented to the notice of our readers had space permitted. We shall here adduce one, and not simply for its own sake, but as bearing upon an interesting physiological problem which at one time was regarded as settled and disposed of.

Mr W., æt 21, has squinted from two years of age, and is not aware of any cause to which the deformity can be attributed, except it be hereditary tendency, to which he ascribes it; several of his relatives being, he says, similarly affected, and some of them in a higher degree than himself. It has existed in the family for several generations. Both eyes are more convergent than natural, but equally prominent; pupils are alike. On regarding objects in front of him, while unobserved, one eye is always inverted, though not always the same eye, as he uses both indifferently, the vision of each being perfect; but if conscious that he is observed, he can prevent the inversion of either eye singly, and maintain them both very nearly parallel. On looking sidewise, one eye is too much inverted, and the other is too little everted. Notwithstanding this want of parallelism, he positively declares that he sees an object, such as the finger held to the side of him, with both eyes at a time; and sees it *single*, and this is the case whether the object be held to his right or left side. He never sees double under any circumstances whatever. The power of inverting both eyes is somewhat imperfect, he being unable to approximate the cornea nearer than two lines of the inner canthus.

This is an extremely interesting case, inasmuch as it throws light on certain phenomena connected with binocular vision. I presume, says Mr H., that most of you are aware that, as a general rule, for single vision with both eyes it is necessary that the image of an object gazed at should fall on identical or corresponding parts of the two retinæ, and if this condition is not fulfilled double vision will result. Hence, certain physiologists have contended that the identity of these parts of the two retinæ is an inherent property of that membrane; while others, on the contrary, affirm that objects are seen single, not by any inherent property residing in it, but simply from certain parts of the retinæ being accustomed to act together; and that under certain circumstances objects will be seen single, though their images are not depicted on such corresponding parts.

Now the case in question is—according to our author—decisive evidence of the correctness of the latter hypothesis, as the individual never sees double, though it must be obvious, from the want of parallelism in the two eyes, that images must occupy *non-identical* points of the two retinæ.

Lecture 3 is devoted to *the remote causes of Strabismus*, immediate and remote. First among these is placed certain congenital defects of the organ of sight, such as deficient sentient power of the entire retina of the eye, or the limitation of sensibility to one part of this membrane, and that not in the axis of the eye; or, again, great inequality in the focal power of the two eyes. Previous to the introduction of the operation of dividing the internal rectus, it was generally believed that the squinting always resulted from the defective vision, of an effort, as it were, made upon the part of the patient to move the eye out of the axis of vision, that it might not interfere with the impressions excited on the retina of the sound eye. But the operation in question has been a sort of *experimentum crucis*, by

which we have learnt that, in many cases, the impaired sight is the effect and not the cause of strabismus; the operation not only restoring the symmetry of the two eyes, but equalising their visual power.

Hereditary tendency and various diseases of the brain are the causes next enumerated, and little doubt can be entertained of their influence. The excess of nervous force, directed to certain muscles, through the medium of volition; under which latter head must be grouped all those cases which owe their origin to ophthalmia and irritation. In Mr C. R. Hall's experience, about one-tenth part owe their existence to this last cause. Another cause appears to be a hypertrophied condition of the inner rectus of the sound eye, while the corresponding muscle of the squinting one presented no abnormal enlargement; and when the strabismus affected both eyes, the least distorted was that in which the internal rectus was enlarged; and the most distorted, that in which it presented only its natural dimensions.

One fact more must be adverted to, viz. the occasional occurrence of strabismus in an eye which was previously straight on rectifying its fellow which squinted; so that *the rectification of a squinting eye* must, to a certain extent, be regarded as a remote cause of a similar affection in the opposite one.

Lecture 4 is upon the immediate proximate cause of strabismus. Impaired vision is one of the most common of these, though no satisfactory solution of the phenomenon has yet been supplied. The changes produced in the muscles by disease or irritation of the nerves, or nervous centres, are often either paralysis or spasm; and these, if of long continuance, terminate in atrophy in the one case, and structural shortening in the other. Though the author's experience leads him to think that the weakened condition of the external rectus is not so frequent a cause of strabismus as many suppose, yet he supplies the following case in point, which cannot be read without interest. We give it short:—

J. P., æt. 30, compositor, applied on March 6, 1847, for a convergent strabismus of the left eye. He stated that it interfered so much with the vision of the other eye, as to incapacitate him for his ordinary business. On examination I found it a case of *Lucitas*. A further examination showed that the strabismus arose from complete paralysis of the external rectus *muscle*, not from any spasm or shortening of its antagonist. He could invert the eye to the same extent as the other, but could not evert it, or bring it within the central axis of the orbit; the cornea, when not acted on by the internal rectus, occupying a position midway between the centre of the eyelids and the inner canthus. What renders the case more interesting, and places beyond a doubt the cause of the deformity, was the complete paralysis of the left half of the tongue. On thrusting it from the mouth, its point deviated to the left side, and it was unsymmetrical, the left half being broad, flat, and flabby—in fact, atrophied; whilst the right was plump, firm, and roundish. Now the nerves supplying the abductor of the eye and the genio-hyoglossus of the tongue are the sixth and the ninth, both of which arise together from the motor track of the medulla oblongata. The patient had been the subject of syphilis, had had ostitis of both tubiæ, and suffered continual pain in his head and neck. This led me to

believe that the occipital had participated in the disease, and the two nerves just named, which are in contact with the basilar process of that bone, were probably secondarily affected. The operation was of course declined, and the strabismus gradually disappeared under the influence of the remedies prescribed for the syphilitic affection. A good case in which the squint was produced by the palsy of the abductor oculi.

In direct contrast to the above case is the far more common variety of the disease, which is obviously due to the increased power and bulk of one or other of the internal recti. The history of strabismus, as well as the phenomena, point to this condition of muscle as one of the most frequent causes of the affection. All those cases, therefore, which arise from irritation, from bandaging up the affected eye—from ulceration, or central opacities of the cornea, from misplaced pupil, and from misplaced retinal sensibility, are probably owing to this condition of the internal rectus. The *modus operandi* of squinting thus produced, is satisfactorily explained on the hypothesis of over action, or very frequent action, of the muscle increasing its strength and bulk; and this change having resulted, the external rectus is overbalanced, and the eye becomes apparently inverted. The fact of the eye being thus given up to the action of the more powerful muscle, will also explain why the external rectus becomes weaker; it is seldom called upon to act, so that it probably becomes smaller and somewhat atrophied, and in this manner the deformity is perpetuated.

Lecture 5 is occupied with the somewhat obscure topic of the subjective phenomena of strabismus. An interesting subject here suggests itself, viz., Why is the distortion, in nearly every instance, either in the outward or inward direction, and not upwards or downwards? Mr Holthouse's estimate of the comparative frequency of the several species of strabismus very much accords with that published by Mr Hall. In this gentleman's experience, out of 213 cases, 13 only were divergent, 19 were double convergent, and all the others single convergent; the left eye being exclusively affected in 110 cases, and the right in 71—a circumstance which is probably due to the greater power of the abductor muscles, and the branch of the 3d nerve, which enters the internal rectus, being generally somewhat larger than that of the 6th, which supplies its antagonist.

The cause of the impaired vision constitutes the most difficult, though probably the most interesting part of the subject. Some ascribe it to *disuse* of the organ, others to the implication of the optic nerve, and others to the same cause which produces the distortion of the eye. The first of these hypothesis is, according to our author, inconsistent with the fact of the immediate improvement subsequent to the operation. Dr Franz has stated that there is merely a transposition of the most acute point of sensation of the retina: upon which our author remarks—the theory of the transposition of the most acute point of the retina to another point, is based on the assumption that such a point really exists, and that it corre-

sponds with the yellow spot of Sœmmering. Experiments, however, he adds, which I have recently made, throw some doubts on the correctness of this opinion, and lead me to believe that in the healthy eye at least, a large portion of the retina is possessed of the same power. Admitting, he adds, that in certain cases the seat of the impaired vision may be in the sentient parts of the organ of sight, or in the brain, I am of opinion that in the majority of cases, *it resides in the dioptric parts of the eye*, which has undergone some change of form or tension by muscular action. "It is only, he adds, upon this hypothesis that we can explain that curious phenomenon, the immediate improvement, and sometimes complete restoration of vision, which follow division of the faulty muscle in strabismus—a fact, by the by, which adds weight to the opinion of those who maintain that the defective vision is, in nearly every instance, the *consequence*, not the *cause*, of the strabismus."

The concluding Lecture, on the treatment of Strabismus, is the most important, as the most interesting of all. Here Mr H. first indicates those cases in which the operation should not be undertaken. They are shortly these,—1*st*, Those in which the distortion enables the individual to see better than he would do were the eye undistorted, as from central opacities, etc.; 2*d*, Those in which the distorted eye is either completely amurotic or affected with cataract; and, 3*d*, When the distortion arises from complete paralysis of either the external or internal rectus. Division of the unparalyzed muscle, in either of the cases, would not rectify the squint, whilst, at the same time, under proper treatment, it is not unfrequently recovered without operation. Another suggestion of the author, not less important, has reference to those cases in which the affected eye is more prominent, and appears larger than the sound one. Operation in such a case will then, in all probability, only increase the fulness, and exaggerate the deformity. On the contrary, where the squinting organ is apparently smaller than the sound one, the prognosis is more favourable, and these cases are the most promising for successful operation.

The common operation Mr H. describes in the following succinct words:—

> Having selected your case, and bound up the sound eye, a proceeding which much facilitates the operation, the spring speculum of Mr Coxeter should be attached to the lower eyelid, whilst an assistant, standing behind the patient, with the finger of one hand, or a speculum, raises the upper eyelid; he then with the other hand, draws the eye from its unnatural position, by means of the double tenaculum inserted into the sclerotic, one or two lines internal to the margin of the cornea. The eye being thus fixed, the operator, with a pair of scissors, makes an incision three or four lines internal to the cornea, or about midway between it and the caruncula lachrymalis, commencing at a point a little below a line horizontal with the lower border of the cornea. This section must be made to include, not only the conjunctiva, but the ocular fascia, extending quite down to the sclerotic coat; the lower blade of the scissors must then be pushed from below upwards, in a vertical direction, care being taken to keep it in close contact with the sclerotic, while all the structures interven-

ing between the two blades, as high up as a line corresponding with the upper margin of the cornea, are cut through with a few sweeps of the instrument.

He then describes the mode in which the knife is employed instead of the scissors, remarking—

The operation must be commenced with a small incision made by the latter, as in the former method. Through this aperture a grooved director is passed beneath the muscle till its point is on a level with the highest part of the cornea. A sharp pointed bistoury, guided by the director, is now made to cut its way outwards, dividing in its passage all the intervening structures.

If the squint should continue in spite of the complete division of the internal rectus muscle, and the separation of any adhesions between it and the sclerotic, the inversion is probably owing to a weakened condition of the abductor muscle, and nothing more is necessary than to bind up the sound eye for a few days; the weakened muscle will thus be called into action, and will shortly acquire power sufficient to prevent a recurrence of the inversion after the sound eye is unbound.

The author particularly cautions against the division of more than one muscle in the same eye, as formerly often practised. The proceeding is followed by great protrusion, and not unfrequently by a squint in the opposite direction. Many of these cases are not so much failures as examples of deferred success, in which the eye eventually rights itself. On the contrary, should no alteration have been taken at the end of eight or ten days, the inner muscle of the opposite eye should be cautiously divided.

Mr Holthouse intimates, in conclusion, that he is now engaged in the investigation of the cure of strabismus without operation, the result of which he means to detail on a future occasion, and which we doubt not will be received with all becoming attention by the professional public.

Handbook of Inorganic Chemistry, etc. By Dr Gregory. 1854.

The Chemistry of Common Life. By Professor J. F. W. Johnston. Vol. I. 1854. Edinburgh and London.

Cyclopædia of Chemistry, with its Applications to Mineralogy, Physiology, and the Arts. By R. D. Thomson, M.D. 1854. Pp. 539. London and Glasgow.

It is a favourable sign of the progress of a science, that continual additions are made to its text-books, even though some of these much resemble others in their contents. An embarrassment of riches is the least perplexing of all dilemmas, and considering how various the tastes of students are, new manuals, if well written, are never unacceptable.

The third edition of Dr Gregory's well known work on inorganic chemistry has remained too long unnoticed on our hands; but we have little more to say of it, than that it is an improvement on pre-

ceding editions, both as regards the number and nature of the subjects discussed. Some carelessness in correcting proofs has led to a rather perplexing confusion in the description of the poles of a voltaic circle, and there is room for a fuller and more explicit account of electricity; but these are minor blemishes, detracting little from the excellence of the book.

Professor Johnston's work, which is now more than half finished, exhibits the skilful fulfilment of a happy idea. His erudition; his literary acquirements; his experience as a writer; his familiarity with geology, agriculture, physiology, and botany, as well as with chemistry, enable him to produce a much more generally useful and attractive book than a mere chemist could have written; and the uniform clearness and terseness of the style make the book very pleasant reading. It will be acceptable to those to whom the matters he discusses have been subjects of study before, and should be specially serviceable to those by whom they are studied for the first time.

The many botanical references which the work contains are illustrated by wood-cuts; and, considering the number and excellence of these, we cannot but commend its cheapness. We trust, however, that the publishers will find it possible to produce hereafter a cheaper "People's Edition," for the sake of that large section of the community who most need the book but can least afford to purchase it.

Dr Thomson's Cyclopædia is a carefully written, valuable work. He has enjoyed extensive opportunities, especially at Glasgow, for the study of chemistry in its technical applications, and has made many laborious investigations into the chemical relations of those substances which are most largely used in the arts. On all such subjects, accordingly, his dictionary treats with authority; and brief though the descriptions are, they are explicit, and sufficiently full for all purposes short of minute directions for prosecuting manufacturing processes.

On those branches also of organic chemistry which have relation to food, drink, respiration, ventilation, disinfection, and other Hygienic topics, to which the author is known to have given much attention, the articles in the Cyclopædia are always of interest and value.

A large space is given to mineralogy, of which the author thus speaks in his preface:—"The preservation of the study of minerals, that is, of elements and salts found in nature, as the essential basis of the science of chemistry, seems so important that attempts to restrict mineralogical arrangements to mere crystallographic systems, are much to be regretted, since that valuable auxiliary in the discrimination of physical forms is as thoroughly applicable to artificial salts as to those found in Nature."

We agree with the author in thinking that mineralogy and chemistry should not be separated, since both suffer by the separation.

The excessive devotion of our chemists to organic chemistry has led for the present to the neglect of mineralogy, but this neglect cannot continue long; and already some of the most distinguished chemists, both in this country and abroad, are advocating the doctrine, that the most complex organic compounds are constructed on the type of the very simplest inorganic ones.

The only fault we have to find with the Cyclopædia is, that its bulk and price have been needlessly increased by a number of references which we fear will be of little service to most readers, however creditable to the erudition of the author, who, since the death of his distinguished relative, Dr Thomas Thomson, stands unrivalled among our chemists for strange, out-of-the-way learning. Perhaps we only display our ignorance when we ask who is specially anxious to know that "*Bardiglione*" is a kind of alabaster; that "*Birousa*" is a name for the Turquoise; that "*Exitele*" signifies white oxide of antimony; or "*Exanthalose*" sulphate of soda? Yet why complain of such things? Can we possibly be too learned? Sir William Jones, when a child, was taught by his mother, and in after-life learned to prize the precept, "There is no knowledge which may not be of use at some time;" and at least it can do no one any harm to learn from Dr Thomson that gin is derived from Juniper or *Genievre;* that rum is the last syllable of sacch*arum;* that whisky is derived from the Gaelic "*Usquebaugh,* pronounced whiskybay;" and that calomel was once named *Draco Mitigatus.* We cordially commend the Cyclopædia to our readers.

Reports on Civil Dispensaries at the Presidency and in the Province of Madras, during the Year 1852. Published by authority of the Right Hon. the Governor in Council. By ALEX. LORIMER, M.D., Secretary to the Medical Board. Madras, 1853.

A PAMPHLET, bearing the above title, has lately reached us. It contains brief abstracts of the reports transmitted to the Central Medical Board from thirty-one dispensaries, of which six are in Madras and its vicinity, the remaining twenty-five in the other large cities and towns of the province. The number of in and out-patients treated in these institutions during 1852, was 119,619. The daily average numbers of patients are stated at 2530, of whom 408 were in-patients. Eight of the dispensaries have been in operation since 1842. They are resorted to chiefly by sick natives; and are served partly by European, partly by native, medical officers; by the latter chiefly as dressers. The gross expenses were 42,528 rupees for the year.

Bowel complaints, cholera, dropsies, and diseases of the lungs, were the most fatal diseases. Cholera was not epidemic during the year; and, generally, the Presidency was unusually healthy. This was supposed to be attributable, in a great measure, to the large fall

of rain, which, for the year, had amounted to 73 inches. Provisions of all kinds were abundant, and at moderate prices.

In the General Hospital of Madras (in which 859 patients were treated in 1852), the students of the Medical College receive clinical instruction. In the same city there are also hospitals for lepers, with a daily average of 55 patients; and for idiots, with an average of 52.

In the abstracts of the reports from the several dispensaries, a few of the more remarkable cases are given in detail; from these we select the following:—

"An old woman, named Paupammah, æt. 65, was attacked in the middle of the night by a buffalo. She was seen about two hours after the accident. There was an irregular jagged crescentic wound, three or four inches long, in the right iliac region, out of which were protruding between three or four feet in length of a small intestine, coiled outside of the wound in the abdomen, and covered with sand and bits of straw; she was perfectly insensible, and quite cold. On washing away with care all extraneous matters, the peritoneum appeared unusually red and vascular; during the manipulation necessary for the reduction of the bowel, the old woman shrunk as if sensible of pain, but did not speak. Prior to the reduction of the intestine, I removed from the abdominal cavity several large black coagula of blood which presented themselves; a considerable quantity of bloody serum also exuded.

"The wound was stitched with four interrupted sutures, and these were supported by a soft compress and a bandage. In spite of my remonstrances, the friends removed her immediately from the hospital, I believe under the impression that she must die, though I told them their removal of her was more likely to cause her death, as it was necessary we should watch her closely, in case any bad symptoms should present themselves. I had inquiries made after her, and on the third day succeeded in getting the friends persuaded to bring her back to the hospital. On the twentieth day she left quite well, not having had one unfavourable symptom. Six months afterwards she was seen in good health, but with a hernial protrusion in the site of the wound, which gave her no inconvenience."—Reported by J. Peterkin, M.D., Cuddapah.

From Mangalore, Dr D. D. Foulis reports that "in chronic rheumatism much benefit was derived from fish-liver fat being well rubbed in morning and evening, which had the effect of soothing pain and reducing swelling." "In cases of atrophia, diabetes, and scrofula," Dr Foulis adds, "fish-liver oil has been largely given, and attended with an amount of benefit truly surprising. Of its virtues there can be no question."

It is most gratifying to learn that so much is done by the ruling powers in India, to relieve the sufferings of the native population. How wide is the field thus opened for professional improvement, both to European and to Indian practitioners! Through these dispensaries, and through the medical colleges of Calcutta, Madras, and Bombay, our science is now in a fair way of being largely communicated to our fellow-subjects in the East. Good must result, directly to them, and indirectly, by reflex influence, to us. The more truth and good principles of practice we scatter, the larger will be the demand for them. Books, instruments, and medicines, will be asked for from Britain to a greatly increased extent. Already, we believe, *men* qualified and willing, might find employment in far larger numbers than they can be got to go. Lastly, many import-

ant additions to our knowledge are likely to be made by people so acute as the Hindoos, when they are once fairly set on the right path of investigation.

Part Third.

PERISCOPE.

PRACTICE OF PHYSIC.

REPORT OF CASES TREATED IN PROFESSOR HEBRA'S CLINIQUE FOR SKIN DISEASES, IN VIENNA. (*Continued from page* 66.)

IV. Psoriasis.—*New mode of treatment.*

28 patients (16 males and 12 females) were treated for this affection. In many of these cases the ordinary method of cure was departed from; vapour baths were employed, and the *huile de cade* was applied to the laminæ, in the manner we have already described for eczema. In nearly every instance this mode of treatment was followed by successful results.

V. Icthyosis.

Two mild cases are reported, occurring in boys who had had the disease from their infancy. Rubbing with *sapo viridis*, and the use of woollen clothing removed the affection.

VI. Prurigo.—*Results; treatment; abnormal case.*

58 men and 17 women were treated for this disease; 29 cases of *prurigo mitis* were cured (24 males and 5 females); 39 cases of *p. formicans* (31 males 8 females) were improved; 2 males and 1 female were dismissed as incurable; and the remainder were in hospital under treatment.

Inunction with *unguentum cinereum* was revived, but with no good effect. The employment of artificial sulphurous baths was found beneficial.

One case was somewhat abnormal. It was that of a man, aged 45, who had considerable infiltration of the skin of the lower extremities, which had lasted between 15 and 20 years. The inguinal glands on both sides were enlarged to the size of the fist. Although such glandular swellings in prurigo generally remain indolent, the tumour on the right side became the seat of acute inflammation, and cicatrised after a period of four weeks. In no other case of prurigo during the year did the glands inflame.

VII. Sycosis.—*Treatment.*

This occurred in 8 men, of whom 6 were cured, and 2 remained under treatment. The therapeutic measures used in all the cases were vapour baths, and inunction three times daily with the *ung. iodid. sulph.* (see formula given in last number of *Monthly Journal*, page 68). Under this treatment the infiltration softens and finally visibly disappears, and a cure is effected without pain, in cases which formerly were only treated by repeated cauterizations with nitrate of silver.

VIII. Favus.—*Sites of disease; trial of a new method of treatment.*

16 males and 6 females were treated for favus. Death took place in the case of one man by the occurrence of gangrene in one-half of his face, which was covered with deep ulcerations of lupus. In 3 cases the favus crusts were found on other parts of the body, besides on the hairy scalp. In two

of these the disease affected the face, and in the third it occurred on the stump of the amputated lower extremity of a little child.

A new method of treatment was tried experimentally, but without any good results. It consisted in first removing the crusts, and thereafter firmly applying strips of adhesive plaster to the diseased parts (which were previously shaved), in order that, by the exclusion of air, the parasitic growth might be arrested.

IX. PEMPHIGUS.—*Peculiar case; P. exfoliativus.*

This occurred in two cases. The first was that of a man, æt. 17, in whom the disease first appeared on both the lower extremities, in the form of from 10 to 15 livid protuberances of the skin, in which, after a few days, an accumulation of serous fluid formed below the epidermis, converting them into tense vesicles. The fluid remained clear for 4 or 5 days, when in most cases it escaped by bursting through the integuments; but in a few instances it became purulent, and afterwards dried, forming hard crusts. There was slight fever for the first few days; but the urine contained no albumen. The most interesting point in this case consists in the bullæ having been formed out of the nodules described, which exactly corresponded to the appearance of *erythema nodosum*. The patient recovered completely.

The second case occurred in a woman, æt. 50, in the form of *P. exfoliativus*. The bullæ became filled with pus, and extended irregularly over the whole body, with the exception of the face. The disease had, as usual, been slow in its progress, having commenced, a year before admission, on the chest, and having thence extended in all directions. This case was under treatment when the report was published.

X. ECTHYMA.—*Site of disease; treatment.*

24 men and 7 women were treated for this disease. The pustules occurred principally on the lower extremities, but in the more severe cases they spread over the trunk and arms. The treatment resorted to was the same as employed in eczema, and its use was attended with results equally satisfactory.

XI. PURPURA.—*Prevention of relapse; mode of treatment.*

Occurred in 8 males; in 7 of these cases the disease was *purpura scorbutica*, and in one case, *purpura rheumatica*. All of them were dismissed cured. In order to guard against a relapse, Hebra considers that it is absolutely necessary in all cases of purpura to preserve the strictest quietness for a few weeks after the disappearance of the hæmorrhagic spots. *Treatment.*—This consisted in the administration of cooling drinks, and the employment of cold epithems, saturated with cold water, or with vinegar and water mixed.

XII. ULCERS.—*Varicose and scrofulous varieties; treatment of each. Trial of olive oil and iodine as a substitute for the ol. jecoris asselli.*

A special ward in the hospital was set aside for the reception of ulcers, and was always filled with cases transferred from other wards.

a. Varicose ulcers.—Among these there were many very severe cases, in which the disease was complicated with caries and necrosis of the tibia. The methods of treatment were diversified, and adapted to the exigencies of each particular case. Minor cases were treated with cold epithems; to those which were more severe, the *sulph. cupri* was applied, either in substance or in solution; and many cases were treated simply by carefully regulated compression to induce healing. In the most aggravated cases the unhealthy purulent surface was first of all destroyed by means of caustic potash.

b. Scrofulous ulcers.—The treatment applied to these was substantially the same as was used for the former class of ulcers; but the administration of cod-liver oil internally was conjoined with it. For some months trial was made of olive oil and iodine, as used in Paris, according to this formula—

℞. Ol. olivarum libr. un.
Iodin. pur. gr. unum—duo. *Misce.*

This substitute, however, was not found to be as efficacious as the ol. jecoris asselli.

A woman, whose scrofulous ulcers were very nearly cicatrised, died suddenly from cardiac disease. At the *post-mortem* there was found to be great atrophy of the heart, with incompetency of the mitral valve.

XIII. Lupus.—*Varieties; general treatment; case with necrosis of frontal bone and medullary carcinoma of the dura mater; case of lupus hypertrophicus fatal by tuberculosis; complication with erysipelas, and with acute laryngitis.*

21 men and 29 women were admitted for this disease; of whom 3 men and 3 women died; and 5 men and 8 women remained under treatment when the report was written. Among these were 6 cases of *lupus serpiginosus;* 32 of *lupus exulcerans;* 7 of *L. hypertrophicus;* and 5 of *L. exfoliativus.*

The treatment consisted in the administration of ʒij-ʒiv. of *ol. jecor. aselli* daily, combined with the external use in most, but not in all, cases of *nitras argenti.* Olive oil was tried as a substitute for the cod-liver oil, on account of its more moderate price, for a period of two months, but its use was not productive of the same beneficial results.

Some cases were possessed of sufficient interest to merit introduction here.

Case I.—Marcus S., æt. 20. Some years ago this patient was in the Imperial Hospital, having on his face, trunk, and upper extremities, scrofulous cicatrices; and having on his neck deeply seated ulcerations. Abscesses formed around each orbit; erysipelatous inflammation of the surrounding textures supervened, and his strength was very much reduced. In fact his case made even a surgeon shudder to look upon it. Two years before admission this lad had typhus, and the following year was attacked with pneumonia. Immediately after this a scrofulous ulcer formed on his brow, above the left orbit; and during the spreading of this ulcer a circular portion of the frontal bone, of the size of a *kreuzer,* exfoliated, whereby the dura mater became exposed. At the point of opening a morbid growth took place, which, protruding through the aperture, enlarged rapidly until a fluctuating tumour was formed, of the size of a child's fist. The tumour became gangrenous, and pre-existing pulmonary tuberculosis simultaneously advanced very rapidly; and, suffering great pain, which was very little soothed by remedies, the poor patient died. *Post mortem.*—A cyst, filled with purulent contents, was found extending from the dura mater into the brain; and, from the external surface of this, a medullary carcinomatous mass had extended outwards, which afterwards became gangrenous. Tuberculous caverns existed in both lungs; and the mucous membrane of the alimentary canal was œdematous.

Case II.—C. B., labourer's daughter, æt. 24. The patient had *lupus hypertrophicus* of the face, and had been under treatment since 1849. The various remedies used were stated in the reports of former years. Latterly $\frac{1}{12}$ gr. of sublimate of morphia[1] has been given internally thrice daily; and a solution of chromic acid in aq. distill. (aa. part. æqual.) has been applied externally, but without any success. Tubercles became developed during the summer in the lungs, and simultaneously infiltrated the lupus, giving a whitish callous appearance to the previously red surface. In this condition the patient sank.

Another remarkable complication was observed in four cases, viz., that of erysipelas, accompanied in all cases with severe fever. This occurred simultaneously in 3 women and 1 man, from the 18th to the 19th October.

Case III.—K. R., æt. 19. In this patient, two days after the supervention of

[1] Muriate of morphia (?)—*Translator.*

the erysipelas, there suddenly occurred violent dyspnœa, and increase of the fever. These were considered indicative of some acute exudation-process in the larynx; for, in the lungs, and other parts of the respiratory apparatus, nothing abnormal was ascertainable which could explain the symptoms present. The patient died the same day, and on the inner surface of the larynx there was discovered a recent croupy exudation; there was œdema of the glottis, and (secondarily) of the lungs.

XIV. SECONDARY SYPHILIS.—*Varieties; connection with primary affections; sites of eruption; time between infection and eruption; types of secondary when contemporary with primary affections;* TREATMENT. *Fatal cases—Autopsies.*

Of the 79 cases treated, 5 were cases of *Syphilis maculosa*, 15 of *s. papulosa*, 10 of *s. squamosa*, 16 of *s. pustulosa*, 27 of *s. ulcerosa*, 2 of *s. vegetans*, and 1 of Syphilitic periostitis.

Local affections of the genitals, of a primary character, were present in 27 instances; they were visible only by cicatrices in 10 cases; and in the majority of the remainder their existence was denied. The maculated, papulous, and pustular varieties of the efflorescence were observed to preserve a greater symmetry and resemblance in their appearance on the two sides of the body, than the other types of the disease. Next to the face, and the flexures of the upper and lower extremities, the efflorescence was most abundant on the sacral region; whence, in some cases, gradually becoming fainter, it radiated towards the groins in a fan-like manner. In one case it was bounded towards the nates by a sharply defined, semi-circular line, which was concave inferiorly.

The shortest space of time between infection and the syphilitic eruption was five weeks.

As regards the connection between the nature of the primary and that of the secondary affections, the following results were obtained:—Of the beforementioned 37 cases where primary affections were present, there were 19 in which secondary syphilis occurred contemporaneously with venereal sores (*schankergeschwüren*), or their visible cicatrices. Of these 19 cases there were 5 papulous, 4 pustular, 8 ulcerous: there was one case of roseola, and one of syphilis vegetans. Simultaneously with condylomata occurred 5 cases of secondary syphilis in all, viz., 1 of *s. roseola*, 2 of *s. squamosa*, 1 of *s. papulosa*, and 1 of *s. pustulosa*. Synchronously occurring with vaginal blenorrhea, without distinct marks of ulceration, but with erosions of the os uteri, there were found 13 cases of syphilitic eruptions, 2 of which were roseolar, 3 papulous, 3 pustular, and 5 of an ulcerous type.

Treatment.—The general treatment differed little from that adopted in former years. The mercurial preparations most used were the *hydrargyri iodidum*, and the *unguentum cinereum*. The preparations of iodine were only used (except in affections of the bones), when salivation by means of the mercury was counterindicated.

Two cases of death occurred among the syphilitic patients. The first was that of a tailor, æt. 25, who had been 5 months under treatment for secondary syphilitic ulcers. After the cicatrization of the ulcers there occurred intense albuminuria, with œdematous swelling of the extremities. In this condition he died, and a few days before his death his abdomen and back became covered with an innumerable crop of boils. At the autopsy there were found osteophytic growths on the inner table of the cranium; numerous callous puckerings (*schwielige einziehungen*) on the surface of the liver (so-called syphilitic cicatrices); and great atrophy of both kidneys. In the second fatal case the syphilis was complicated with degeneration of the kidneys, which gave rise to albuminuria during life. On examination after death one of the kidneys was found to be a mass of Bright's disease, and the other one was filled with purulent abscesses.—*Zeitsch. der Gesellsch. der Aerzte zu Wien.* 1853. *Hft.* 8 and 9.

SURGERY.

MR PAGET ON OPERATIONS IN CASES OF HERNIA WHEN THE SAC AND ITS CONTENTS HAD BEEN REDUCED EN MASSE.

Mr Paget has recorded two cases of inguinal hernia in which the sac and intestine having been reduced *en masse* by the taxis, the symptoms of strangulation continuing, he laid open the inguinal canal and succeeded in reaching the hernial tumour, and relieving the constriction at the mouth of sac; in both cases with successful results.

The best collective evidences for the reduction *en masse* appear to be—1st, The signs of strangulation continuing, or first ensuing, after the apparent reduction of a hernia; 2d, The loss of a hernial sac, more of which, according to the history of the case, ought to be felt in or beyond the inguinal canal; 3d, A feeling of fulness at the internal ring, and of an ill-defined, firm swelling in the abdomen behind or below the ring; 4th, An amount of pain at and about the internal ring, which is quite disproportionate to that in any other part of the abdomen, as well as to the other signs of strangulated hernia; 5th, The unusual drawing up of the testicle. Mr Paget then observes:—

In these particulars, the cases I have related corroborate those already published, and confirm the rule that, when these signs are found coincident, we should act in the belief that the reduction *en masse* has taken place. But the cases presented two rare and deceptive features.

1st, In each of them, the pushing back of the sac was so far incomplete, that the lower portion of it, containing fluid, remained in, and in one case projected a little beyond the inguinal canal. Feeling this portion of the sac, one might have assumed that no more had ever projected beyond the abdomen. But the cases show that we must count, among the indications of this accident, not only the complete disappearance of a hernial sac, but the marked diminution of one; that it may excite a suspicion, if we can feel, in and beyond the inguinal canal, only a much smaller hernial sac than, according to the history of the case, there should be. If, in either of these two cases, the hernia had been properly reduced, its empty sac should have been felt, not only in the inguinal canal, but along the descending portion of the spermatic cord.

2dly, In the second case, the sac had been pushed from its connections five or six weeks before the severe strangulation ensued, and, once in the course of that time, intestine had probably been strangulated in, and returned from, the displaced sac. Such an event has not, I believe, been previously recorded; but a single occurrence of it diminishes the confidence with which we might expect that, in all these cases, there would be the history of a very recent reduction of a hernia.

The incompleteness of the displacement of the sac, to which I just now referred, suggests a rule in operating. If, in the suspicion that a hernia has been reduced *en masse*, the inguinal canal be laid open, and no sac at all be found in it, the probability of an erroneous reduction is increased; but if, instead of no sac, one be found containing fluid, we might be apt to conclude that this is the whole sac, and that the intestine has been duly reduced from it. But the cases show that the part of the sac containing and strangulating intestine may be pushed back from the internal ring, and that another part, containing fluid, may remain in the inguinal canal: they thus establish, as a rule, that, in all such suspicious cases, we must not desist in the operation without being perfectly satisfied that all is right in the abdominal cavity, so far as it can be reached from the inguinal canal. In both the cases here related, after the exposure of part of the sac in the canal, the detection of its displaced but still constricting mouth was impossible, except by such an examination as would have been unjustifiable in an ordinary case. The sac's mouth was only just within reach of the forefinger, and the sensation received

by the outstretched finger touching the intestine in the sac was at first deceptively like to that from intestine in the abdominal cavity.

I would add a remark on the after-treatment of strangulated hernia as illustrated by these cases.

The abstraction of blood by nearly a hundred leeches, in the three days following the operation, in the first case, was intended against what appeared to be an active local inflammation of the peritoneum and adjacent tissues, in consequence of the violence used in the displacement of the sac and in the operation. The giving of wine on the third day after the operation in the second case was dictated by the fear that a peritonitis would presently ensue, against which treatment might be of little avail.

The effects of both these opposite treatments appeared to be good, and they indicated, as many other facts do, that there are at least two forms in which peritonitis ensues after operations for hernia. In one class of cases it appears essentially the local consequence of injury ; in the other, it is also, and rather, the local indication of a general blood-disease. In the one, it is an active local disease, amenable to local treatment ; in the other, it is only one part, one feature, of a constitutional disease of low type, which local treatment can scarcely affect for good.

We may probably compare the cases of the two classes with those of phlebitis after amputations ; of which, some are simple local inflammations of injured veins ; others are inflammations of veins, associated with diseases of many other parts, and declaratory of diseased blood. The parallel would hold in many respects, and, among them, in this, that the peritonitis or phlebitis of local origin and nature ensues very early after the operation, or is continuous (in the case of peritonitis) with that which preceded the operation ; while the disease of constitutional origin, whether peritonitis or phlebitis, comes on later, and with an interval of two, three, or many more days, in which no signs of it may be detected. There are similar differences and correspondences in the earlier and the later erysipelas after operations ; the former is mainly a local disease, the latter is the localised indication of a general disease ; and, as in the corresponding cases of peritonitis and phlebitis, the one is a comparatively trivial, the other a very grave, disorder.

Now, the respective indications of the earlier and the later occurrence of phlebitis and of erysipelas after operations, are, I believe, recognised by many ; and the time that has elapsed between the operation and the access of the disease is often reckoned among the facts determining the plan of treatment. A similar rule should be held concerning operations for hernia. In general, if peritonitis ensue within twenty-four hours after the operation ; or, if it existed before the operation, and continue or is increased after it, it may be assumed to be chiefly a local disease, a simple inflammation of injured parts, which may be treated by local or other bleeding, by abstinence from food and stimulus, and perfect rest ; or which, if it be slight, may be left to spontaneous recovery. If, on the other hand, the peritonitis first manifests itself more than twenty-four hours after the operation, it is probably an indication of a general blood-disease of some low type, for which opium and well-regulated food, or even stimulants, afford the best hope of remedy.

I need hardly say, that this difference in the time of first appearance must not be taken as alone sufficient for the diagnosis of the two forms of peritonitis, or for guidance to practice. I believe only that it may be reckoned among the most useful indications. Neither may twenty-four hours after the operation be fixed as an exact time by which, according to their advent within or beyond it, we might class all the cases of peritonitis ; it is, I believe, a fair general estimate, but nothing more. Nor must the one form of the disease be regarded as exclusively a local, or the other exclusively a constitutional malady ; each is only chiefly the one or the other ; and, in different cases, we may find the constitutional and the local elements of disease prevailing in various proportions.—*Medical Times and Gazette.*

MIDWIFERY.

REPORT OF M. DEPAUL ON THE TREATMENT OF DISPLACEMENTS OF THE UTERUS BY INTRA-UTERINE PESSARIES, AND DISCUSSION AT THE ACADEMIE IMPERIALE DE MEDECINE OF PARIS.

This subject, which has lately created an extraordinary sensation in the Academy of Medicine, was first brought before them by Dr Broca, who detailed a case from his own practice, where death had occurred after the use of the Uterine Catheter. M. Cruveilhier followed up the statement by the relation of another fatal case, and expressed himself as strongly opposed to the use of uterine instruments. A committee, composed of MM. Robert, Huguier, and Depaul, was appointed ; and their report, drawn up by M. Depaul, after entering at great length into the whole subject of uterine displacements, has condemned in most severe and decided terms, the use of the intra-uterine pessaries and bougies. The following is a condensed account of the cases, the report, and of the discussion which followed it.

CASES OF MM. BROCA AND CRUVEILHIER.

Broca's Case.—A woman, æt. 39, who had borne three children, the youngest ten years ago, was admitted to the Hôpital de Lourcine, October 4th, 1853. She was thin, and wasted (flétrie), but her general health was tolerably good, and she stated that she had never had any serious disease. About a year before admission, from which period she dates her illness, the menses became suppressed, and never returned ; at the same time pains in the thighs, disorder of the digestion, habitual and obstinate constipation, and frequent calls to make water came on. She stated that she had got much thinner, and suffered much. On examination, very marked antiversion was detected ; the cervix was enlarged and granular, and a considerable quantity of glairy and purulent matter was discharged from the os. On the 7th October the uterine sound was introduced, it reached to $6\frac{1}{4}$ centimetres. The uterus was easily brought to the normal direction, and kept there for five minutes ; the patient complaining of no pain. On the 8th and 10th the same manœuvre was repeated, and the patient declared herself much relieved. On the 11th, slight pain was felt on the introduction of the instrument, and the patient complained of the hypogastrium when the uterus was replaced in position ; the sound was left in for two or three minutes. The pain became much more intense on the 12th, and was followed soon after by fever, nausea, bilious vomiting, and constipation ; leeches and cataplasms were applied, and on the 15th the fever had ceased and the patient seemed better ; but the vomiting continued, and intermittent pains came on frequently in the abdomen, apparently spreading from the uterus. On the following days the attacks of pain were more frequent, the vomiting became incessant, the pulse small, and the abdomen tympanitic, and the patient died on the 23d, after a long and painful struggle. *Autopsy, twenty-four hours after death.* Skull not opened. Thoracic organs healthy. In the abdomen, old and strong adhesions of the right extremity of the transverse colon, and of the great omentum to the abdominal parietes ; the stomach, duodenum, and jejunum were greatly distended to about 60 centimetres above the ilio-cœcal valve. *At this point the intestine was adherent to the uterus, and became suddenly contracted (retréci); above, it measured* 16 *centimetres in circumference, below, it was reduced to the size of the index finger.* The whole of the large intestine, as far as the descending colon, was strongly retracted (*fortement revenu sur lui-même*) and empty ; beyond this point the calibre was diminished, but not to the same degree. *Below the adhesion, the intestine was discoloured but healthy ; above it, as far as the duodenum, which was sound, there were evident traces of congestion and inflammation.* Independently of these old adhesions, there were recent ones formed by a greyish gelatinous juice, semitransparent, and not yet organised ; in short, there were the evidences of two attacks of peritonitis, one of old standing, the other recent.

The right Fallopian tube was enormously dilated, and connected by old adhesions to the uterus and rectum ; no trace of the right ovary could be found. The cavity of the Fallopian tube contained a considerable quantity of chocolate coloured pus. At the angle of the uterus, near the insertion of the right Fallopian tube, was found a small collection of pus very near the peritoneal surface."

M. Broca supposes that the introduction of the sound caused first metritis, and subsequently peritonitis, and that the recent being added to the old adhesions, had completed the intestinal obstruction. On the other hand M. Valleix contends, that the obstruction pre-existed before the sound was introduced, and was quite independent of it. The pathological appearances are certainly described in a confused manner in M. Broca's report ; at one part it is stated " that there was no fluid in the peritoneum, and no recent false membrane, and that, with the exception of the old adhesions, the peritoneum might be called healthy." And at the end of the description we are told of the exudations of recent lymph (*des adherences formées par un suc gelatineux grisâtre, demistranparent, non encore organisé*), showing recent peritonitis.

Cruveilhier's Case.—A young woman, æt. 24, married for four years, and extremely distressed at having no children, was found to have a slight anteversion of the uterus, to which her barrenness was ascribed. The introduction of the uterine sound having occasioned great pain, she removed to Paris and put herself under the care of M. Valleix. During a residence of a month there, the sound was introduced five times, but could never be retained beyond a few hours, on account of the sickness, distension of the abdomen, and severe pain (angoisses), which required the withdrawal of the instrument. Her health became so much affected, that her mother insisted on giving up the treatment and returning home. On her arrival there, the family physician recognised the symptoms of metro-peritonitis, in an aggravated form. Her condition becoming worse, M. Cruveilhier was sent for ; he found the anteversion still persistent ; an extreme degree of marasmus ; the symptoms of peritonitis, especially the pain of abdomen, had somewhat abated, but the uterus was extremely tender. The pulse was filiform and numbered 120 in the minute. The chest was examined ; some mucous râles existed at the apex of the right lung, but there were no signs of tubercles. A week after the patient died. The examination of the body was not allowed.

M. Valleix is of opinion that the patient was tubercular, and had probably sunk under tubercular peritonitis.

M. DEPAUL'S REPORT.

In commencing his report, M. Depaul undertakes to prove,—1st, That uterine affections, usually attributed to displacement, have an entirely different origin. 2d, That in most of the alleged cases, another more frequent pathological condition, which produces uterine symptoms and even sometimes causes displacement, has been overlooked. 3d, That science possesses a simple and rational treatment for displacements, as efficacious, or more so, than intra-uterine pessaries. 4th, That the facts adduced in favour of the treatment by these instruments, only show their entire inefficiency ; and 5th, That we must take into serious consideration the numerous facts which prove that the most formidable affections and even death itself, may be the consequence of these manœuvres, which are besides, on their first aspect, *repugnant to common sense!* (*repugnent à la raison*).

Instruments.—Before proceeding to these points, the reporter specifies the instruments used in the treatment of uterine displacements, to which allusion is made. These are of two kinds (sufficiently well known in this country), 1. *The Uterine Sound,* of Simpson and Kiwisch ; (Recamier and Amussat had previously used a similar instrument). This instrument, which is of undoubted service in diagnosis, is also used to restore the displaced uterus to the normal

position; but it is never left permanently in the uterus. 2. The *pessaries*, of different kinds which are used for the replacement and permanent support of the uterus, remaining for a considerable time in its cavity. These are known as Simpson's and Kiwisch's, although the original idea of the invention is claimed for Amussat and Velpeau. They are 1st, The wire pessary, or pubic pessary, having a stalk to support the interior of the uterus, in connection with an apparatus resting on the pubis. 2d, The spring pessary. 3d, The ball pessary, with the stalk for introducing it. 4th, The galvanic pessary, made of zinc and copper. 5th, The dilating pessaries, used in obstructive dysmenorrhœa, sterility, etc. The three last instruments consist of a metallic stalk which is passed into the cavity of the womb, and is fixed on an oval disc or ball. Although the use of the uterine sound is not without danger, it is principally against the intra-uterine pessaries that the criticisms of M. Depaul are directed.

Pathology, etc.—The reporter then enters on the first division of his subject: viz., the Pathology of Uterine Displacements. *What is the normal direction of the uterus? What are its displacements? Do these displacements produce the symptoms which have been ascribed to them?* M. Depaul does not agree without reserve with Cruveilhier, and others who maintain that the uterus has no certain direction; and he rejects the opinion of Boulard and Verneuil, that anteflexion is the normal position of the womb. He maintains that however liable to vary from accidental causes, the uterus is placed normally in the direction of the axis of the brim of the pelvis. When the axis of the uterus is more inclined in any one direction, we have either *ante*, *retro*, or *latero-versions*. When the whole uterus is pushed from its position, we have total *displacement* (*refoulement*), which may also take place *forwards*, *backwards*, or *laterally*. When the uterus is bent on itself, *ante*, *retro*, or *latero-flexions*, are produced. There are also, more rarely, *flexions of the neck on the body* of the uterus; and *inflections* of the uterus, where it is curved once, or twice like the letter *S* italics. Another condition which may exist alone, or combined with the preceding, is the *descent* of the uterus (*abaissement*), generally the inferior extremity, but sometimes the middle, or possibly even the upper part of the uterus passing low down into the pelvic cavity. These displacements by descent appear to have been overlooked in France by those who used the "instrumental treatment" ("*traitement mécanique*").

These different displacements are so common, that more than half probably of the female sex are subjects of them, and would require treatment, if these conditions were really diseases. But such is not the case; their influence has been greatly exaggerated, and it may be shown that the *symptoms attributed to these deviations belong to some other pathological condition.* In a critical analysis of M. Valleix's cases of displacement, treated by the intra-uterine pessaries, concomitant disease of the uterus, as hypertrophy and engorgement, or granulations and ulcerations of the cervix, leucorrhœa, etc., are found to have existed in the largest number: in a few, the uterine deviation discovered only after death, had produced no symptoms during life; and in some the morbid symptoms remained after the uterine displacement had been corrected. The cases of M. Gaussail, analysed in the same way, are similarly complicated. The symptoms of all the patients were much alike—viz., uneasiness in walking, pains in the loins, white or sanguineous discharges, constipation, and difficult micturition, and disorder of digestion. Yet all these affections are ascribed by M. Valleix and Gaussail exclusively to the displacement, taking no account of the hypertrophy, ulceration, or other diseased state of the uterus, which existed at the same time. The facts of M. Piachaud (pupil of M. Valleix) are of the same inconclusive stamp as the preceding; but he adds that he has witnessed dangerous (*graves*) hemorrhages, and in two cases perforation of the uterus in consequence of the use of the pessaries!

If in these cases, complicated with other lesions, the symptoms cannot be with certainty referred to the deviation exclusive of the lesions, still less is there evidence to show that the deviations *alone* give rise to serious symptoms

MM. Dubois and Lisfranc declared that displacements were innocuous, unless attended with pathological lesion or chronic inflammation. In a number of cases of displacements of all kinds observed by M. Gosselin, at the Lourcine, none of the women had any uterine complaint whatever; in a number of other cases, where uterine pains existed, there was no displacement, and the pains were owing either to inflammation or neuralgia; and in a third set of cases, where both uterine pains and displacement co-existed, the pains ceased after repose, and the use of antiphlogistics, narcotics, etc., although the displacement persisted. In 27 cases of simple deviation, observed by M. Depaul, only two had any uterine symptoms whatever; the one (anteflexion) had more frequent micturition, and a sense of weight after long walks; the other (descent of uterus) had pains in the inside of the thighs, and a feeling of weight in the pelvis, and that only, after fatiguing exertion. Moreover, in diseased states of the uterus, if the morbid condition of the organ is cured, the painful symptoms entirely disappear; and every day's experience proves, that all the uterine symptoms may be produced by lesions of the organ, unattended by any displacement whatever, to which they could possibly be ascribed. For the truth of the latter statements the reporter appeals to his own, and to the general experience of the profession.

With regard to the influence of the dilating pessaries on sterility, M. Depaul has similar objections, viz.: that in the facts observed no adequate account has been taken of concomitant pathological conditions; and he maintains that displacements and flexions could only cause sterility when of very old standing, where the uterus is almost always atrophied, and dilatation consequently useless.

Admitting, however, that in a *very small number of cases* the mere displacement of the uterus may disorder the health, or may have an injurious influence on co-existing diseased conditions, M. Depaul believes that there exist numerous efficacious methods of treatment which do not compromise the health or life of patients. Of these means, he enumerates, rest, fixation of the uterus (*immobiliser l'uterus*), taking off the weight of the intestines, avoiding tight-lacing, using hypogastric belts, and lastly, the different pessaries (not intra-uterine) particularly that of M. Garriel, composed of caoutchouc distended by air; also stuffing the rectum in cases of retro-version, as practised by M. Huguier. He mentions also the pessaries in whale-bone of Dr Mayer, and that of caoutchouc of M. Joret. He discountenances the use of cauterisations to produce adhesions for the purpose of replacing the uterus.

Cases.—The most interesting inquiry in a practical point, however, is an examination of the *statistics of the treatment by intra-uterine pessaries*. The number of cases published amounts to about 180, and of these, according to M. Valleix, 129 were cured. A result, at first sight, so strongly in favour of the treatment, is, however, completely reversed by the history of the cases themselves. The reporter here gives a short analysis of M. Valleix's cases, twenty in number, of five cases by M. Gaussail, and three by M. Piachaud. With regard to Dr Simpson's practice he was unable to give the results, as that gentleman, when applied to, had, he said, communicated only assertions without proofs; he could only, therefore, give an idea of his success from the information received from indirect sources.

It would be tedious to give the whole of these cases; we subjoin a few to show the method of analysis, and how inconclusive the cases are, or rather how they are turned into evidence against the treatment they were published to recommend.

Valleix's cases.—Fifth case. C., æt. 31, some years after accouchment suffered from hysteria, leucorrhœa, fatigue in walking, and weight in the pelvis, for which she was treated by repeated leeching. Coming under M. V.'s care in 1851, she was pale and anemic; the uterus was voluminous, not painful, lying tranversely forwards. *On the anterior lip, which was red and voluminous, there was a prominence of a deeper colour, presenting in the middle a little white point.* The neck was cauterised on three successive occasions with the acid

nitrate of mercury. Afterwards, the catheterism was practised six times, leaving three or four days interval. At the end of a month the uterus was replaced in its normal situation. *The cauterisation was then resumed and repeated thirteen times during nearly two months.* There was no accident, and the cervix was cured. That cure, ascribed to the uterine sound, ought plainly to be attributed to the cauterisations. Several of the other cases are very similar to this.

Ninth case. Anteflexion with hypertrophy, the uterus being heavy, with little mobility, the sound penetrating seven centi-metres. The intra-uterine pessary (*redresseur*) having occasioned dangerous symptoms, an energetic antiphlogistic treatment was employed, in consequence of which the patient was cured.

Thirteenth case. Retroversion, and chronic metritis with ulcerations and numerous red granulations. The intra-uterine pessary was applied once, and left in for a fortnight; long after this the patient underwent treatment for anemia, neuralgia, and for a fissure of the anus which had been overlooked at first. The cervix was cauterised for several months.

In others of his cases there appears insufficient evidence of the state of the uterus, or even of the cure which is reported to have ensued. M. Gaussail's cases are generally complicated with metritis, and such varied and prolonged methods of treatment were used, that it is difficult to ascribe the cure to any one in particular. His fifth case he concludes by admitting that "*the treatment* (*by intra-uterine pessaries*) *had produced no amelioration,—the patient felt more fatigued in walking, standing, or by simple movement of the arms. The modifications in the position of the uterus were hardly recognisable. The patient and the medical man were convinced that this treatment could no longer be endured.*" M. Piachaud's cases have already been alluded to. In addition, two cases are quoted, observed by M. Gaube, in which all the morbid symptoms were cured, although the use of the pessary failed to replace the uterus.

The last case given is one of anteversion, where the patient, dismissed cured from hospital by M. Valliex, returned next day in as bad a condition as ever. Scanzoni, in Canstatt's Jahresbericht, states that in twenty cases of displacement, where he tried the mechanical treatment, he did not once obtain a permanent cure. It appears, moreover, that M. Valleix has been continually *improving* the intra-uterine pessary, by diminishing the length of the uterine stalk, till at last he has given it up altogether, and uses only the temporary introduction of the uterine sound, but always, he assures us, with the same successful results.

Having examined the statements of the principal partizans of this mechanical treatment in France, viz., MM. Valleix, and Gaussail, etc , M. Depaul proceeds to show that this treatment has no greater favour or success in other countries, particularly Great Britain. In regard to Dr Simpson's practice, he quotes from an eye-witness, that although in many cases the relief is immediate, and in a few instances the instrument can be worn with impunity for months or even years, yet in *the great majority* of *females* some morbid state of the uterus or its appendages, etc., comes on; hemorrhages, rectal fissures, ulcerations of the rectum, metritis, metro-peritonitis, pelvic abscesses were pretty frequent consequences, and more than once death was the final result. Drs Churchill, Ashwell, and Gream's statements are quoted as to the dangers of the treatment, together with Dr Robert Lee's case, where the pessary, or, as he calls it, the impaling machine, was extracted from the uterus. Two fatal cases of peritonitis caused by the pessaries, described by Dr Oldham; his strongly expressed opinions against the practice; the similar views of Montgomery of Dublin, and Matthews Duncan of Edinburgh, are adduced as strong evidence of the unfavourable estimation in which the practice is held by a large number of eminent obstetricians.

The same objections which have been made to the pessaries are also applicable to the uterine sound or catheter (*hysteromètre*), especially when used to replace the uterus in position. According to Depaul, in the vast majority of

cases it is unnecessary even for diagnosis; it may even lead into error, and its use is not exempt from serious dangers. Guéneau de Mussy has recorded a case of fatal peritonitis in consequence of passing the uterine sound; three cases of abortion in the early stage of pregnancy, and one case of abortion and death has occurred in the practice of Huguier, Nonat, and Valleix. M. Depaul then passes under a final review the cases of Valleix, Gaussail, Piachaud, Huguier, Nonat, etc., and finds in nearly every one some of the following results from the use of the intra-uterine instruments: hemorrhages nearly in every case, and sometimes of dangerous amount; uterine and abdominal pains; rigors and fever; syncope and phlegmonous inflammation around the uterus, or in the iliac fossa; metro-peritonitis. A case of enormous phlegmon in the neighbourhood of the uterus is given in detail from M. Cazeaux; one of metro-peritonitis from M. Gaube; six cases (one fatal) from M. Nonat, in none of which was the displacement benefitted by the treatment, which gave rise to the dangerous complications, pelvic abscesses, metro-peritonitis, etc.; and this enumeration is summed up by the fatal cases of MM. Broca and Cruveilhier, which originated the inquiry before the academy; followed by four fatal cases from the practice of MM. Valleix, Nelaton, and Aran, and the testimony of M. Maisonneuve that similar accidents have occurred in his hands.

Conclusion.—From these detailed investigations the reporter draws up the following conclusions as the unanimous opinion of the committee:—

In the great majority of females, uterine displacements do not injure the health, and constitute a mere deformity without importance. Where displacements co-exist with inflammation or neuralgia, when the latter affections are cured, the former disappear, or if they persist are quite innocuous. The small number of simple displacements which occasion inconvenience are easily and safely treated by simple methods without incurring the dangers of the intra-uterine instruments, which, even when they momentarily replace the uterus, fail to fix it in the normal position. The facts adduced to show the efficacy of the mechanical treatment have been wrongly interpreted, and the cures ascribed to it have been owing to other means employed at the same time. The fatal cases observed both in France and England are numerous enough to show the perils of this treatment, and to warn its boldest partisans. In addition to the mortality, the other dangers are of the most alarming kind; pains, sometimes agonising, hemorrhages, anemia and nervous disorders, rigors, fever, syncope, peritonitis, pelvic abscesses, metro-peritonitis, etc., not to mention the cases where the treatment could not be endured. The uterine sound, of great advantage in the diagnosis of certain affections, is very often of no service, and from the great risks attending its use, should be reserved for the exceptional cases which require it; and, finally, that the different intra-uterine pessaries ought to be proscribed, because they are useless, and impotent to produce the good effects expected from them, and they subject the patients to the most serious dangers.—*Bulletin de l'Académie*, 31st May 1854.

DISCUSSION ON M. DEPAUL'S REPORT.

After some remarks from M. PIORRY, the discussion was opened by M. MALGAIGNE, who, while admitting the zeal and talent of the reporter, regretted to find so much party spirit in the report, which more resembled the impassioned argument of a special pleader, than a plain and impartial judgment of facts. He could not consider as *repugnant to common sense* a treatment, which had been practised by Velpeau, Amussat, Simpson, Huguier, and Valleix. It appeared even from the report that in some cases, the intra-uterine pessary had been useful, and sometimes it had procured instantaneous relief, showing that the symptoms were really owing to displacement, and he had often witnessed cases of descent of the uterus, where the use of a pessary had put a stop to all the symptoms; the hypogastric belt also, by replacing the uterus, had often produced the same beneficial results. It appeared to him, that the important inquiry was to establish a differential diagnosis of uterine affections; since the

same symptoms were ascribed by some exclusively to displacements, by others exclusively to enlargement (*engorgement*), by others to granulations, ulceration, etc., etc. In neuralgia of the uterus, the pessary, he thought, might certainly be serviceable. In short, the intra-uterine pessaries ought not to be entirely rejected, but their use should be restricted to cases without inflammation, acute or chronic, the instrument should be employed cautiously, and not be allowed to remain permanently; and, above all, the treatment should be controlled according to rational indications; the practitioner should know *what he does and why he does it.*

M. Depaul, in reply, stated, that by the expression repugnant to reason, he meant no more than that *à priori* considerations were opposed to this method of treatment. He defended the report against the objections of M. Malgaigne, and answered some statements which M. Gaussail had addressed to the Academy; he maintained the impartiality and justice of his criticisms.

M. Huguier, who had not signed the report in consequence of differing from M. Depaul in some of his conclusions, made a full and able statement of his views. With regard to the intra-uterine pessaries, he thought that they ought not to be entirely rejected; but that their use should be confined to those exceptional cases of displacement giving rise to serious symptoms, which resist other means of treatment, that their application should be made with great caution and carefully watched. He especially commends their efficacy as emmenagogues, when other means have failed. On the pathological question, he explained the circumstances under which uterine displacements occasion no symptoms; and after distinguishing the local and sympathetic affections which are occasioned by inflammations, etc., of the uterus, he defines those symptoms which properly belong to and are occasioned by uterine displacement, particularly disorder of the menstruation, and distressing affections of the bladder and rectum (frequent micturition, retention, obstruction of the rectum, etc.) He considers that deviations, especially ante and retro-flexion, may occasion sterility; and maintains that the displacements are undoubtedly curable. In conclusion, M. Huguier relates his experience of the general and local treatment of the displacements by pessaries, etc., and describes the methods by which he has succeeded in replacing the uterus when retro-flexed by the insertion of pledgets (mêche) of lint in the rectum.

M. Hervez de Chegoin was of opinion, that the displacements may occasion most distressing symptoms, which are only to be relieved by replacing the organ. But this result ought to be accomplished by vaginal pessaries external to the uterine cavity. The intra-uterine pessaries had produced so many serious accidents, that he was surprised any one could follow a method attended with such risks. He considered that they ought to be altogether dismissed from practice.

M. P. Dubois denied that the vaginal pessaries recommended by M. Chégoin, could ever effectually replace a retroverted uterus; consequently, he considered the intra-uterine instruments to be perfectly rational, and indeed, the only means capable of effecting the end in view. The only question was are they without danger? It is certain that their introduction frequently gives rise to severe, sometimes even fatal consequences; sometimes the instruments cannot be tolerated; while, in some cases, they are quite innocuous, and occasionally their use is attended with immediate, or even permanent, relief. M. Dubois, however, maintains that in no instance do they ever attain their proposed object; the uterus is never replaced permanently in position by their use. This assertion he supports both on theoretical grounds, and by the result of his examination of cases treated by himself, by Valleix, and by Simpson. Although, however, the displacement is never removed by this method, the uterine symptoms may be alleviated or even cured by it. These results are owing to the pessary relieving the weight of the uterus, and the tension of its ligaments; to the alteration in the morbid action by contact when the uterus

is inflamed, and to the alteration of its morbid sensibility in cases of neuralgia. In the latter class of cases the method is of particular service, and often relieves all the symptoms, even where no deviation had existed. Many of M. Valleix's cures occurred in cases of neuralgia. Consequently, the intra-uterine pessaries ought to be retained in practice, not as directors of the position of the uterus, but as modifying agents which may be placed in competition with other efficacious means already known as such, cauterisations, douches, etc. And in all cases, the general treatment should be attended to as well as the local, a principal condition to which M. Valleix owes much of his success.

M. CAZEAUX expressed his opinions at great length. He objected to the report for stating the unfortunate and fatal cases, and omitting the successful ones. He admitted that by removing the displacements, both the symptoms which it occasions, and even the concomitant lesions, granulations, etc., may be cured. In opposition to M. Dubois, he maintained the efficacy of the (M. Chégoin's) vaginal pessaries in retroversion; and he considered inflexion to be a congenital condition in the child, adopting the opinion of Boulard, Follin, and Verneuil. He is opposed to the intra-uterine pessary, as mostly useless and often dangerous, but he considered that the uterine sound, with proper precautions, may be rendered safe, and is of real service.

M. GIBERT maintained that too much importance had been attached to uterine diseases altogether, and great illusions existed in regard to them; that they often depended on constitutional states; and he mentioned instances of several diseases of the uterus, where erroneous and overstrained views of their pathological consequence had led to methods of treatment as severe and dangerous as they were useless and uncalled for.

M. VELPEAU, in one of the most important addresses delivered in the discussion, entered at considerable length into the details of the question, and opposed in decided terms, the conclusions of M. Depaul's report. He passed a high eulogium on M. Valleix, and attached great importance to the results of his practice. He maintained that practitioners have not exaggerated the effects of uterine displacements, nor have they attributed to them those affections which resulted from other pathological conditions. Wherever disease of the uterus, enlargement or engorgement (if not mistaken for anteflexion, as has often happened), or ulceration, or leucorrhœa occurred, no practitioners could overlook these conditions or confound them with the effects of simple uterine displacement. And, although there may be cases of uterine displacement, without urgent symptoms, just as there is hardly any important organic disease the symptoms of which may not be latent, still no one could doubt the serious affections which may be caused solely by uterine displacements, and may be removed by replacing the uterus. M. Velpeau was convinced, that although these deviations yielded, in general, a favourable prognosis, yet that they produced, especially in nervous women, affections of real danger. M. Velpeau next examined the means of remedying these affections, enumerated by M. Depaul and others; and while, recognising their usefulness in certain cases, he was quite persuaded of their radical inefficiency. In consequence of this conviction, he had himself long ago attempted the use of intra-uterine pessaries, but had desisted after the trial of a few months, being deterred more by theoretical considerations, than by the results of practice. He now thought that the dangers of these pessaries had been exaggerated, and might be avoided by more careful and experienced manipulation. The intra-uterine pessaries had not indeed solved the difficulties in the radical treatment of uterine displacements, but they had already rendered great services, and would with further experience render more; they ought not to be rejected, but their employment ought to be regulated judiciously; and he trusted to the experience of M. Valleix to disclose the advantages and the disadvantages which belonged to this method of treatment. M. Velpeau was of opinion that the conclusions of the report ought to be suppressed, and that the Academy could only vote in reference to the cases brought forward by MM. Broca and Cruveilhier.

M. Depaul explained that the conclusions already given were merely the summing up of his report, and that he would, after the discussion was finished, submit to the Academy a series of conclusions embodying the unanimous opinion of the committee.

[The discussion is not yet terminated, but we shall conclude the subject, and, we trust, with a commentary by Professor Simpson, in our next number.]

BRAUN ON ECLAMPSIA DURING PREGNANCY.

This author has recently published a lengthy paper on the above interesting subject, of which the following is a very brief abstract :—

1*st*, Convulsions may arise, during this period, from hysteria, epilepsy, cerebral diseases, poisons, or uraemia,—resulting from Bright's disease.

2*d*, Their most common causes are uraemia and Bright's disease.

3*d*, The least frequent causes are primary cerebral diseases ; and when these occur in connection with albuminous nephritis, they are the *results*, not the causes, of the convulsions.

4*th*, Hysteria and epilepsy may exist, during gestation, in a chronic form, so as to exercise no injurious influence either on pregnancy or labour, or on the life of the child ; and they may be quite unconnected with Bright's disease.

5*th*, Convulsions in every form may occur from these causes in the unimpregnated female ; and also in men, from all of them, except hysteria.

6*th*, The altered constitution of the blood, and the detention of the venous blood in the kidneys by the pressure of the enlarged uterus, are most commonly the causes of the Morbus Brightii, which occurs during pregnancy.

7*th*, Convulsions are caused by the urea, which is uneliminated from the blood on account of the renal disease, becoming changed into the carbonate of ammonia.

8*th*, When, in cases of Bright's disease during pregnancy, we find carbonate of ammonia in the blood, we may prognosticate the occurrence of convulsions ; but when, at this time, the urea in the blood exists merely in small quantities, or chemically unchanged, we need not dread eclampsia.

9*th*, Parturition and uterine irritation neither cause this chemical transformation, nor occasion uraemic eclampsia.

10*th*, The abortions which so frequently happen during uraemic convulsions, are the results, not the causes of the eclampsia.

11*th*, There is no connection between eclampsia and labour-pains.

12*th*, Albuminuria does not result from convulsions arising from functional interruptions, and does not generally occur in those of an epileptical and hysterical character.

13*th*, Albuminuria continues throughout pregnancy, although the eclamptic attacks may have ceased : but when convulsions cease *after labour*, it soon disappears, provided the renal disease be only in the incipient stage.

14*th*, The disappearance of the albuminuria after parturition, is principally due to the diminished volume of the uterus.

15*th*, Morbus Brightii (without convulsions) may be palliated, but not removed, during pregnancy, although it readily yields to remedies after parturition.

16*th*, Albuminuria occurs in all cases of eclampsia which do not depend upon hysteria, epilepsy, primary cerebral diseases, or poisons.

17*th*, Epileptic convulsions may occur simultaneously with those from Bright's disease and uraemia.

18*th*, Uraemic convulsions, when frequently recurring, occasion the death of the fœtus ; but its life is not endangered by those from hysteria or epilepsy.

19*th*, During an attack of uraemic eclampsia, reflex sensibility is almost wholly suspended ; and after it we oftener find ædema and anaemia of the brain, than hyperaemia and consecutive apoplexy before it.

20*th*, Venesection (according to Kiwisch, Litzman, Sedgwick, Blot, and

King) is injurious in eclampsia ; and the author has found its action very uncertain in uraemia. He considers inhalations of chloroform to be the best and safest means we possess for subduing and removing uraemic convulsions, both during pregnancy and after labour.

21*st*, The most certain diuretics for removing the uraemia of Bright's disease are the benzoic, tartaric, and citric acids.

22*d*, The artificial induction of premature labour diminishes the danger of uraemic eclampsia alike to the mother and child ; but this practice should not be universally resorted to in cases of Bright's disease during pregnancy, but only when necessitated by the occurrence of convulsions. Braun considers the tampon as the best method for its induction when necessary.

[The treatment here proposed appears to us by no means universally applicable. Where the convulsions occur during pregnancy, or in an anaemic patient, or where, as happens in infantile eclampsia, they depend rather upon a superpolarity of the cerebro-spinal system than on actual toxaemia, we consider the inhalation of chloroform as unquestionably the best treatment for arresting the paroxysms. But when, on the other hand, they occur during parturition, or in the puerperal state, in a robust, plethoric patient, we should be inclined, from all we have seen of the disease, to place more confidence in copious venesection, smart purgatives, and cold to the head.

Moreover, for the induction of premature labour in such cases, we should be inclined to prefer the use of sponge-tents and uterine douches to the clumsier method of the tampon.]

DRS PRICHARD'S AND PAGAN'S CASES OF DIFFICULT LABOUR ARISING FROM EXTENSIVE CONTRACTION OF THE VAGINA AND OS TINCÆ.

On the evening of the 13th October 1852, I was requested to visit Mrs Branagan, æt. 25, residing in Partick. I found her considerably emaciated, and suffering from occasional uterine pains, though not of an expulsive character. She mentioned to me that she had just completed her seventh month of pregnancy, and expressed much fear and anxiety about her state. I enjoined the recumbent posture, and ordered her an opiate.

October 14*th*.—Had a quiet night, bowels opened, urine passed freely. Uterine pains continue. *The opiate to be repeated.*

11 *o'Clock* P.M.—Since last visit has been suffering from severe expulsive pains at regular intervals ; complains of debility and nausea. Pulse 120, skin hot, thirst urgent. When examined, *per vaginam*, the passage was found contracted from cicatrices, to such an extent as scarcely to allow the introduction of a finger. The os tincæ was very rigid and partly dilated, the cervix being bound to the posterior wall of the vagina, by a broad cartilaginous band. The head of the child was readily felt by the introduction of a finger into the anus. Expulsive pains continuing at regular intervals, without producing any dilatation, and the patient losing strength, I determined upon instrumental delivery, and requested the assistance of my friend Professor Pagan. A firm band, about two inches broad, perfectly undilatable, extended across the posterior wall of the pelvis. To this, what remained of the vaginal portion of the uterus (its anterior lip) was attached. Dr Pagan introduced a finger into the rectum, and with a blunt pointed bistoury cut the band as deeply as safety would permit Very little space was gained by this incision, and four others were made, two on either side of the pelvis. A considerable amount of blood escaped. In consequence of the attachment of the anterior wall of the cervix uteri to the cicatrices, comparatively little space was gained by these incisions, and Dr Pagan determined, therefore, to slit up the anterior lip. Having done this, he was able to pass a couple of fingers into the vagina, and by them to guide the blades of the forceps along the sides of the child's head, when, applying very considerable extracting efforts, he fairly tore the head through the contracted vagina. The child, a female, was born alive. A little warm spirits and water were given the mother, and she was left at four A.M.

October 15th, 11 *o'Clock* A.M. Has had some sleep. Pulse 120; skin moist; lochial discharge moderate; no thirst; has passed urine. Child well, and has taken some milk and water.

Evening.—Pulse 120; skin cool; tongue moist; ordered an opiate.

She continued to improve, although slowly, it being a month before she was able to be out of bed, and nearly another month before she was quite recovered. The incisions slowly healed, the vagina remaining much contracted. There was no secretion of milk.

On inquiring into the history of her previous confinement, I found that she had been delivered on 25th December 1847, of a still-born child, after a severe labour of three days. The perineum had been lacerated, and she suffered great pain, and laboured under *stillicidium urinæ* for upwards of two months after her delivery. It is probable that there had been much sloughing of the vagina, which would necessarily be followed by the great contraction described as existing at the time of her second confinement.

Note by Dr Pagan.—Since Dr Prichard's case, one nearly similar occurred among the poor women attended from the hospital. The patient was in labour of her second child when seen by me, and believed herself to be between the sixth and seventh month of pregnancy. Upon introducing the finger within the orifice of the vagina, a perfectly undilatable falciform band extended entirely across the pelvis, merely permitting the introduction of the finger between this and the anterior wall of the pelvis. The breech of the child, which was presenting, and which was at first mistaken for the os and cervix uteri, was directed against this aperture, but even under vigorous pains no progress had been made in the dilatation of this preternatural band, during many hours continuance. Free incisions were made, as in Dr Prichard's case, and these permitted the expulsion of a still-born premature child. The patient made a perfectly good recovery.

This woman was delivered by the perforator, after being long in labour of her first child. She was the victim of a vesico-vaginal fistula, and no doubt the cartilaginous-like band, which occupied so large a space in the pelvis in her second labour, had replaced the natural structures destroyed by sloughing after her first.—*Glasgow Medical Journal.*

[We highly approve of Dr Pagan's practice in these cases, but have had some difficulty from Dr Prichard's description in understanding the nature of the lesion, and how the first incisions were made.]

DR DIEZ ON STRICTURE OF THE FUNDUS UTERI AS A CAUSE OF DANGEROUS HÆMORRHAGE.

Under this new title the author alludes to a description of hæmorrhage that most experienced practitioners are familiar with as arising from partial contraction of the uterus. After the separation of the placenta, and while globular contraction of the uterus can be felt above the pubes, alarming hæmorrhage comes on. As there is good contraction of the uterus, and pains, often of considerable severity, occur, this is at first inexplicable. On passing the hand through the widely-opened os, we enter a tolerably spacious cavity filled with coagula, whose walls are firm and unyielding, and remain quite passive to the pressure of the hand. Pushing on further, we perceive a more or less narrow, often cartilage-like stricture, separating the fundus from the body of the uterus. It is exactly the same as is observed in strong incarceration of the placenta, only, as a general rule, the contracted part is inclined to one side (usually the right), and takes on a more oval form.

The means calculated to check hæmorrhage under other circumstances are here useless or mischievous. Increasing the pains by means of ergot, cold, external pressure, friction, etc., does injury by increasing the abnormal contraction, and by favouring the flow of blood from the non-contracted part. Of a more useful character are such means as tend to arrest hæmorrhage without exciting pain, as compression of the aorta (taking care, however, not to com-

press the fundus uteri), acids, alum, cinnamon, ipecacuanha, warm astringent injections, etc. The most efficacious means, however, is the attacking the pathological condition upon which the hæmorrhage depends, by the employment of belladonna, in conjunction with, or in quick succession with, ipecacuanha, together with compression of the aorta. When, on account of pressing danger, the operation of these means connot be waited for, the painful and often difficult procedure of dilating the strictured part must be resorted to, allowing the hand to remain within the fundus until the organ contracts in a normal manner.—*Monatsbericht für Geburtskünde*, Band ii. p. 1.

[The use of remedies such as belladonna and ipecacuan is too feeble a measure against any dangerous form of hæmorrhage after delivery. The use of the ordinary means for exciting the relaxed portions of uterus to contraction will, of course, be useful. Of these cold frictions, ergot should certainly be resorted to in the first instance.]

PROFESSOR HOHL'S CASE OF DOUBLE UTERUS, WITH TWINS, AND PLACENTA PRAEVIA.

A delicate woman, aged 30, was subject, during the first three months of her second pregnancy, to periodically recurring attacks of uterine hemorrhage, which, although easily checked at that time, returned more violently in the seventh month. At this period her abdomen was found very much distended at both sides, but level in the central region extending from the umbilicus to the symphysis pubis. Percussion yielded a tympanitic sound in this hollow, and a dull sound on either side. An inch and a half above the pubis the uterus could be felt distinctly, through the parietes, dividing into two parts, of which the one on the right side was largest. Both were convex on their internal, and somewhat concave on their external surfaces; and each resembled in appearance the normal gravid uterus at the full time. In each, the fœtal heart could be distinctly heard on auscultation, and the form of a child easily felt by external tactile examination. On internal exploration, the vagina was found quite normal; and at its roof the cervix was felt, short and broad, and having two ora uteri, through both of which the presenting parts of the children could be distinguished. Over the right os uteri was situated one of the placentæ, and a portion of the other projected from the left os. On account of the violent uterine hemorrhage, Hohl induced premature labour, and delivered the children by turning. The right placenta was spontaneously detached, but that on the left side adhered so firmly, that it had to be artificially separated. The twins, who weighed three lbs., died almost immediately after their birth. Subsequent examinations of the uterus, with the sound and the finger, confirmed the correctness of the original diagnosis.—*Deutsche Klinik. I.* 1853.

[This is one of the most singular cases on record. Lately the medical journals have contained notes of two cases of pregnancy occurring in a rudimentary uterine horn, described by Rokitansky and Scanzoni. The women died from rupture of the horn (just as happens in cases of tubal pregnancy), and the cases were subjected to post mortem examination and verification.]

PROFESSOR LANGENBECK'S CASE OF SUCCESSFUL OVARIOTOMY.

An unmarried female, æt. 34, had, for five years, had a gradually enlarging ovarian tumour, which at length attained so great a size that the abdomen was as distended as it is at the end of pregnancy. This tumour extended from the pelvis to below the false ribs; it was easily moveable, and was distinctly fluctuating to the touch. At the desire of the patient, and under the influence of chloroform, the operation of ovariotomy was performed. A finger's breadth above the symphysis pubis, an incision of $2\frac{1}{4}$ inches was made through the integuments of the abdomen, nearly in the line of the linea alba. The cyst, being seized with hooks, was first punctured by a trocar, which evacuated nearly 9

quarts of clear limpid fluid, and thereafter it was pulled out until the pedicle of the tumour was completely exposed. The abdominal incision was closed with six sutures; strong double ligatures were then passed through and tied round the neck of the tumour; and next, a ligature was passed both through the pedicle and the lips of the wound. Finally, the tumour was removed by cutting through its stalk, immediately above the ligatures. The tumour presented one very large cyst; on the extirpated ovary there were two smaller cysts, the size of hazel-nuts, and the Fallopian tube was adherent by its fimbriated extremity. The evacuated fluid contained merely a few epithelial cells, and it coagulated readily with heat. The wound cicatrized in nine weeks, and during the healing process the patient was considerably troubled with colic pains.—*Deutsche Klinik.* No. 4. 1853.

TOXICOLOGY.

CASE OF ATTEMPTED POISONING BY COPPER. BY M. A. CHEVALIER.

Pierre Brunet had been married for five years to Madeleine Buges. For nearly four years they lived happily together, but after that time Brunet appeared to have lost all affection for his wife; he worked as a miller, and came very seldom to see her—indeed only five or six times in fifteen months. On the 26th of March 1853, Brunet came to see his wife, and, contrary to his usual habit, remained all night. In the morning the wife went out to wash some clothes, leaving her husband alone in the house. Brunet left the house about ten o'clock, saying that he would return in the evening. He did not do so, however; and his wife, having waited for a long time, at last began to prepare her supper. For this purpose she took out of the cupboard the remains of a plate of haricot beans which had been served up the day before. She remarked that the beans had a curious colour, being much darker than before. She heated them, however, but had only tasted them twice, when she felt a painful sensation in the throat; and although she did not again attempt to eat them, she suffered from sore throat for several days afterwards. She immediately suspected that her husband had attempted to poison her. Next day she told several persons what had happened, and showed them the beans; every one observed their singular colour. Three or four days afterwards, Brunet returned to his wife's house; she showed him the beans, asked him what he had mixt with them, and inquired whether he would have any objection to eat them himself. Brunet answered, that if there was nothing among them but what he had put, he would eat them next morning for breakfast. Before he got up next morning, his wife threw the contents of the dish into the garden.

The authorities, however, soon got notice of what had passed, and officers were sent, who collected the beans (which had been buried), and the earth which surrounded them, as well as a napkin, on which were stains, which appeared to have been caused by the beans.

On prosecuting the inquiry, it was found that a young man had seen in Brunet's possession a small box full of greenish-blue powder. This witness also stated that a little of the powder had dropped on the stone floor and stained it. He showed the mark to the magistrates, who had the stone in question lifted. All the different articles were put into the hands of M. Chevalier and of M. Lassaigne, for chemical examination.

They first carefully examined some of the earth in which the beans had been buried. A few pieces of beans and some entire ones were found in it. It was observed that the interior of the haricots was stained of a pale green colour; there were also a few greenish particles mixt with the mass of earth; and on their being picked out and touched with a solution of perrocyanide of potassium, they became of a violet red colour, indicating, therefore, that these were particles of salt of copper. About six ounces of the earth were now mixt with eight ounces of distilled water, and placed in a clean porcelain capsule; rather more than two

ounces of pure sulphuric acid was then added. Considerable effervescence was produced, due to the presence in the earth of carbonates and organic matter. The black residue was moistened with nitric acid, and heated so as to get rid of the excess of acid; it was then treated with boiling distilled water. The resulting fluid, being filtered, was of a rather dark-brown colour; it was divided into several portions, and treated as follows:—1. A portion was placed in a Marsh's apparatus, but no stains were produced, thus proving the absence of arsenic and antimony. This experiment was performed because two green pigments (Scheele's green and Scheveinfurt green) partake of the properties both of copper and arsenic. 2. A stream of sulphuretted hydrogen was passed through another portion of the liquid. A brown precipitate was produced. 3. A small piece of bright iron being put into a little of the same liquid, in a quarter of an hour was covered with a red coating of metallic copper. 4. The rest of the liquid was evaporated to dryness. The residue was placed in contact with an aqueous solution of ammonia; the mixture assumed the characteristic blue colour, indicative of the presence of copper. All these experiments proved that the earth contained neither arsenic nor antimony, but a small quantity of a salt of copper.

The beans themselves were next analysed. For this purpose they were placed in a new porcelain crucible, which was submitted to heat until the beans were carbonised. The ashes were then put in a tube, and treated with ammonia. After some hours the ammonia had assumed the blue colour, indicative of the presence of a salt of copper.

The stains on the stone floor were then examined, and their colour proved to depend upon a salt of copper; these stains were almost insoluble in water, thus showing that the salt was not the sulphate. The napkin was found to have several greenish spots upon it; these were touched with a solution of ferrocyanide of potassium, when they immediately became of a reddish colour, from the formation of a cyanide of copper.

It now became a question whether the earth in question might not contain copper as a normal ingredient, as it is well known that various soils do. Three portions of the earth from different parts of Brunet's garden were accordingly sent for analysis, when it was found that in one no copper could be detected, while in the two others very minute traces were recognised. The amount of copper present was, however, so small, that it could not be confounded with the more considerable quantity of the metal found in the earth among which the beans had been buried.

In order to prove that the copper contained in the beans had not been derived from this source, some beans were mixt up with portions of earth from Brunet's garden, moistened with distilled water, and exposed to the air for four days. At the end of this time they were carefully examined, but not a trace of copper could be detected.

The accused, who for a long time maintained his innocence, being at last overcome by the proofs which were brought against him, confessed that he had mixt something with the beans, with the view of poisoning his wife, but that the substance he had employed was the powder scraped off from the ends of a number of lucifer-matches. Some of the powder obtained in this way was analysed by M.M. Chevalier and Lassaigne, who proved that the blue colour depended on the presence of Prussian blue, and that it did not contain a trace of copper.

The result of these analyses was, that a salt of copper had been mixed with the beans, and that the salt was in all probability verdigris (subacetate of copper.)

Brunet was found guilty by the jury, and sentenced to the galleys for life.—*Annales d'Hygéne et de Médecine légale.* January 1854.

CASE OF POISONING BY DIGITALINE. BY DR ACHILLE CHEREAU.

Madame X——, thirty-two years of age, had suffered for several years from palpitations of the heart, supposed to depend upon dilatation of the origin of the aorta, but had been much relieved by the use of digitalis, administered at

first in syrup, afterwards in powder. The remedy at last became so disagreeable to the patient, that she was on the point of abandoning it. Digitaline was then substituted for the crude drug, and was administered, mixt up with sugar, in the form of granules, each granule containing one milligrame (about $\frac{1}{60}$ of a grain) of digitaline. (According to Trousseau, this quantity is equal in efficacy to about $1\frac{2}{3}$ grains of the powder of digitalis).

About six o'clock on the evening of the 31st December 1853, Madame X—— having become much excited during a quarrel with her husband, seized the phial in which the granules were contained, and before she could be prevented, swallowed its whole contents. The exact number of granules could not be ascertained, but could not, it is stated, have been smaller than 40 (equivalent to $\frac{2}{3}$ of a grain of digitaline.) The lady's husband immediately ran to procure medical assistance, and within half an hour of the occurrence, Dr Chereau was on the spot, and immediately administered $2\frac{1}{2}$ grains of tartar emetic. Vomiting was not produced within an hour, although nearly a quart of tepid water was drank. At last the patient pushed her finger back into the pharynx, and vomiting was very speedily induced. It should be mentioned, that the patient had dined an hour before swallowing the poison. Unfortunately the vomited matters were thrown away, contrary to the doctor's express injunctions. Dr Chereau's next object was to obviate the bad effects which might be produced, by any of the digitaline which had been retained in the system. For this purpose he ordered the patient to swallow every half hour, a cup of strong coffee, containing two table spoonfuls of brandy. At half-past nine o'clock three cups of coffee, and three ounces of brandy had been taken, and nothing particular was observable in the condition of the patient. The same treatment was ordered to be continued during the night; when seen the following morning she was in a state of considerable depression,—heart's action scarcely perceptible, pulse 52, respiration slow, regular, but very feeble, skin cold, pupils of natural size, and quite contractile. On being spoken to, she was readily roused, and stated that she suffered from violent headache, with a sense of impending suffocation. Dr Chereau ordered the application of sinapisms, and the continued administration of brandy, with a view to combat the action of the digitaline, by the contrary effects of the alcohol. At four in the afternoon the lady's husband came in a state of great agitation, to inform the doctor that his wife had been delirious for two hours—that she was talking nonsense—was desirous of getting up, and could not distinguish objects. Dr Chereau found, on his arrival, that the pulse had risen to 72, and was fuller and firmer, that the skin was warm, the face flushed, and the eyes injected,—in short, his patient was intoxicated! At eight o'clock Madame X—— fell asleep, and the following morning all disagreeable symptoms had disappeared.—*Union Médicale*, January 10, 1854.

[The above observation is in many respects unsatisfactory, still, from the rarity of cases of poisoning by digitaline, we have thought it right to lay it before our readers. If the quantity swallowed were really such as is stated, it is singular that no bad effects showed themselves earlier, as the symptoms generally come on rapidly, when the dose taken has been large; and, according to Taylor, a dose of more than $\frac{1}{16}$ of a grain could not be taken by an adult, without causing symptoms of poisoning.—(On Poisons, p. 749.) In this case the woman did not vomit for more than an hour and a half, and yet no alarming symptoms had made their appearance. This case shows the impropriety of trusting to tartar emetic for the evacuation of the stomach, in cases of poisoning. Sulphate of zinc is far superior to it, in the certainty of its action. Dr Christison mentions a case of poisoning, by opium, where a scruple of tartar emetic failed to produce vomiting, but where sulphate of zinc was immediately efficacious.—(On Poisons, p. 776.) Stimulation appears to have been carried by Dr Chereau to a quite uncalled-for extent. The symptoms of poisoning by digitaline were never very well marked; as, although there was some depression of the heart's action,

along with coldness of the surface, and a slight feeling of suffocation, the pupils were neither dilated nor insensible, and there was an almost total absence both of nervous and inflammatory symptoms. Moderate stimulation (succeeding of course the action of emetics), with the application of warmth to the surface, constitute the most suitable treatment. Some have also recommended the administration of astringent vegetable infusions, as it is stated, that the tannin they contain renders the digitaline insoluble].

Part Fourth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING IX.—*March* 11, 1854.

THE BLOOD IN A CASE OF CHOLERA.

Report on the bodies exhibited to the Physiological Society, February 11, 1854, in the blood and some of the organs of a patient dying of cholera. (See an account of the case by Dr Cowan, "Monthly Journal," March 1854, p. 248.)

The bodies in question, when examined with a power of 250 diameter linear, were mostly oval, but sometimes circular, the long diameter averaged 1-100th, 1-80th min.; the short diameter, 1-150th, 1-100th min. When seen rolling over, they appeared as flattened spheriods or ellipsoids, sometimes with a projection at one side, corresponding to the position of an internal granule or nucleus, to be hereafter described. Occasionally they were somewhat angular, and, in a few instances, fusiform or pyriform. In still rarer examples, the bodies were prolonged at one end into a projection resembling a cilium, much attenuated at the extremity, and distended at the base. The regular form which prevailed in the vast majority, was the flattened ellipsoid. There was manifestly a very delicate and transparent limiting membrane, apparently quite homogeneous. Attached to this were one, sometimes two, rarely three, minute granules or nuclei, with an extremely distinct outline circular or spherical, and not exceeding 1-1000th min. in diameter. These nuclei, (?) when more than one, were generally placed at distant points of the circumference of the bodies.

In colour, the bodies in question were faintly yellowish. When compared with the red blood corpuscles they appeared pale.

On adding water, even in considerable quantity, the bodies in question were nearly unaltered—perhaps, rendered somewhat paler in regard to colour. The contour and the nucleus remained quite distinct. When this observation was made in the blood, the ordinary red blood corpuscles, which were in considerable numbers and apparently unaltered, were seen to disappear in the usual manner, leaving the bodies in question unchanged.

Syrup and other dense re-agents corrugated the wall, and thereby rendered the nucleus undistinguishable.

Acetic acid, when added in minute quantity, rendered the whole external wall and nucleus paler, the former more than the latter. Even with moderately strong acetic acid, they did not at once disappear, but with concentrated acid in large quantity, they dissolved entirely.

The above observations lead to the consideration of the following questions, viz. :—1*st*, What is the relation of these bodies to the blood and tissues? 2*d*, Are they an altered form of any corpuscle normally existing, or are they a new and peculiar formation?

In reference to the first question, we see no reason to doubt that the blood was the real habitat of the bodies in question, and that their presence in the other tissues was owing to the blood which they contained. The only organ to which we need refer as to this point is the spleen, in which the remarkable change in colour (to a dingy greenish purple) may possibly be explained by the great numbers of these peculiar bodies observed in its tissues.

In regard to the second question, we are disposed, on the whole, to look upon the bodies in question as a modification of the red blood-corpuscles ; not improbably a product of their decay or disorganization. This view appears to be favoured—1*st*, By the existence of colouring matter in these bodies, though less in quantity than in the red corpuscles. 2*d*, By their complete solution in acetic acid (though it is admitted that their resistance both to this re-agent and to water was such as to distinguish them in a marked manner from ordinary blood-corpuscles). 3*d*, By the observation which it appeared to us we were able to make, of intermediate forms (so variable, however, in character as not to admit of description). If this view of the origin of these bodies be correct, we conceive that the existence in this blood of red and white corpuscles having, in all respects, the ordinary characters and reactions, is a sufficient evidence that the change which produced the bodies in question was one effected in the living fluid, and not a mere chemical transformation of the blood-corpuscles after death. Neither can the change be admitted as one characteristic of cholera, inasmuch as it appears to us fully established (as the evidence of one of our number who has had ample opportunities of observation) that cholera produces death in all its stages without any such alteration of the blood or of the spleen as the one alluded to.

DR DE CHAUMONT ON THE SYMPATHETIC NERVOUS SYSTEM.

While the researches of modern physiologists have thrown great light on the functions of the cerebro-spinal nervous system, and demonstrated, for the most part, the operations which each several portion of that system governs, we are still left in considerable darkness as to the functions of the sympathetic or plexu-ganglionic system of nerves. A number of facts have been ascertained by physiological experiments on animals, and the results of artificial lesions of the different parts been noted, but little or nothing has been done either towards forming a comprehensive theory of the functions of these nerves, or observing the part they play, either as primary or secondary agents in disease, and in the pathological changes which take place in the human body. It is evident that some very important duty has been assigned to a tissue which pervades nearly every part of the body, and which is invariably present there, even when the cerebro-spinal system is completely absent, as in the acephalic monsters. The researches of the earlier of the modern French physiologists, those of Dr John Reid, and more lately, the interesting and valuable experiments of MM. Claude Bernard and Brown Séquard, have thrown great light upon this difficult and obscure subject. These researches were communicated to the Imperial Academy of Sciences, in whose Comptes Renders for the 16th and 23d January 1854 will be found M. Brown Séquard's last paper ; whilst a translation of it, and an abstract of M. Bernard's researches, have been published in the last number of the Monthly Journal. The remarks I have to offer are more especially in reference to the results obtained by these two gentlemen. These have been ultimately the same, and differ only in the views which each has taken of the rationale of the action which produced the phenomena. M. Bernard has found that section of the sympathetic nerve is productive of increased temperature, and a proportionate power of resisting the usual effects of death, such as putrefaction, etc., in the part of the body whose nervous connection has been thus cut off. Superexcitation of the nerve by galvanism or other methods, caused a diminution of the temperature; the same result followed the section of the cerebro-spinal nerves, unless the fibres cut contained some sympathetic filaments—in which case the temperature was

raised. Similar results were obtained by M. Brown Séquard, and they were attributed by him to the sudden afflux of blood to the part consequent on the paralysis of the capillary vessels, caused by the cutting of the sympathetic fibres supplying them where the temperature was raised, and to the deficiency of blood consequent on the contraction of the vessels, arising from the increase of the nervous force from the superexcitation of the nerve where the temperature was diminished. This view he supports by the results of experiments detailed in his paper in the Comptes Renders of the 23d January 1854. Although Bernard combats this view, he nevertheless admits that the increase of temperature does *not* take place where the blood is *absent*. On the other hand, Brown Séquard conceives that this action is not special to the sympathetic, but is also a result of section of cerebro-spinal fibres, and irritation of parts of the cerebro-spinal centres. He is here probably incorrect, having very likely cut nerves which contained sympathetic filaments, such as the sciatic (Bernard) and irritated parts of the brain, such as the ganglia at the base, which contain large quantities of ganglionic corpuscle, and seem to have a close analogy to sympathetic centres. If, now, we make use of these results, a very valuable application of them might be made to clear up obscure points in pathology and therapeutics.

The body (as has been long known) may be divided into two grand divisions—animal life, comprehending the functions of the limbs and shell of the trunk, and whose governing centre is the cerebro-spinal, and organic life, comprehending the functions performed by the contents of the trunk, and whose centre is the sympathetic system. These two are the type and antitype of each other, each tissue in the one having its homologue in the other, thus :—there are two kinds of nerves, the cerebro-spinal belonging to the animal life, the sympathetic, or plexu-ganglionic, to organic life; two kinds of muscle, the striated to animal, the flat nucleated to organic ; two great divisions of membranes, the serous being especially the animal, and the mucous the organic. These two systems, though distinct, are yet closely dependent on each other, for wherever an organic function, such as nutrition, is exercised in animal life, organic nerve is present; whilst, on the other hand, the distribution of animal nerves to the organs of organic life regulates the functions of the latter according to the necessities of the former. The characteristic of animal muscle is great suddenness and force in contraction, whilst organic fibre is thrown into action more gradually, and continues longer in a contracted state. To this the only exception is the heart, in whose case there is required muscle which will act suddenly and powerfully, without being directly under the power of the will. All these ends are answered by this organ possessing the striated or animal muscle, but without any direct nervous communication from the cerebro-spinal centre, deriving the governing force from its own peculiar ganglia, and from the solar plexus. Again, the *organic* membrane, the *mucous*, is peculiarly fitted for absorption, whilst the *animal*, the *serous*, has no primary tendency to that function, but, on the contrary, resists the presence of extraneous matter by sudden and rapid inflammations. Now, as the organic life is that which man has in common with all organised beings, animal or vegetable, it is the last to loose its vitality; and as the animal life is that peculiar to beings capable of exercising volition, the death of such a being must be the permanent separation of the organs of animal life from the conscious principle, self, or ego, which originates that volition. Hence all death must be death at the brain ultimately, but the proximate causes may be divided into two classes corresponding to the two systems of nerves, the cerebro-spinal or animal, and the plexu-ganglionic or organic, and each of these again into three species, those of the first class being, *concussion*, *coma*, *asphyxia*, those of the second, *concussion*, *asthenia*, *anæmia*. To the consideration of these forms I shall afterwards revert, and shall in the meantime speak of the differences between the cerebro-spinal and plexu-ganglionic nerves. The former system is directly under the control of the will exercised through the one great centre, the brain, and in

consequence all the fibres pass directly either to it or to the spinal cord. They run all parallel to each other, never branching or crossing each other, so that no interruption exists between the part excited and the nervous centre. There are no secondary centres disseminated through the system. On the other hand, the plexu-ganglionic system consists of a great number of centres, primary and secondary, and of various degrees of power and importance. The fibres cross frequently, and at the points of intersection corpuscles are formed, and flexuses arise, becoming secondary centres. In mathematics it is laid down as an axiom that the extremities of lines are points, and the intersection of one line with another is also a point. If for point we substitute nervous centre, and for line nerve, we shall at once see why the cerebro-spinal fibres never intersect, for that would at once form a secondary centre, which is not only not required, but would be absolutely injurious to the proper performance of the functions of animal life. Now, the use of these numerous centres generating force probably is to provide greater rapidity of reflex action than would take place if it were referred to the brain, and also to break up and disperse the force so as to prevent its being acted upon directly by the power of the will; for we find that wherever a branch is sent from the cerebro-spinal centres to an organ of organic life, it is reflected through a ganglion or broken up into a plexus before reaching its destination, so as to modify the power of volition over the organ to which it is distributed. Thus the cerebro-spinal branches sent to the iris are reflected through the lenticular ganglion, and the pneumo-gastric is broken up and dispersed through the pulmonary flexuses, whilst any branches that it does send to the heart before that are mixed with and modified by sympathetic fibres. A very distinct line is thus drawn between animal and organic life. Now, from the experiments of Bernard and Brown Séquard it would seem that the function of the plexu-ganglionic nerves is to retain the walls of the blood-vessels in a proper state of equilibrium between extreme contraction and extreme dilatation. When they are over-excited, they communicate increased force to the vessels, which become consequently contracted; and when, on the other hand, they (*i.e.*, the nerves) are extremely depressed, the vessels dilate, there is a determination of blood to the part, and the consequence of congestion ensue. It is evident that, if one part of the body be over-supplied with blood, some other must *pro tempore* be deficient of that fluid—and this general rule may be laid down. Any cause which causes an increased determination of blood to the organs of organic life, will cause diminished circulation in the organs of animal life; and, on the other hand, a diminished quantity of blood in the organs of organic life will cause increased circulation in the organs of animal life. Hence any cause which, by depressing the force generated in the great organic centres of the thorax or abdomen, would give rise to dilatation of the vessels and increased determination of blood to the viscera, would at the same time induce diminished circulation in animal life, with consequent diminution of temperature, feeble pulse, and sinking. Now, the consequence of increased afflux of blood to a part is effusion of the watery portions of the fluid, and where that part is the intestinal canal, these watery portions would pass off from the mucous membrane, and be thrown out either by vomiting or purging, or both. Is not this the state of things in malignant cholera? The morbific poison inhaled into the lungs is probably immediately communicated to the solar flexus through the fibres distributed to the capillaries of the lungs, thus causing a sudden depression of force and consequent dilatation of the intestinal capillaries and its results.

When, on the other hand, a cause acting on the solar plexus superexcites it, increased force is communicated to the visceral capillaries; these contract and occasion a deficiency of blood, and consequent deficiency of secretion and constipation is the consequence; then there is of necessity increased circulation in animal life, the blood must continue moving, and as the same amount has to go through less space it must of necessity go quicker, therefore the heart's actions are increased in number and power, besides receiving greater force from

the original sur-excitation of the ganglionic centre; increased heat is thus the consequence. This is the state of matters in the inflammatory fever or synocha.

Between the two extremes of depression and sur-excitation we have all gradations, and I think it more than probable that all diseases of a general nature, such as fevers, plague, etc., are to be referred to an altered state of the plexu-ganglionic system. Inflammatory spots have been found on the ganglia in both cholera and plague.

The phenomena consequent on the administration of many therapeutic agents may be explained by action on the sympathetic. The agents which cause loss of sensibility may be divided into two classes, the *first*, which act by depressing the sympathetic nervous force, and the *second*, by exalting it. Into the *first* category enter alcohol, chloroform, hydrocyanic acid, nicotine, etc. Chloroform, when inhaled, causes first a certain amount of stimulation, and soon afterwards depression of the ganglionic system; the blood is then retained in the viscera, less is sent to the organs of animal life, and decrease of temperature ensues. This was observed first by Duméril and Dumarquay, and subsequently by Bernard. I can corroborate these observations by experiments made by myself on animals. The quantity of blood sent to the brain soon becomes less and less till at length the equilibrium of pressure is destroyed, and insensibility to pain ensues; subsequently the circulation in the spinal cord decreases, and muscular contraction no longer continues. This is the only stage to which anæsthesia is ever intentionally carried. The next is ultimate depression of the ganglionic centres, and consequent death by asthenia from stoppage of the circulation. The action of alcohol is quite analogous, only less rapid. Hydrocyanic acid acts most rapidly, the stage of excitement being almost imperceptible. The *second* class comprehends opium and such like. Opium acts by exalting the nervous force, causing decreased circulation in organic, and increased circulation in animal life, and consequent increase of animal heat (observed by Duméril and Dumarquay.) The insensibility is brought on by excessive circulation in, and consequent super-excitation of the brain. It is a remarkable fact, and one which I have observed in several instances, that extreme insensibility to the effects of opium, and extreme sensibility to the effects of alcohol, chloroform, etc., often go together, while, at the same time, purgatives are productive of very powerful effects. This arises from an original diminished polarity of the sympathetic rendering it insensible to the exalting force, opium, but extremely sensible to the depressing forces, the anæsthetics and purgatives. In these cases, too, extreme sensibility to the action of mercury is observable. In other cases, of an opposite nature, considerable tolerance of mercury and purgatives, as well as of the anæsthetics, with extreme sensibility to opium, observable. In these the original state of the sympathetic is an exalted polarity. The action of mercury may also be to some extent explained, it is a depressor of the sympathetic, causing consequent determination of blood to the membrane of organic life, the mucous, and increase of the usual secretions.

If we look at the action of the class of anæsthetics, we shall find that they depress the sympathetic in the inverse ratio of their electro-positive relations. Thus hydrocyanic acid, the most negative, depresses the most rapidly; alcohol, the most positive, the least rapidly. We may, therefore, state as a general rule, that substances depress the ganglionic system in the direct ratio of their negativity, and exalt it in the direct ratio of their positivity. So that all acids depress, while alkalies exalt, and intermediate substances according to their mutual relations. Thus acids cause diarrhœa and stop sweating, that is, by depressing the sympathetic the organic capillaries become distended, and effusion of the watery portions takes place to a certain extent, while at the same time decreased circulation in animal life ensues, with consequent decrease of temperature, and stoppage of effusion of the watery portions of the blood.

The plexu-ganglionic system exerts a remarkable influence over sensation,

both special and common; neither can exist without it. All the nerves of special sense have ganglionic filaments and corpuscles: there is a ganglion at the roots of all the different cerebro-spinal nerves: pressure, as by aneurisms, on the chain of ganglia causes acute pain. What the function is, and how exercised is very obscure, but it is observable that the more exalted the polarity of the sympathetic in an individual, the more sensitive he is to pain. This is observable in the class of people who are easily affected by opium, while those who are very tolerant of that drug, are comparatively indifferent to pain. This exalted polarity is very frequently present in individuals of high intellect and great genius, those, in short, whose cerebro-spinal system is also exalted in force. Thus an individual who is extremely sensitive to pain, whose bowels are naturally constipated, who is tolerant of the class of anæsthetic drugs, but who is very easily affected by opium, will very probably be a man of great mind; and the probability will be all the more strengthened if he have a large head. On the other hand, a person who is comparatively tolerant of pain, whose bowels are easily affected by purgatives, who is incapable of standing much alcohol, chloroform, etc., but who is very insensible to opium, will, in all probability, be a plodding, working man, of good sound judgment and common sense, but very little genius; and the more so if he have a small head.

Reverting, as I promised, to the modes of death, we have, as before stated, two orders divided into three species:—Death by concussion of the brain arises from direct action on the nervous centre. Death by coma is caused by an over-excitation of the brain, generally by some poison circulating in blood, also by mere excess of blood, and by mechanical pressure. Death by apnœa is the same as death by coma, only the cause of excitation is the unoxidised blood. Thus all three are death at the brain. Similarly we have three modes of death by the sympathetic: death by shock or concussion, causing sudden depression, and consequent stoppage of the circulation. The usual pressure being thus suddenly taken off the brain, death ensues. Death by asthenia is more gradual depression of the sympathetic and consequent stoppage of the circulation. Death by anæmia is ultimately death by asthenia, the cause being deficiency of blood. All these three modes of death by the sympathetic are ultimately death by the brain. They might be thus arranged in a tabular form:—

Death at the brain by	direct violence,	concussion,	
	increased or altered circulation,	coma,	
		apnœa,	
	deficient circulation,	concussion of solar plexus,	sudden.
		asthenia,	gradual.
		anæmia,	gradual.

SUPERNUMERARY FINGERS.

Dr Matthews Duncan showed two supernumerary little fingers, which he had removed from a newly born male infant. They each had two bony nuclei and a nail, and were attached by thin skinny pedicles on the outer edge of the hand, over the heads of the metacarpal bones. The mother of this child had born another male child with a similar malformation; the fingers contained only one bony nucleus, and were attached to the skin on the first phalanx. Dr D. had seen, within the last four years, four other cases of supernumerary fingers, and was inclined to think that supernumerary little fingers and toes were the commonest malformation, certainly the commonest malformation by excess. In one of his cases there were supernumerary pediculated little fingers and toes. But that of the left hand was merely represented by a skinny tubercle. In another case there was only one supernumerary little finger on the left hand, and having a single bony nucleus. In another the little fingers were perfect in themselves, and their articulation with the metacarpus; the hand, instead of appearing deformed, was really very

pretty. In still another case, there was a supernumerary thumb of two phalanges, the first being articulated to the head of the single metacarpal bone, and causing much deformity.

CANCER OF THE CERVIX UTERI.

Dr Matthews Duncan demonstrated, under the microscope, the structure of a cancerous tuber, which he had encised from the cervix uteri. Whatever opinion the Society might form of the mass as demonstrated, there could be no doubt of the case being one of malignant disease of the cervix. The microscopic character of the tumour confirmed this diagnosis. Dr D. wished to show it to the Society, as he might again bring before the Society the result of the operation.

Dr Bennett observed, that the structure exhibited was that of epithelioma.

POISONING BY TARTRATE OF ANTIMONY.

Dr Haldane exhibited the pharynx and larynx of a man who had died of pneumonia. This patient had been treated with tartrate of antimony in large doses, but when between forty and fifty grains had been taken, he began to complain of severe sore throat. The antimony was discontinued, but the pneumonia continued to advance, and death occurred four days afterwards. At the post-mortem examination, the whole of the right lung, with the exception of a small portion of the upper lobe, was found to be in a state of grey hepatisation. The mucous membrane of the pharynx was affected with an apthous ulceration. The ulcers were generally covered by an adherent white false membrane. The free extremity of the epiglottis was similarly affected. Immediately above the right vocal cord was a small ulcer. Dr Haldane considered that there could be no doubt but that the affection of the throat in this case was the effect of the antimony. In cases of poisoning by antimony, a pustular affection of the throat had been several times remarked, but its occurrence was undoubtedly exceptional. In this case it appeared interesting to discover whether antimony was present in any of the tissues. Dr H. accordingly analysed a portion of the liver, and detected in it a considerable quantity of antimony. Antimony was also found in the lung affected with pneumonia, but in much smaller quantity than in the liver.

MORBUS CORDIS.

Dr Haldane also showed the Society a heart, in which the mitral orifice was exceedingly contracted. The margins of the two flaps of the valve had become agglutinated together, excepting in the centre, where a mere chink was left, which scarcely admitted more than a goose quill. The changes consequent upon this lesion, which had occurred in the heart, were very interesting. The left ventricle was much atrophied, its cavity was about the normal size, but its thickness was much less than natural. The columnæ carneæ were not nearly so large as they are in the right ventricle of a healthy heart. The right ventricle, on the contrary, was much hypertrophied, the columnæ carneæ in particular were very highly developed, being five or six times the size of those in the left ventricle. Both auricles were a little hypertrophied. The heart weighed $16\frac{1}{2}$ oz., this increased weight being almost entirely due to the hypertrophy of the right ventricle. The pulmonary artery was fully a third larger than the aorta. Blood had been extravasated in several situations into the tissue of both lungs.

MR KIRK ON SCALARIFORM TISSUE IN SPLINT COAL.

Mr Kirk exhibited specimens of beautifully preserved scalariform vessels which he had found in great abundance in a true splint coal in the collection at the Royal Botanical Garden. To the naked eye this coal appeared to be composed almost entirely of the stems of sigillariæ. In recent discussions regarding the nature of coal, the occurrence of such vessels had been entirely denied, and their absence had been brought forward as an argument in favour

of a theory supported by certain eminent microscopists, that coal consists entirely of altered coniferous wood. Such a thing is quite opposed to the statements of the most distinguished fossil botanists, by whom the coal epoch is marked as being characterised by the large number of acrogenous plants then existing.

ON THE POLLEN OF ZAMIA.

Dr Balfour exhibited under the microscope the fresh pollen of *Zamia horrida*, which in its ordinary condition presents an elliptical form, like that of a grain of wheat, with a groove on one side, constituted apparently by the folding inwards of the edges of the extine and intine. When the pollen is moistened, the groove disappears, and the grains become spherical. The statement of some, that the pollen of Cycads is angular, is not confirmed by an examination of that of Zamia horrida.

ON SCALARIFORM VESSELS AND TRUE CONIFEROUS STRUCTURE IN COAL.

Dr Balfour also exhibited specimens of splint coal with distinct markings of Sigillaria's, which he considered as having been concerned in its formation. Under the microscope he exhibited scalariform and porous vessels, as taken from these specimens, and he confirmed the observations made by Mr Kirk. He also showed true punctated woody tissue, from a specimen of Bohemian coal transmitted by Professor Harkness. From what he had seen, he was led to believe that cells and vessels of various kinds entered into the composition of coal in different instances, and that it was not correct to restrict the term coal to the carbonaceous fuel formed by woody tissue, whether ordinary or coniferous.

THE UNIVERSITY AND TOWN COUNCIL OF EDINBURGH.

We have thought it advisable that the origin and nature of the dispute between the Senatus Academicus and the Town Council, which has been terminated by the decision of the House of Lords, should be more extensively known. We have, therefore, taken great pains in drawing up the following account, the accuracy of which may be depended on :—

Dr Hamilton, in the year 1815, applied to the Senatus for admission of his class of midwifery into the curriculum of study for medical graduates. This having been refused, he applied to the Town Council a few years afterwards, urging his claims, and at the same time communicated to them, or to some of the leading members of the Council, a MS. paper, written by the late Baron Hume—previously Professor of Scotch Law. The purport of this was, to warn the professors of the risk of disputing the authority of the Town Council, patrons of the University, over the College, which depended on the wording of the Act of 1622, constituting the College—which he thought greater than that of the patrons of other Universities, and likely to be injurious to the College, if generally known.

In 1824, the Town Council transmitted Dr H.'s memorial to the Senatus with an expressed opinion in its favour. The Senatus, by the advice of the Medical Faculty, replied that they had in contemplation several additions to the medical curriculum, and would consider the claim of Dr H.'s class along with the other proposed additions. The Faculty then revised the Statuta Solennia, and recommended the addition of a fourth year of study, and of various classes, of which the class of midwifery and diseases peculiar to women and children was one. These statutes were approved by the Senatus and transmitted to the Town Council; and as they contained the addition of the class of midwifery to the prescribed course of study—it was hoped that any collision of the two bodies on this matter was averted.

This addition to the Statuta, however, contained a note intimating to the students, that those who had commenced their studies before its publication, would not be required to take any of these additional classes, nor the additional

year of study, which were thus imposed for the first time—*i.e.*, that these changes were not to have an *ex post facto* operation upon them; and in this the Senatus acted *in conformity with the practice of the University in all previous cases of addition to the medical curriculum.*

Dr Hamilton on this again applied to the Town Council, to have his class added to the curriculum *instanter*, and the Town Council sent a message to the Senatus, insisting that attendance on this class, and *this only, out of all the additions then made to the curriculum*, should be made imperative, even on those candidates for graduation who had previously commenced their studies, and to whom, as the Senatus thought, the faith of the University was pledged, that no addition to the laws, previously in force, should affect them.

The Senatus, having considered this point carefully, and finding that the number of students in this predicament was considerable, regarded the demand of the Town Council as an act of gross partiality to an individual, and *unanimously* declined to comply with it; and on *this specific point*—not, as has often been said, on the refusal of the Senatus to add the class—the Town Council brought the first action against the Senatus. Such was the origin of the protracted litigation which has just terminated—the object being to have it declared by law, whether a Town Council or an Academical Senate should regulate the studies of candidates for graduation.

It would be wearisome to detail the numerous contests which have been carried on between the two corporations since then. We have stated the origin of the first action; it is only necessary that we should give the cause of the last and decisive one. The rule of the University has hitherto been only to receive certificates of attendance on those lectures which are given in university collegiate schools. Much inconvenience resulted from this rule to students, and the Medical Faculty—aware that some of the excluded Medical schools equalled the others in celebrity—resolved in 1845 to receive the certificates of the large hospital schools in London, and of the teachers appointed or approved by the Royal College of Surgeons in Dublin. This opening of the classes preparatory to a degree, was agreed to by the Senatus, on the understanding that such courses of lectures as were to be received, were by teachers specially appointed by an act of election of some public body, and were in every respect equivalent to those given by the Edinburgh professors. The patrons of the University rejected these changes; but, advised, no doubt, by parties in the interest of the extra-academical teachers, resolved that not only the foregoing schools in London and Dublin, but likewise all the private lecturers in Edinburgh itself, without exception, should possess the privilege of educating for the University degree, to the extent of one-third of the limited number of classes required. This regulation was not only an infringement on the privileges of the professors, but from its indiscriminate character was thought likely to be very injurious to the cause of medical education, inasmuch, as although some of the extra-academical teachers were well qualified, others were not, and all were at that time self-appointed. The Senatus, therefore, resisted the resolutions of the Town Council by an action at law, and, ultimately, appealed to the House of Lords; but Judgment was given against them in the terms published in our last number.

It must be confessed that a Town Council is a very extraordinary kind of corporation to assume the functions of a University Court. We have only to imagine the Mayor and Town Council of Oxford or Cambridge, regulating the statutes for degrees in those distinguished Universities, or the Lord Mayor and Aldermen of London holding a conclave to determine whether a course of general pathology was, or was not, necessary in medical education, to be struck with its absurdity. It is admitted, on all hands, to be "anomalous," and that nothing of the kind exists anywhere else in the civilised world. Nor is this to be wondered at; for, supposing the patrons to be guided merely by a sense of public duty in laying down academic regulations, they are a corporation very ill fitted for exercising such a power:—*first*, Because they are too numerous a body; *secondly*, Because they are too fluctuating a body, the leading members of them being

liable to change every two or three years; and *thirdly*, Because the purposes for which they are elected are essentially different from the regulation of literary and scientific degrees. A University which is to be regarded only as an appendage to a Town Council thus appointed, cannot expect to maintain a high literary and scientific character.

The professors, therefore, we think, only did their duty to the University in resisting this claim of the patrons by all the means in their power; and that they were right in this will appear from the sentiments of two impartial Government Commissions. For at the time when the litigation commenced, they made a representation to Government, which resulted in a Royal Commission to inquire into this and other Scotch Universities; and although no legislative measure has been carried into effect in consequence of this commission, yet they have had the satisfaction of finding, that, both by that Royal Commission, and by another, afterwards appointed to inquire into the constitution of the Scottish Burghs, it was recommended that this power should be taken from the Town Council, and that a University Court should be established, consisting of a very different class of men, and of more permanent duration.

Whether the reports of these commissions will yet be acted upon, or not, is of course unknown. But it may be confidently maintained, that in the only two instances in which the authority of the patrons has been interposed before the present year, in opposition to the opinion of the professors, as to medical degrees, it has been an act of partiality to individuals, and we believe, has been injurious to the character of the University. One of these cases was already mentioned, viz., giving to an *ex post facto* operation attendance on Dr Hamilton's class, and on *it only*, of several, made imperative at the same time; the other was making the class of general pathology imperative on all graduates of this University, although it *does not exist* in any other University in the kingdom. Such acts of partiality in favour of active and ambitious individuals are what must be expected and always have been apprehended, in a University which is regarded only as an appendage to a Town Council consisting of men of business, appointed to that office on political and burgh considerations merely, and not on account of literary or scientific attainments. Although this may not be perceived in the proceedings of the present Council, it may be anticipated that in similar circumstances, similar proceedings will take place in the course of the next, or succeeding generations; and on these grounds the professors cannot regret that, although expressing their willingness to have all the regulations for a degree subjected to the control of a University Court, specially appointed for that purpose, they have resisted by all the means in their power the claim of the Town Council to control regulations for academic degrees.

As matters now stand, the Town Council have absolute power, and although jealous of their privileges, we cannot for a moment suppose that their intention is to injure the University. Perhaps it has not yet occurred to them, that if they seriously intend to administer all the affairs of the institution, and decide on every regulation for the study of literature, science, and the learned professions, they will soon be overwhelmed with a quantity of business, which they did not anticipate. Besides, is it possible for them to appoint any committee of their number qualified for the task? We believe that they themselves would, on consideration, reply in the negative. Hence, they must have recourse to advice from parties who they deem qualified to judge of academical affairs, and we need scarcely point out the great danger that is from time to time likely to arise in this way. We would therefore suggest to them the propriety of themselves instituting a University court. Of this the Lord Provost for the time being might be Chancellor. The students should be allowed to elect a Rector, say every two or three years. The Principal of the University, and a few other individuals distinguished from their office as literary and scientific men might make up a Council, elected for a certain number of years, and changing from time to time as may be determined on. Some such plan appears to us the only method of removing the present difficulty. It would reconcile

both parties. The Town Council would retain its privileges; the suggestions of both Royal Commissions would be acted on, and an academical "anomaly," which it is impossible can long continue, be at once put an end to.

THE MEDICAL GRADUATES BILL AND THE EDINBURGH COLLEGE OF PHYSICIANS.

NOTHING can more strongly evince the selfish and narrow objects of certain so-called medical reformers than their attempts, while benefiting themselves, to exclude others from any privileges they may obtain. In fact, the latter purpose seems in many cases to be the chief end they have in view, and for which they would willingly sacrifice any amount of good. The transactions of the last month are strongly illustrative of these tendencies, and we trust will convince the most sceptical, that among corporate bodies, as among individuals, honesty is, after all, the best policy.

One of the great principles of medical reform is equality of privilege throughout the kingdom. Common-sense dictates the necessity of permitting a well-educated physician to practise his profession in one county or city as well as in another. There are two ways of accomplishing this. Either the exclusive privileges of some corporations may be taken away, or similar privileges can be given to those bodies which did not previously possess them. The result is the same. Hitherto one of the great obstacles to medical reform has been the exclusive privileges of the universities of Oxford and Cambridge. By a happy effort of genius, it occurred to the Senate of the London University, that they might contrive to pass a law which would confer on their own graduates the same rights as are enjoyed by those of Oxford and Cambridge. They therefore skilfully pointed out, that in consequence of the Private Lunatics Bill, their graduates are exposed to penalties for the performance of duties the proper discharge of which by competent persons is of the highest importance to the public. We willingly concede the competency of these parties to sign certificates for lunatics; but when it is alleged, as we find it to be in a printed memorial to Lord Palmerston, that the London medical graduates comprise "the *élite* of the young medical men of the best metropolitan and provincial schools," and that "their examinations are generally admitted to be more extensive, searching, and practical than those at any other institution in the country," we have only to say that the zeal of the Senate has outrun its accuracy. This, notwithstanding, was the representation which Lord Palmerston acknowledged in the House of Commons had converted his opposition into support of the Medical Graduates Bill. It so happened, however, that it was pointed out by a worthy member of the House, that there was another English university which, although it had no graduates at all, ought still to be put upon an equality with the other. He accordingly moved, when the bill was committed, that there should be inserted, after London, the words, "AND DURHAM," which, to the surprise, doubtless, of the promoters of the bill, was at once agreed to.

But it now became evident that if Durham was to possess the privileges of Oxford and Cambridge, the Scotch and Irish universities should possess them also. A bill has been accordingly brought into the House by Mr Cowan and Colonel Blair to that effect, which, we sincerely hope, will receive the sanction of the legislature. Of course, it was never anticipated by the London University that these results would follow their attempts at legislation, but so it happens, that their intended monopoly opened up the unexpected means of overcoming one of the great obstacles to medical reform. If the licensing boards for generalpractice, imitating the tactics of the London University, instead of seeking a repeal of the Apothecaries Act of 1815, could succeed in extending the provisions of that act to themselves, then the equality of practice which has been so ardently desired by medical reformers would be at once attained without injury to any individual corporation.

This satisfactory state of things, however, is too unexpected to please the old reform committees of our colleges. It is not their mode of proceeding—they can't understand it—their consistency, forsooth, is in danger. Accordingly, the Royal Colleges of Physicians and Surgeons of Edinburgh, have determined by majorities at very small meetings summoned to consider this question, that they ought to oppose the various bills before Parliament. In short, they would rather perpetuate the present unsatisfactory state of things, under the ridiculous plea, that their plans of medical reform—which have been nothing but a series of failures—will be interfered with, than adopt a new and efficient method of obtaining the advantages they desire. They remind us of the old Austrian Aulic Council who preferred seeing their armies beaten in detail by Napoleon in every campaign, rather than concentrate their forces, and change their effete system of tactics. It should be known, however, that the number of Fellows who voted at these meetings, constituting *both* majorities, were very small, and that the representations and petitions of the two Royal Colleges in point of fact represent the opinions of only twenty-two individuals.

The Edinburgh College of Physicians, we believe, has gravely compromised itself. The party now predominant in its councils, in their attempts to obtain privileges of some sort, first endeavoured to become the licensers of the general practitioner, and so encroach upon the privileges of the Royal College of Surgeons. This notable plan has at length been rightly viewed by the sister College, and been broken off by it. Now, the endeavour is to disfranchise the Scotch graduates, and their own Fellows practising throughout the country, of those privileges which, as a corporate body, they were instituted to maintain. The petition of the College to Parliament is a suicidal act, which we are satisfied the Fellows of the College and Scotch graduates generally will repudiate. It will, we trust, exhibit the dangerous and foolish policy of the small junto who now direct its affairs. The true interests of every College of Physicians depend upon a close alliance with the Universities, and in supporting the dignity of their graduates,—certainly not in efforts to injure physicians in order that they may mix themselves up with the boards who license for general practice. The bold and unscrupulous attack now made by a certain party in the Edinburgh College, on the rights of its Fellows, on the Universities, and on the Scotch graduates generally, cannot fail to expose the narrow and selfish views with which as medical reformers they seem to be actuated.

THE RECENT INQUEST, AND THE GREAT RESPONSIBILITY OF HOSPITAL GOVERNORS.

The recent inquest in London on the body of Alfred Richardson, displays a system of things which has long been an opprobrium to the great medical charities of this country. A boy, three years and eight months old, is operated on for stone in the Royal Free Hospital, by Mr Weedon Cooke, junior surgeon, assisted by his senior, Mr Thomas Wakley. It is acknowledged by Mr Cook that, after cutting through the membranous portion of the urethra, the point of the knife slipped out of the grooved staff, so that when the forceps were introduced they did not enter the bladder. After efforts which, at the least, occupied more than forty minutes, he requested Mr Wakley to assist him, and he, after eight minutes occupied in further attempts, also abandoned the endeavour to extract the stone. The child was taken back to bed, and died the third day afterwards. On dissection it was shown that the bladder had not been cut into, and that it contained a stone. So far we have only to do with an unsuccessful first case of lithotomy in the hands of a young surgeon. But it will be remembered that some months ago Mr Gay, after seventeen years' service as surgeon to this hospital, was most unjustifiably dismissed from his office, simply because a sketch of his life, which appeared in a medical journal, was displeasing to the governors. In consequence, Mr Thomas

Wakley became senior surgeon, and this Mr Weedon Cooke fills up the vacancy created by the discharge of Mr Gay. It would seem that one of the nurses told the last-named gentleman of the unfortunate case at the hospital from which he had been discharged, whereupon he went to Mr Evans, a solicitor, and asked him to find out all about it. "My object," he says, "was to arm myself against those who had done all they could to injure me and traduce my character. I told Mr Evans I had no vindictive feeling against any medical officer, but that I wished to have evidence as to the doings of the committee of the hospital." Mr Evans accordingly finds out the address of the parents, and, with their consent, demands an inquest as a preliminary step to maintain an action of damages for the loss of their child, under Lord Campbell's act. Mr Cooke having heard of these proceedings, employs another solicitor, a Mr Steele, *one of the governors of the hospital*, to attend to his interests in the matter. The two lawyers meet, and Mr Steele suggests that the parents should submit the matter to arbitration, that a public inquiry would injure the hospital, and that to prevent this he would pay L.100. This arrangement was consented to by Evans on the part of the parents, but it became whispered about that a large sum of money had been paid to suppress an inquiry, and a memorial was presented to the Home Minister by whose order an inquest was held. After an investigation which lasted ten days, the jury returned the following verdict:—"We find that the deceased Alfred Richardson died of inflammation caused by an operation unskilfully performed by Mr Thomas Weedon Cooke and Mr Thomas Wakley, jun. The jury finding great difficulty in coming to a satisfactory conclusion respecting the other matters deponed to before them, they have resolved to exercise the discretion permitted to them by declining to give any opinion on them."

Such, divested of all party and personal considerations, appear to be the simple facts of this case. Doubtless, they will be differently viewed by various individuals. For our own part, we ascribe all the angry feelings which have been excited, as well as the loss of human life, entirely to that disgraceful system which converts our great charities into establishments for carrying out the private ends of the governors. These parties seldom consider when called upon to elect a medical officer, that they are trustees for the benefit of the sick poor—the question with them is not who is best qualified, or who is the most skilful physician or surgeon—but, on the contrary, who can command most votes, or bring in most subscriptions. The governors of the Royal Free Hospital never took into consideration the important fact, that when they dismissed an experienced surgeon, and intrusted the lives of the sick poor to younger men, that they incurred a responsibility of great magnitude. Nor do the managers of other institutions, when they elect the youngest and most inefficient men they can find, ever reflect on their own culpable neglect of duty, and on the lives that may be sacrificed to their spirit of monopoly. Not long ago an attempt was made to introduce as assistant physician to Bartholomew's Hospital, a very young man, simply because he was the son of an old medical officer who had great influence. The profession throughout the country were agitated on this matter, and with difficulty succeeded in preventing the appointment. We could name an institution at no great distance, where young unknown lads, just out of their clerkships, have, through family interest, been made physicians and surgeons, while their seniors, men already distinguished by their professional labours, have been unscrupulously rejected. Nothing, perhaps, more thoroughly indicates the hollowness of mere profession, than to find the parties who, as governors and managers of these charities, systematically prostituted their patronage, and betrayed the interests both of the profession and of the sick poor, the loudest in their cry for medical reform, and the most influential as journalists, or as committee men, in urging their own narrow views on the attention of government. If dishonest in one capacity, we may be sure they will be equally so in another. Messrs Wakley, junior, and Cooke, are the victims of this state of things. They must now see that the improper

exclusion of their senior surgeon to make room for themselves, so far from being an advantage, as they may at first have imagined, has in fact been the cause of their professional ruin—that a system of hospital nepotism is the worst to flourish under—and that, even in medical politics, there is such a thing as retributive justice.

MEDICAL NEWS FROM THE EAST.

We have seen a very interesting letter from a professional gentleman of this city, now present with the army in Turkey, from which the following is an extract :—

"We remained at Schumla, only as visitors, for about ten days, and as such were very civilly treated, and had an opportunity of seeing a great deal. In the military hospital there were 1200 patients, and in the barracks (converted into an hospital for the time) nearly 500 more. Of these there were about 400 wounded, who had been conveyed from Silistria, and whilst we were there about 200 more were brought in. The remainder were made up of cases of scurvy, dysentery, typhus, which was very prevalent, small-pox, and ordinary cases. Both hospitals were sadly over-crowded. The effects of this appeared to a considerable extent in the military hospital, which is well situated, about 150 feet above the level of the town ; but it told to a far greater extent on the barrack hospital, which lay low in the town. It was indeed a veritable pest-house, and what between the unhealthy position of the hospital, the over-crowded wards, and the miserable surgery, a more distressing sight could not well be imagined than the poor fellows who had fought so splendidly at Silistria, lying there huddled together in rows on the floor, scarcely one progressing towards recovery, and a large number dying. It was a curious thing to see the immediate change for the worse, which occurred on the admission of the poor fellows into the hospital. I saw 150 removed from the bullock carts, in which they had been jolted for six days from Silistria. In spite of their exhaustion, the wounds were all doing well ; two or three days in hospital, and they all went wrong. Many of them were attacked by hospital sore. In many, scurvy, from which they had suffered on the Danube, reappeared, and they sank from dysentery ; their sores converted into what is certainly well-named *bullock's liver;* others became typhoid, some apparently became inured to it, and their wounds, after remaining stationary for some time, healed slowly. In the less sickly hospital matters progressed more favourably, although here too they were by no means free from scurvy and hospital sore. Among the wounded there were many from lance, sabre, and bayonet, but the great proportion were cases of gunshot wounds, and as the firing of small arms on Silistria was almost entirely with the minié rifle, I presume most of the wounds were from the conical ball. Ten days at least had elapsed since the receipt of the injury in all the cases I saw, and at the end of this time I could find no mark to distinguish the wound made by the round musket shot, and that by the conical ball. The *entrance* wound was always first healed. I saw no exception to this. In all positions, too, the wound of exit was larger than that of entrance. In a few cases the wound of exit was attacked by hospital gangrene after the entrance wound was healed ; others less fortunate were attacked simultaneously at both wounds. Many of the cases, I need scarcely say, were of the most interesting kind, and afforded a fine field for conservative surgery. I saw gunshot wounds at shoulder, elbow, and knee, all left to nature, which frequently meant,—the patients left to die. In a few, amputation was performed, and the patients when they did recover, had conical stumps, the circular incision being used always, and the covering of soft parts which was left, being very scanty. The dressings employed were very simple, consisting of a mass of charpie and a bandage applied twice a day. Very little attention to position, and still less to keeping steady wounded joints. I had no opportunity of judging of the merits of primary as compared with secondary amputations, but I asked the question of the surgeons. They seemed

amused, and said, of course far more recovered after primary amputation. This I have no doubt, is partly owing to the miserable hygienic condition in which they are placed by the time a secondary amputation comes to be performed.

"The Turkish soldiers are well fed, and as far as I could see, the wretched condition of the wounded at Schumla, was simply owing to the crowded state of the hospitals, and the unhealthy state of the town. The place is a mass of dung-heaps, little or no attempt at sewerage, and a filthy fœtid stream running through the middle of it. The situation of the town too, is very bad. Five-sixths of its circumference is shut in by high mountains, and, except before a thunderstorm, I never felt a breath of air in the place. The weather was oppressively hot, and the sun in the middle of the day too much for an Englishman to stand. Six surgeons had fallen victims to typhus within about three weeks; a fact which, taken along with what I have described, was, not encouraging as to our taking service in the Turkish camp. I had intended going on from Schumla to Silistria, but finding the impossibility of doing anything surgically in the Turkish camp, and hearing from Nasmyth that the place could not possibly hold out many days, and that the risk of entering the place was very great, I gave up the idea. It is a matter of deep regret to me now that I did not make the attempt, for I think I might have been the means of saving that gallant fellow, Butler's life. He was struck by a spent minié ball in the forehead, which wounded the scalp only; he was stunned at the time, but at his work again the following day. The next thing I heard was that he was delirious. I had returned to Varna by this time, and the following day I heard he was dead. His case, from all I have heard was one of those described by Pott, and there is no doubt the trepan might have saved him. It is very vexatious for me to think that, if I had got into the town, I might have saved his life; but I had no idea of his being seriously wounded, or I should have gone to him at all hazards. The way in which he and young Nasmyth have conducted themselves in Silistria is in every one's mouth here. Omer Pasha himself allows that the place has been held by them; and the Sultan, before Butler's death, had conferred the highest military honours on them."

PUBLICATIONS RECEIVED.

Lettsomian Lectures on Insanity. By Forbes Winslow, M.D., D.C.L. London. 8vo. 1854. Pp. 160.

On Gout; its History, its Causes, and its Cure. By William Gairdner, M.D. Third Edition. London. Small 8vo. 1854. Pp. 400.

The Retrospect of Medicine. January to June, 1854. Edited by W. Braithwaite. London. Small 8vo. Pp. 464.

The Diseases of the Fœtus in Utero (not including malformations), with an Outline of Fœtal Development. By Henry Madge, M.D., etc. London. Small 8vo. 1854. Pp. 200.

The Micrographic Dictionary. Part III.

Nice and its Climate; with Notices of the coast from Marseilles to Genoa. By Edwin Lee. London. Small 8vo. 1854. Pp. 168.

Notes on Spain; with a special account of Malaga and its Climate. By Edwin Lee. London. Small 8vo. 1854. Pp. 144.

The effects of Civilization on the Fortunes of the Medical Profession. An Address. By Henry Dayman, Esq., Surgeon. London. 8vo. 1854. Pp. 30.

Ein Beitrag zur Entwickelungsgeschichte der Krebshaften Geschwülste. Von R. Remak. Reprint.

The American Monthly Medical Journal.—We have continued to send our Journal in exchange, but have as yet only received the January and February Nos. of the present year. We have been informed, indeed, by the publishers, that the Nos. have been forwarded to Miller of Henrietta Street, but our application to that house of business, has remained unanswered. If some means cannot be discovered for forwarding the American Monthly regularly, we shall reluctantly be compelled to stop the exchange.

The Boston Medical and Surgical Journal.—The above remarks apply also to this Journal. We have only received one No. during the present year.

Archives Générales de Médicine.—We have during the last few days received the April, May, and June Nos. of this Journal, with a note intimating that the other Nos. of the year will soon be forwarded. As our own Nos. are invariably sent with the greatest *regularity*, what we require is similar exactitude on the part of our foreign friends.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Weight and Dimensions of the Heart in Health and Disease.* By THOMAS B. PEACOCK, M.D., Assistant Physician to St Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest.

THE following paper contains a series of observations with calculations based upon them, intended to illustrate the weight and dimensions of the heart in health, and the changes which it undergoes in different forms of disease.

The observations have all been collected by myself within the last few years; some have already been published in connection with a series of tables of the weight of the brain,[1] but most of them have not previously appeared in print, and all the weights of the diseased heart and the measurements, are now first collected and analyzed. For the purpose of presenting the facts in a more complete form, and in order to furnish the means of ready comparison between the results here arrived at, and the data from which they are deduced, I have preferred to give the whole of the observations together, rather than to refer to the former papers for those previously published.

The mode in which the observations were made is as follows:—The heart having been removed from the body, and the aorta and pulmonary artery cut across about an inch above their origin, the left ventricle was laid open by an incision commencing near the aortic orifice, and continued across the apex to the posterior wall, so as fully to expose the cavity, without interfering with the attachments of the valves. The right ventricle was exposed by a similar incision extending from near the origin of the pulmonary artery to the apex, and the auricles were laid open transversely. The organ was then deprived of coagulum, washed, wiped, and weighed.

The measurement of the girth of the heart externally was taken with a piece of tape or string, from the line of the septum in front, to the corresponding situation behind. The capacity of the orifices

[1] Monthly Journal of Medical Science, vol. vii. (New Series, vol. i.) 1846.

was ascertained by the passage of graduated balls numbered from 1 to 15, and measuring from 21 to 63 French lines—each

WEIGHT AND DIMENSIONS

No.	Age.	Sex.	Weight of Body.	Weight of Heart.		Girth of Right Ventricle.	Ext. Girth of Left Ventricle.	Length of cavity of Right V.	Length of cavity of Left V.	Thickness of Walls of Right Ventricle.		
			lb	oz.	dr.	lines.	lines.	lines.	lines.	Base.	Mid-point.	Apex.
1	7 days	F.	7	0	13	25	19			2 lines		
2	10 months	M.		1	10	29	25			1 line		
3	1 year	F.	15	2	0	34	32	22	19	2	2	$1\frac{1}{2}$
4	3 years	F.	22	1	10	35	35	21	21	1 line		
5	3	M.		2	15	35	35			1 line		
6	$4\frac{1}{2}$	M.		2	9	36	30	25	22	$1\frac{1}{2}$	$1\frac{1}{2}$	1
7	8	F.	56	5	1	42	36	36	30	1	2	1
8	8	F.	24	3	0	36	30	27	24	1	$1\frac{1}{2}$	1
9	11	M.	43	4	6	38	37	34	28	2	2	1
10	13	F.	$46\frac{3}{4}$	6	2					$1\frac{1}{2}$	2	$1\frac{1}{2}$
11	18	F.		7	15	52	48	34	31	$1\frac{3}{4}$	2	1
12	20	M.	97	11	0	49	48	48	39	2	2	$1\frac{1}{2}$
13	24	M.		8	8	50	46	40	34	$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$
14	24	M.		9	$1\frac{1}{2}$	60	60	34	32	2	2	$1\frac{1}{2}$
15	24	M.		9	0	60	54	42	40	2	$1\frac{1}{2}$	1
16	25	M.		8	8	60	38	35	32	2	2	1
17	26	M.	101	8	11	54	48	45	36	$1\frac{1}{2}$ line		
18	27	M.	120	11	12	52	52	45	42	2	$2\frac{1}{2}$	$1\frac{1}{2}$
19	30	M.		8	$15\frac{1}{2}$			40	36	$1\frac{1}{4}$	2	1
20	31	M.		9	4	48	53	46	37	2	2	2
21	37	M.		6	$4\frac{1}{2}$	50	42	36	36	2	2	$1\frac{1}{2}$
22	66	M.		10	8	60	46	45	37	2	2	1
23	20	F.		7	4	49	38	40	32	2	2	1
24	24	F.		10	11	60	48	48	42	$1\frac{1}{2}$	3	$1\frac{1}{2}$
25	47	F.		10	3			42	36	$3\frac{1}{2}$	$2\frac{1}{2}$	2
26	60	F.	87	11	0	52	40	42	36	2	2	1
27	66	F.	70	7	0	48	42	42	27	$1\frac{1}{2}$ line		

number being 3 French lines more in circumference than the former. The capacity of the orifices having been ascertained, and the state

OF HEALTHY HEARTS.

Thickness of Walls of Left Ventricle.			Thickness of Septum.	Circumference of Apertures.				Cause of Death.
				Tricuspid.	Pulmonic.	Mitral.	Aortic.	
Base.	Mid-point.	Apex.	lines.	lines.	lines.	lines.	lines.	
2 lines				13	13	13	13	
3	3	$1\frac{1}{2}$		22	19	22	19	Acute hydrocephalus and pneumonia.
3	3	2	4	28	22	25	$17\frac{1}{2}$	Bronchial phthisis.
$3\frac{1}{2}$	4	$1\frac{1}{2}$	3	26	21	25	18	Burn, 4 hours.
3	4	2		31	24	28	22	Diseased kidneys and dropsy; empyema.
3	4	$1\frac{1}{2}$		36	25	30	20	Phthisis, 4 valves to pulmonary orifice.
3	4	2		33	30	33	27	Burn of abdomen.
$3\frac{1}{2}$	$3\frac{1}{2}$	2		36	27	30	24	Abscess of liver; diseased mesenteric glands.
5	5	2		39	33	36	24	Fractured skull.
$4\frac{1}{2}$	5	$1\frac{1}{2}$		42	36	42	30	Fever; enlarged spleen.
7	7	3	6	48	42	45	30	Phthisis and morbus renum.
4	7	2	6	43	29	37	26	Phthisis.
5	6	$2\frac{1}{2}$	6	45	39	42	36	Meningitis.
5	6	3	5	60	42	48	39	Ascites and diseased liver.
6	5	2	6	60	34	42	31	Fever; foramen ovale not entirely closed.
4	6	2	6	54	45	51	42	Disd. kidneys and bladder.
5	6	3		60	42	48	36	Ulceration of intestines.
5	7	2	7	50	32	38	28	Fever, cirrhosis hepatis.
$5\frac{1}{2}$	$3\frac{1}{2}$	2	$3\frac{3}{4}$	51	45	48	36	Abscess of brain.
6	7	$2\frac{1}{2}$	$6\frac{1}{2}$	54	48	51	42	Phthisis.
6	5	$2\frac{1}{2}$	5	51	39	48	36	Phthisis.
5	7	$2\frac{1}{2}$	6	51	39	39	36	Fever.
6	8	2	4	42	33	37	29	Variola.
$3\frac{1}{2}$	5	2		60	45	48	36	Ac. capillary bronchitis.
7	5	$2\frac{1}{2}$		54	39	45	30	Ac. asthenic bronchitis supervening on chronic.
4	7	2		40	34	36	39	Bronchitis.
6	7	2		51	42	48	33	Phthisis.

of the valves observed, the orifices were laid open, and the length of the ventricular cavities measured from the attachments of the aortic and pulmonic valves to the apex, and the thickness of the parieties ascertained at three points; near the base, where the walls begin to narrow; at the mid-point between the base and apex; and at the

WEIGHT OF HEALTHY HEARTS—MALES.

No.	Age.	Weight of Body.	Weight of Heart.	Cause of Death.
		lb.	oz. dr.	
28	6 mths.		0 13½	
29	2 ,,	24	2 10	Burn, 21 days.
30	2 yrs.	31½	2 12	Burn, 6 hours.
31	4		4 4	Burn, 12 hours.
32	5	21	3 0	Coma, succeeding cholera.
33	8	48	6 4	Scarlatina.
34	10	45	4 4	Cholera.
35	11	42	8 8	Typhoid fever.
36	11		5 2	Typhoid fever.
37	11	38	4 0	Chorea.
38	11		6 0	Phthisis.
39	14		9 0	Amputation of toe; morbus renum.
40	14		6 8	Typhoid fever.
41	15	88	8 0	Laceration of internal organs from injury.
42	15		6 0	Phthisis; copious hæmoptysis.
43	16	47	5 8	Phthisis.
44	16		7 0	Bronchial phthisis.
45	16		6 8	Typhoid fever.
46	17		8 8	Typhoid fever.
47	19		11 0	Typhoid fever.
48	20	100	10 0	Diphtheritis.
49	21		8 8	Strumous pyelitis.
50	21	106	9 8	Phthisis.
51	22	127	8 0	Phthisis.
52	24		8 0	Phthisis.
53	24		10 0	Fever.
54	24	90	7 8	Phthisis.
55	24		10 4	Phthisis.
56	25	103	10 0	Phthisis.
57	25	125	10 8	Fever; white spot on pericardium.
58	25		9 0	Fever.
59	26		8 12	Lunbar abscess.
60	27	145	9 0	Phthisis.
61	27		11 4	Variola.
62	28	108	8 8	Fever.
63	28	97	9 8	Phthisis; softening of brain.
64	28	108	10 0	Fever.
65	28	97	7 8	Morbus renum.
66	29		11 0	Morbus renum.
67	29		11 0	Phthisis; white spot on pericardium.
68	32		11 8	Double pneumonia.
69	32		11 8	Morbus renum.
70	33		10 8	Ulceration of cartilages of knee.

apex. The septum was cut across about half way between the base and apex, and measured across its centre.

No organs were weighed in which there was an undue proportion of fat on the surface, and none were measured which, whether from disease or post-mortem change, were very flaccid. The weight em-

WEIGHTS OF HEALTHY HEARTS—MALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.		Cause of Death.
	Years.	lbs.	oz.	dr.	
71	33		9	0	Diseased liver.
72	33	99	8	0	Phthisis; aorta atheromatous.
73	33		11	0	Apoplexy; aorta atheromatous.
74	35		8	0	Morbus renum.
75	35	100	7	8	Phthisis.
76	35	87	7	4	Morbus renum.
77	35		6	12	Phthisis.
78	35		11	8	Laceration of aorta, from violent muscular exertion.
79	37	112	8	0	Fever, white spots on pericardium.
80	37	90	9	10	Abscess on buttock and secondary deposits.
81	37		9	0	Pneumonia.
82	37	84	9	4	Phthisis.
83	37		9	0	Fever.
84	38		8	8	Fever.
85	38		10	0	Meningitis; pneumonia.
86	38	100	10	12	Fever, chr. bronchitis.
87	38		8	8	Erysipelas of head.
88	38		11	0	Delirium tremens; softening of brain.
89	39		6	0	Carcinoma pylori.
90	39		11	0	Phthisis.
91	39		9	12	Phthisis.
92	40		11	0	Phthisis, two valves to aortic orifice.
93	40		11	12	Fever.
94	40		11	8	Cirrhosis hepatis.
95	40	130	11	8	Delirium tremens.
96	40		10	4	Phthisis.
97	41		9	8	Phthisis.
98	42	115	11	12	Pneumonia.
99	42	114	8	8	Phthisis.
100	45	79	9	8	Phthisis.
101	45		10	0	Phthisis.
102	47	90	10	0	Injury; purulent deposits; white spot on pericardium.
103	46	129	10	0	Phthisis.
104	50		9	0	Fever.
105	50		9	0	Fever.
106	51		9	12	Pleuropneumonia.
107	52		10	8	Bronchitis; white spots on pericardium.
108	53	115	10	8	Complicated dislocation of astragalus; white spot on pericardium.
109	53	108	5	0	Cirrhosis hepatis and morbus renum.
110	54	146	11	8	Meningitis and apoplexy.

ployed is avoirdupois, and the measure French lines and inches—the French line being 0.888 of an English inch, and the French inch equal to 1.065 English inch, or rather more than $\frac{1}{15}$ in. longer. The French line is equal to 3.25 millimetres. The French measures were made use of as having been those employed by M. Bizot and some other observers.

WEIGHTS OF HEALTHY HEARTS—MALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.		Cause of Death
	Years.	lb.	oz.	dr.	
111	54		9	0	Pleuritis; peritonitis.
112	55	80	8	8	Tertiary symptoms.
113	55		8	0	Phthisis.
114	56		11	8	Cut throat.
115	56	84	7	0	Phthisis.
116	60	102	11	8	Disease of stomach.
117	60	101	10	0	Fractured ribs; pneumonia.
118	60	129	11	0	Phthisis; white spot on pericardium.
119	60		11	8	Phthisis.
120	60		11	0	Chronic dysentery.
121	62	99	10	8	Phthisis.
122	70		11	8	Fever; white spot on pericardium.

WEIGHT OF HEALTHY HEARTS—FEMALES.

123	10 wks.		1	3	
124	1 year and $\frac{3}{4}$th	$13\frac{1}{2}$	1	8	Subacute hydrocephalus.
125	5 years	28	2	14	Cholera.
126	6		2	12	Burn, 14 hours.
127	7	32	2	3	Fever.
128	7		3	0	Fever.
129	11	36	3	14	Cholera.
130	17	59	4	8	Phthisis, necrosis.
131	17		7	0	Phthisis.
132	18	113	9	8	Fever.
133	18		10	8	Bronchitis; phthisis.
134	19		7	12	Typhoid fever.
135	19		8	0	Cholera.
136	20	90	9	0	Phthisis.
137	21	86	5	12	Phthisis.
138	21		10	8	Morbus renum.
139	21	97	10	0	Phthisis.
140	22		10	4	Fever.
141	23	85	6	12	Phthisis.
142	23		8	8	Disease of ear and brain.
143	24		11	0	Acute capillary bronchitis.
144	25	86	9	12	Pneumonia.
145	25		5	8	Phthisis.
146	25	66	8	0	Tertiary syphilis.
147	25	117	9	0	Variola.

The tables include 183 observations of the weights of organs regarded as healthy, and 145 observations of the weights of hearts presenting either primary or secondary disease. The measurements are of 27 healthy and 41 diseased organs. The whole series is a careful selection from a larger number of observations, of which those only are given which are regarded as accurate and complete.

WEIGHT OF HEALTHY HEARTS—FEMALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.		Cause of Death.
	Years.	lb.	oz.	dr.	
148	26		9	0	Acute phthisis.
149	28	97	9	8	Fever.
150	28		7	4	Morbus renum.
151	28		7	12	Phthisis.
152	28		7	4	Morbus renum.
153	29		9	8	Disease of liver.
154	30		8	8	Pneumonia; morbus renum.
155	30		10	0	Cholera.
156	31	104½	9	0	Delirium tremens.
157	31	89	9	12	Fever.
158	32	107	9	12	
159	33	84	8	8	Fever.
160	34	88	7	8	Phthisis.
161	34	103	11	0	Phthisis.
162	34	71	6	8	Morbus renum.
163	35	58	5	8	Phthisis.
164	36		8	8	Ac. supervening or chronic bronchitis.
165	36		10	12	Fever.
166	38	79	11	0	Phthisis.
167	39		8	8	Scrofulous abscess of liver.
168	39		10	0	Pleuritis, morbus renum.
169	39	73	7	7	Phthisis.
170	39	95	9	0	Phthisis, puerperal mania.
171	39		9	8	Morbus renum.
172	40		9	0	Sudden death during convalesence from fever.
173	40		9	4	Pneumonia; gangrene of os uteri after fever.
174	40	75	8	0	Relapsing fever.
175	45	109¾	11	0	Phthisis.
176	45	91	8	0	Tubercular peritonitis.
177	47	109	9	12	Morbus renum.
178	50		7	0	Phthisis.
179	51		8	8	Phthisis, white spot on pericardium.
180	52	74	8	0	Fever.
181	53	101	10	12	Phthisis.
182	56		10	8	Fever.
183	58	66	8	0	Gangrene of leg from obliteration of aorta.

WEIGHTS AND MEASUREMENTS

No.	Age.	Sex.	Weight of Body.	Weight of Heart.	Girth of Right Ventricle.	Girth of Left Ventricle.	Length of Cavity of Right Ventricle.	Length of Cavity of Left Ventricle.	Thickness of Walls of Right Ventricle.			Thickness of Walls of Left Ventricle.		
									Base.	Mid Pt.	Apex.	Base.	Mid Pt.	Apex.
	Yrs.		lb.	oz. dr.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
1	25	F.		9 0	54	40	44	32	4	4	2	4	4	2
2	5	F.		3 12						2		4	4	2
3	15	M.		10 0	54	42			2	to	$5\frac{1}{2}$	$3\frac{1}{2}$	to	$4\frac{1}{2}$
4	20	M.		12 0	66	42			4	7	3	6	5	3
5	$8\frac{1}{2}$	F.		6 8	54	42			4	3	1	5	6	2
6	45	M.	149	17 8	56	58	47	52	2	2	2	6	9	3
7	33	M.		12 8	60	54	42	40	$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	6	6	$1\frac{1}{2}$
8	35	M.	78	10 12	54	48	42	36	3	3	2	6	8	3
9	44	M.	127	12 12	54	50	48	40	2	$1\frac{1}{2}$	1	5	7	2
10	40	M.		12 8	60	54	48	41	$2\frac{1}{2}$	2	1	$5\frac{1}{2}$	6	3
11	50	M.		14 0	60	57	48	40	2	2	2	4	7	3
12	65	M.		12 8			36	36	3	$2\frac{1}{2}$	1	7	7	$2\frac{1}{2}$
13	65	M.	137	12 0	48	39	51	40	2	2	1	6	8	2
14	65	M.		40 12	96	86	62	51	3	$2\frac{1}{2}$	2	8	11	$3\frac{1}{2}$
15	71	M.		21 0	72	66	48	42	2	3	$1\frac{1}{2}$	10	11	$2\frac{1}{2}$
16	72	M.		14 0	76	54	51	36	3	$4\frac{1}{2}$	$1\frac{1}{2}$	6	6	$2\frac{1}{2}$
17	78	M.		18 0	54	66	42	36	3	5	2	6	9	4
18	60	F.		14 0	60	60	48	42	2	2	2	5	7	$2\frac{1}{2}$
19	62	M.		14 8				36	2	3	$1\frac{1}{2}$	8	9	4
20	62	M.		16 0			42	36		$2\frac{1}{2}$		$6\frac{1}{2}$	7	$3\frac{1}{2}$
21	24	M.		15 8	62	62	50	49	2	$1\frac{1}{2}$	$\frac{1}{2}$	$7\frac{1}{2}$	$7\frac{1}{2}$	$1\frac{1}{4}$
22	75	F.		10 12	48	45	40	38	2	2	2	8	8	3
23	33	M.		17 12	54	62	60	48	2	3	2	6	10	3
24	32	M.		16 0			56	42	2	$2\frac{1}{2}$	$\frac{3}{4}$	4	7	$1\frac{1}{2}$
25	42	M.		16 0	53	63	50	44	$1\frac{3}{4}$	$2\frac{1}{4}$	1	$6\frac{1}{2}$	$5\frac{1}{2}$	2

OF DISEASED HEARTS.

Thickness of Septum.	Circumference of Apertures.				Cause of Death.
	Tricuspid.	Pulmonic.	Mitral.	Aortic.	
lines.	lines.	lines.	lines.	lines.	
	51	39	45	30	Chronic bronchitis, with deformed spine.
	39		33	25	Abnormal septum in right ventricle.
	39	13	36	33	Abnormal septum in right ventricle ; aorta arising from both ventricles.
	45	12	36	30	Great contraction of pulmonary orifice, with open foramen ovale.
	42	36	39	19	Open foramen ovale ; thickening of valves.
11	58	43	56	39	Double pneumonia; no valvular disease.
	60	48	51	39	Plenopneumonia ; some tendency to excavation at apex of left ventricle.
	60	42	48	39	Diseased hip ; malformation by defect of aortic valves, with thickening of valves.
7	42	35	39	34	Fever.
$6\frac{1}{4}$	54	48	54	42	Fever and bronchitis ; slight thickening of valves.
5	60	48	51	42	Erysipelas of heart ; slight thickening of aortic and mitral valves.
6	54	48	48	39	Intussusception ; valves somewhat thickened, and aorta dilated.
6	57	42	45	39	Fever, with icterus and hæmorrhage from bowels ; no material valvular disease.
10	63	54	60	42	Sudden death ; slight atheromatous degeneration of aortic and mitral valves, and of aorta.
8	60	42	51	36	Bronchitis ; thickening of aortic and mitral valves ; dilatation of aorta.
4	60	45	54	36	Chronic bronchitis ; atheromatous disease of aorta and pulmonary artery ; thickening of valves.
	60	45	54	39	Scalp wound ; thickening of aortic and mitral valves; great dilatation of aorta.
	60	39	45	33	Some thickening of aortic valves ; disease and dilatation of aorta ; fatty degeneration.
	57	42	48	33	Contraction of aortic orifice, and thickening of the valves ; rupture of septum ventriculorum.
	54	42	48	36	Mass of bone projecting into the aortic orifice ; valves ossified ; recent pericarditis.
	57	45	51	42	Apoplexy ; aortic valves much thickened and decurtated antheromatous dis. of mitral valves and of aorta. Extreme contraction of aortic orifice from malformation and ossification of valves ; death from strangulated hernia.
	60	42	54	30	Injury of aortic valves from violent exertion.
	60	48	60	42	Incompetency of aortic valves, with recent endocarditis and fibro-cartilaginous degeneration of muscular structure.
	60	45	51	36	Extensive disease of aortic valves; obstruction and incompetency ; aortic disease.

WEIGHTS AND MEASUREMENTS

No.	Age.	Sex.	Weight of Body.	Weight of Heart.		Girth of Right Ventricle.	Girth of Left Ventricle.	Length of Cavity of Right Ventricle	Length of Cavity of Left Ventricle.	Thickness of Walls of Right Ventricle.			Thickness of Walls of Left Ventricle.		
										Base.	Mid Pt.	Apex.	Base.	Mid Pt.	Apex.
	Yrs.		lb.	oz.	dr.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
26	50	M.		23	0	66	60	66	50	$2\frac{1}{2}$	$3\frac{1}{2}$	$2\frac{1}{2}$	7	7	3
27	55	M.		34	0	90	81	72	60	$2\frac{1}{2}$	5	2	10	$5\frac{1}{2}$	1
28	56	M.		15	4	58	55	48	36		$3\frac{1}{2}$		6	5	$3\frac{1}{2}$
29	57	M.		11	8	44	60	38	38	2	3	1	7	7	$2\frac{1}{2}$
30	46	F.	91	16	0			52	52	2	2	$1\frac{1}{2}$	6	7	3
31	27	F.		8	8	60	48	42	36	2	2	1	4	5	2
32	23	F.		10	8	59	47	44	39	3	2	1	5	5	2
33	39	F.		15	12	75	52	51	42	2	3	$1\frac{1}{2}$	5	5	2
34	40	F.		12	8	66	50	48	42	2	2	1	6	7	3
35	68	F.		12	0	54	48	45	36	2	2	2	5	5	2
36	8	F.		10	8	60	48	36	36	2	2	$1\frac{1}{2}$	5	5	3
37	11	F.		7	8	41	45	33	33	$1\frac{1}{2}$	2	$1\frac{1}{2}$	5	$6\frac{1}{2}$	$2\frac{1}{2}$
38	12	F.		12	0	54	60	36	42	$2\frac{1}{2}$	$2\frac{1}{2}$	$2\frac{1}{2}$	5	5	$2\frac{1}{2}$
39	18	F.		13	8	72	54			2	2	1	6	6	3
40	39	F.		22	0						4		9	11	3
41	63	F.		22	0	55	55	43	42	2	2	$1\frac{1}{2}$	7	10	2
42	37	F.		9	0					$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	4	4	4

OF DISEASED ORGANS—*continued.*

Thickness of Septum.	Circumference of Apertures.				Cause of Death.
	Tricuspid.	Pulmonic.	Mitral.	Aortic.	
lines.	lines.	lines.	lines.	lines.	
	60	48	54	45	Retroversion and perforation of aortic valves; some disease of mitral and of aorta.
	60	51	60	45	Incompetency from adhesion of valves and dilatation of orifice; aortic disease.
	54	45	54	39	Incompetency; recent endocarditis.
9	48	39	42	36	Softening of brain and apoplexy; old pericarditis; thickening and incompetency of aortic valves; dilatation of aorta.
		37		29	Thickening and decurtation, and incompetency of aortic valves; aorta dilated and diseased.
4	57	39	39	30	Peritonitis, from passage of gall-stones. Pericardium entirely adherent from old attachments; mitral valve thickened and rigid.
6		29	22	25	Great contraction of aperture, and induration and thickening of mitral valve.
5	54	39		33	Sudden death. Very great contraction of mitral aperture, and induration and thickening of valves.
5	40	30	24	27	Fever. Great contraction of mitral aperture, and some thickening of aortic valves.
	54	36		30	Great contraction of mitral aperture, and induration and thickening of valves.
	36	33	45	30	Free regurgitation through mitral aperture, from dilatation of the orifice; valves slightly opaque and thick. L. aur.; 1½ to 2 l. thick.
5	42	30	36	30	Combined aortic and mitral disease, chiefly mitral, sequence of rheumatism of two years' duration.
	48	33	36	30	Great a. and mitral disease; sequence of rheumatic fever 9 years before.
5	42	36	42	27	Thickening and contraction of mitral, with some of aortic aperture and valves; old pericarditis; sequence of rheumatism 15 months before.
	51	39		30	Very great contraction of mitral aperture, with thickening of the aortic valves; aorta atheromatous.
	51	32	43	35	Very great contraction of mitral aperture; crescentic thickening of aortic valves; tricuspid valve thickened.
	21	34	18	30	Very great contraction of tricuspid and mitral valves; slight thickening of aortic valves.

WEIGHT OF DISEASED HEARTS, MALES—*continued.*

No.	Age,	Weight of Body.	Weight of Heart.		Cause of Death, and kind of Disease.
	Years.	lbs.	oz.	dr.	
43	14		16	12	Dilatation and hypertrophy of walls of left ventricle, without valvular disease; recent lymph on pericardium.
44	18		28	0	Destruction of aortic valves occasioning incompetency; malformation.
45	18		12	8	Pericarditis; purulent deposits after amputation.
46	22		12	0	Compound fracture.
47	25		12	0	Laceration of walls of heart from injury.
48	22		12	0	Phthisis; some thickening of tricuspid valve; hypertrophy and dilatation of right ventricle.
49	24		14	8	Combined mitral and aortic valvular disease.
50	23		13	0	Pneumonia; hypertrophy of left ventricle, without valvular disease.
51	21		24	0	Incompetency of aortic valves, with thickening of mitral valves.
52	25		12	0	Phthisis.
53	29		14	4	Left ventricle hypertrophied and dilated; aorta dilated.
54	26		15	0	Incompetency of aortic valves, with thickening and opacity of mitral.
55	26		13	0	Delirium tremens; some thickening and opacity of left valves.
56	27		15	0	Fever; chronic bronchitis of left valves and emphysema.
57	29		12	8	Phthisis; no valvular disease.
58	29		14	0	Cholera; great contraction, thickening, and induration of mitral valve.
59	38		21	0	Aneurism of septum of left ventricle, with hypertrophy and dilatation of both ventricles.
60	38		12	0	Phthisis and pneumonia; some opacity of pulmonary valves.
61	39		21	0	Apoplexy; great hypertrophy of left ventricle, but no valvular disease; aorta much dilated and diseased.
62	32		12	8	Phthisis; no valvular disease.
63	33		21	0	Incompetency of aortic valves, from malformation by defect; thickening; morbus renum.
64	36		13	0	Pneumonia; hypertrophy of left ventricle; white spots on pericardium; no valvular disease.
65	38		15	0	Chronic bronchitis and emphysema; no valvular disease.

WEIGHT OF DISEASED HEARTS, MALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.		Cause of Death, and kind of Disease.
	Years.	lbs.	oz.	dr.	
66	30		24	0	Phthisis, pneumonia ; aneurism of transverse arch ; no valvular disease.
67	37		14	8	Delirium tremens ; slight opacity and thickening of left valves.
68	34		16	0	Fever ; no valvular disease.
69	32		14	8	Incompetency of aortic valves ; diseased kidneys and spleen.
70	38		19	0	No material valvular disease ; dilatation of aorta.
71	38		11	8	Phthisis ; aorta dilated and diseased ; some thickening of walls of right ventricle.
72	34		20	0	Hypertrophy and dilatation of right ventricle, without valvular disease.
73	30		14	8	Pneumonia ; hypertrophy and dilatation of left ventricle, without valvular disease.
74	30		13	8	Chronic bronchitis ; no valvular disease.
75	36		23	8	Aortic valves incompetent ; some thickening of mitral and diseased aorta.
76	36		17	8	Mitral valvular disease ; some thickening of aortic valves ; hypertrophy, and dilatation both of right and left ventricles ; hemiplegia.
77	33		14	0	Morbus renum, recent pericarditis ; no material valvular disease.
78	34		22	4	Incompetency of aortic valves ; slight thickening of mitral.
79	36		12	0	Fever ; no valvular disease.
80	36		24	0	Incompetency of aortic valves.
81	32		18	0	Old pericarditis and disease of aorta, without material valvular disease ; dysentery after typhoid fever.
82	40		12	0	Aortic valvular obstruction ; malformation.
83	40		16	0	Recent pericarditis ; aortic and mitral valves somewhat thickened ; aorta dilated and diseased.
84	40		15	0	Fever, with jaundice ; some thickening of mitral valves, and hypertrophy of left ventricle.
85	44		15	8	Phthisis after fever ; no valvular disease.
86	45		11	8	Fever ; white spots on pericardium ; some thickening and opacity of aortic and mitral valves.
87	42		14	12	Chronic bronchitis ; some thickening of aortic and mitral valves.
88	45		13	8	Fever ; hypertrophy, with dilatation of left ventricle ; no valvular disease.
89	43		13	8	Chronic bronchitis and emphysema ; no valvular disease.
90	40		16	0	Pericardium universally adherent ; aortic valves healthy ; mitral somewhat thick.

WEIGHT OF DISEASED HEARTS, MALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.		Cause of Death, and kind of Disease.
	Years.	lbs.	oz.	dr.	
91	42		13	6	Fever ; no valvular disease.
92	43		14	8	Recent pericarditis ; chronic bronchitis; hypertrophy of right and left ventricles.
93	44		17	4	Old pericarditis; no valvular disease ; great hypertrophy of left ventricle.
94	45		16	4	Chronic bronchitis; thickening of mitral and aortic valves; dilatation of aorta.
95	46		10	0	Phthisis; white spots on pericardium ; no valvular disease ; aneurism of aorta.
96	44		20	0	Obstructive and regurgitant disease of aortic valves; rupture of aorta ; dissecting aneurism.
97	40		16	8	Obstruction of regurgitant disease of aortic valves.
98	50		11	12	Pleuropneumonia ; old pericarditis ; thickening of mitral valve.
99	51		13	0	Pericarditis ; bronchitis ; aneurism of coronary artery ; dilatation of cavities, with atheromatous disease of aortic and mitral valves, and of aorta.
100	51		13	12	Fever; no valvular disease.
101	51		21	8	Extensive thickening, and contraction of mitral, aortic, tricuspid and pulmonic valves ; thickness, 4, 5, and 2 lines; great hypertrophy of right ventricle, old pericarditis; morbus renum.
102	53		11	8	Chronic bronchitis ; white spots on pericardium and adhesions.
103	55		23	0	Aortic valves incompetent and some disease of aorta.
104	52		12	8	Chronic phthisis; no valvular disease.
105	58		12	8	Hemiplegia; no valvular disease.
106	50		23	8	Aorta much dilated and diseased ; no material valvular disease.
107	59		14	8	Aortic valves thickened and ossified, and aorta diseased.
108	53		10	8	Compound dislocation of astragalus; recent pericarditis.
109	57		13	0	Cut throat—no valvular disease ; dilatation and hypertrophy of left ventricle ; aorta dilated and diseased.
110	52		14	8	Morbus renum ; mitral thickened; left ventricle hypertrophied.
111	51		21	8	Mitral valvular disease, with some thickening of aortic valves and diseased aorta.
112	58		23	0	Much obstruction at aortic orifice and some regurgitation; mitral valve slightly thickened; aorta dilated.

WEIGHT OF DISEASED HEARTS, MALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.	Cause of Death, and kind of Disease.
	Years.	lbs.	oz. dr.	
113	67		13 0	Chronic bronchitis and emphysema ; thickening of mitral valve and hypertrophy of left ventricle.
114	60		11 8	Ulcer of stomach ; pneumonia ; opacity of aortic and mitral valves; aorta dilated and diseased.
115	57		16 0	Phthisis ; valves healthy, but mass of bone projecting into upper part of orifice from walls of aorta.
116	65		18 4	Apoplexy ; thickening of mitral and aortic valves ; hypertrophy and dilatation of left ventricle ; diseased aorta.
117	62		14 0	No valvular disease ; hypertrophy with dilatation.
118	71		23 0	Diseased prostate ; no valvular disease ; hypertrophy, with dilatation of right and left ventricle ; aorta dilated.
119	74		19 8	Convulsions ; thickening and ossification of aortic valves.
120	78		21 0	Thickening and ossification of aortic valves ; some thickening of mitral ; dilatation of aorta.
121	80		14 8	Fractured arm and delirium tremens ; white spots on pericardium ; no material valvular disease.

WEIGHT OF DISEASED HEARTS, FEMALES.

No.	Age.	Weight of Body.	Weight of Heart.	Cause of Death, and kind of Disease.
		lbs.	oz. dr.	
122	5 mos.	11	2 4	Malformation ; aperture in septum of ventricles, with contraction of tricuspid aperture.
123	14 yrs.	95	11 12	Morbus renum.
124	17		11 0	Fever ; some thickening of mitral valve, with hypertrophy and dilatation of left ventricle.

WEIGHT OF DISEASED HEARTS, FEMALES—*continued.*

No.	Age.	Weight of Body.	Weight of Heart.	Cause of Death, and kind of Disease.
	Years.	lbs.	oz. dr.	
125	16		11 8	Lobular pneumonia; thickening and opacity of mitral valve.
126	24		15 8	Morbus renum; no material valvular disease.
127	29		11 8	Fatty degeneration of heart and kidneys.
128	21		18 0	Great contraction of mitral orifice and thickening of valves, as well as of pulmonic and tricuspid; aortic orifice small.
129	28		12 0	Morbus renum; coma; hypertrophy, with dilatation of left ventricle.
120	29		8 8	Phthisis; aneurisma aorte.
131	34		15 0	Phthisis; pneumonia; general hypertrophy and dilatation without valvular disease.
132	34		12 8	Chronic bronchitis and emphysema; hypertrophy of right ventricle; no material valvular disease.
133	36		12 0	Fever; no valvular disease.
134	36		13 12	Morbus renum; no valvular disease.
135	30		12 8	Phthisis; no valvular disease; aorta greatly dilated.
136	40		17 0	Combined mitral, aortic, and tricuspid valvular disease; chiefly aortic; with regurgitation.
137	44		23 0	Great incompetency of aortic valves; orifice dilated and valves thickened and indurated.
138	50		11 8	Chronic bronchitis; no valvular disease; hypertropy and dilatation of right ventricle.
139	53		23 0	Combined mitral and aortic valvular disease; pleurisy.
140	57		12 8	Chronic phthisis.
141	60		14 0	Dissecting aneurism of descending aorta; no valvular disease.
142	60		17 8	Dilatation of aorta; and great hypertrophy and dilatation of left ventricle, without valvular disease.
143	66		18 0	Injury of head; no valvular disease; dilatation of aorta; hypertrophy and dilatation of left ventricle.
144	66		8 12	Recent endocarditis destroying aortic valves; pneumonia; meningitis.
145	67		18 8	Contraction of aortic orifice, and induration and thickening of valves.
146	70		13 0	Apoplexy; aortic valves thick and rigid; aorta dilated and atheromatous; hypertrophy of left ventricle.

Table I.

Showing the weight of the healthy heart in males and females, at different periods of life, as deduced from 155 observations; males 94; females 61.

Ages.	Males.		Females.	
	Nos. Weighed.	Mean Weight.	Nos. Weighed.	Mean Weight.
Years.		oz. dr.		oz. dr.
15 to 20	9	8 $2\frac{6}{9}$	9	8 $1\frac{6}{9}$
20 to 30	27	9 $0\frac{4}{27}$	21	8 $10\frac{9}{21}$
30 to 40	31	9 $7\frac{29\cdot5}{31}$	19	8 $13\frac{16}{19}$
40 to 50	9	9 $11\frac{1}{9}$	5	9 3
50 to 60	15	9 12	6	9 $7\frac{2}{6}$
60 to 70	3	10 $13\frac{1}{3}$	1	7 0
	94		61	

Table II.

Showing the different weights of the healthy heart in males and females, between 20 and 55 years of age, deduced from 125 observations.

Weights.	Males.		Females.	
	Nos. Weighed.	Per Centage.	Nos. Weighed.	Per Centage.
oz. oz. drs.				
5 to 6	2	5·2	3	12·2
6 to 7	2		3	
7 to 8	10	39·4	10	44·8
8 to 9	20		12	
9 to 10	18	39·4	13	42·8
10 to 11	12		8	
11 to 11 12	12	15·7		
	76		49	

	oz. dr.
Average weight of the healthy heart in 76 males from 20 to 55 years of age, ...	9 $8\frac{56\cdot5}{76}$
Ditto ditto in 49 females ditto	8 $13\frac{8}{49}$

From the above table all organs weighing more than 11 oz. 12 dr. are excluded; if the cases, in which, without other disease, it weighed 12 oz. be calculated, the observations are extended to 83 males and 51 females, and the average weight of the heart in males is 9 oz. $11\frac{81\cdot5}{83}$ dr.; in females 8 oz. 15 dr.

TABLE III.

Weight of the healthy heart, in persons from 20 to 55 years of age, in cases of phthisis, and of all others, exclusive of bronchitis and morbus renum, specifying the average weight in acute and chronic diseases, separately.

	Nos. Weighed	Mean Weight	Extremes	Mean Age
		oz. dr.	oz. dr. oz. dr.	Years.
MALES.				
Phthisis, . .	27	9 $3\frac{11\cdot5}{27}$	11 0 and 6 $4\frac{1}{2}$	34·03
All others, . .	44	9 $9\frac{30}{44}$		36
Acute cases only,	30	9 $13\frac{3\cdot5}{30}$	11 12 „ 8 8	36
Chronic cases only,	14	8 $14\frac{10\cdot5}{14}$	11 8 „ 5 0	35·9
FEMALES.				
Phthisis, . .	17	8 $6\frac{1}{17}$	11 0 „ 5 8	34·4
All others, . .	23	9 $2\frac{9}{23}$		31·4
Acute cases only,	16	9 $5\frac{3}{16}$	11 0 „ 7 4	34·3
Chronic cases only,	6 [1]	8 $9\frac{2}{6}$	9 8 „ 8 0	30·2

The above table contains only the cases in which the weight of the heart did not exceed 11 oz. 12 dr.; if those in which it weighed 12 oz. be included, the result will stand as follows:—

Males,	Phthisis,	30	9 oz.	$7\frac{16\cdot5}{30}$ dr.
„	Others,	47	9 „	$12\frac{6}{47}$ „
Females,	Phthisis,	17	8 „	$6\frac{1}{7}$ „
„	Others,	24	9 „	$4\frac{7}{24}$ „

TABLE IV.

Weights of the heart in all cases of phthisis, bronchitis, and morbus renum, compared with the weights of the healthy heart in all other cases, hypertrophy, pericarditis, and morbus aortæ excluded.

Weights.	Phthisis.		Bronchitis.		Morbus Renum.		All others.	
	Males.	Fem.	Males.	Fem.	Males.	Fem.	Males.	Fem.
5 oz. to 6 oz		3					2	
6 „ to 7 „	2	2				1		
7 „ to 8 „	6	3			3	2	1	5
8 „ to 9 „	2	4		2	1		17	7
9 „ to 10 „	10	1				3	8	9
10 „ to 11 „	6	4	1	1	1	1	4	2
11 „ to 12 „	4		1	1	1	2	10	
12 „ to 13 „	3	2	2	1				
13 „ to 14 „			3			1		
14 „ to 15 „		1	3		1			
15 „ to 16 „	1					1		
Mean age, . .	34·1	35·3	46·4	38·4	30·1	33·4	36·5	31·4

[1] One case, in which the disease causing death is not stated, is excluded from this calculation.

INFERENCES.

PART I.—*Weights of the Healthy Heart.*

1. The weight of the healthy heart in persons from 20 to 55 years of age, averages in males 9 oz. 8 dr., and in females 8 oz. 13 dr. The mean difference between the weights of the organ in the two sexes being thus 11 drachms.

Estimates of this description must always be, to a certain extent, arbitrary, for as the heart is found, in some cases, to be considerably above its ordinary weight, without the proportion of its walls and cavities being materially altered, or the organ being otherwise diseased, it is not easy to say at what point it ceases to be healthy. The above calculation is based only upon the hearts which weighed less than 12 oz., if those in which it attained the weight of 12 oz. be added, the average becomes somewhat higher, or 9 oz. 11 dr. in males, and 8 oz. 15 dr. in females, and the difference between the weights in the two sexes is 12 drachms.

These averages differ in some degree from those given by other observers. Thus Bouillaud[1] says that the average weight of the heart is from 8 to 9 oz. (systeme usuel, or from 8 oz. 10 dr., to 9 oz. 11 dr. avoird.), and may amount to 10 or 11 oz. (10 oz. 12 dr., and 11 oz. 14 dr. avoird.), but his observations refer to only 14 cases, and include individuals of both sexes and of various ages from 16 to 38. The mean weight of 8 adult hearts given in his table is 9 oz. 2 dr. (9 oz. 15 dr. avoird.), the extremes being 7 oz. 6 dr. (8 oz. 5 dr. avoird.), and 11 oz. 3 dr. (12 oz. 4 dr. avoird.)

Dr Clendinning[2] estimates the average weight of the male heart in persons from 20 to 60 years of age, at $8\frac{1}{2}$ oz. avoird.; the female heart at $7\frac{2}{5}$ oz. avoird., and his calculations are based upon 118 observations (58 males and 60 females). Dr Reid's[3] researches give the average weight of the heart in 89 males, from 25 to 55 years of age, as 11 oz. 1 dr., and in 53 females, as 9 oz.; the difference being $2\frac{1}{2}$ ounces.

2. The weight of the healthy heart differs in different forms of disease, being greater in persons who die after short periods of illness, and less in those who have suffered from protracted and emaciating diseases.

This result is illustrated in table 3, from which it will be seen that the weight of the heart in adult males who sank under acute diseases, averaged 9 oz. 13 dr., while in those who had died of chronic diseases, phthisis, bronchitis, and morbus renum being excepted, it averaged 8 oz. 14 dr. In females the heart weighed in those who had died of acute diseases, 9 oz. 5 dr., and in those who sank from chronic diseases, 8 oz. 9 drachms.

[1] Maladies du Cœur, 2me Ed., 1841, t. i., p. 25.

[2] Med. Chir. Trans., vol. xxi., 1838, p. 55.

[3] Lond. and Ed. Monthly Journal of Medical Science, 1843. Also, Physiological, Pathological, and Anatomical Researches, Ed. 1848, p. 376.

Dr Reid found the mean weight of the heart in 9 adult males, who had died from accidents, to be 12 oz. 6 dr., while, as before stated, in the adult males who sank from disease, it weighed on an average 11 oz. 1 dr.; but in both these calculations the weights of hearts are included, which exceeded what I have regarded as the limit of health, either in cases of accident or disease.

3. The general inference to be deduced from these observations, would appear to be that in adult males who have died from acute diseases, or from the effects of accidents, the ordinary weight of the healthy heart is from 9 to 11 oz. avoird., and in those who have died from chronic diseases from 8 to 10 oz. In females the ordinary weight of the heart in acute cases may be regarded as from 8 to 10 oz., and in chronic diseases from 7 to 9 oz. Occasionally, however, in persons of small and delicate frame, and who have died from exhausting diseases, such as cancer of the stomach or chronic affections of the liver, the heart will be found to weigh only 5 or 6 oz.; and in large and powerful persons of the male sex who have been suddenly killed or have died after a very short illness, the organ may weigh 12 oz. or perhaps even more, without exceeding the limit of health.

4. The heart increases in weight with the advance of life.

This inference is borne out by the facts collected in table 2, for the exception formed by the single case in advanced life, in females, is unimportant. It is also confirmed by the observations of Drs Clendinning and Reid, and by a table, compiled from their data, in Dr Sharpey's edition of Quain's Anatomy.[1] It is not, however, clear, to what period of life this increase extends, and whether, in very advanced age, there is not, as is shown to be the case with the brain, a more or less marked decline in weight.[2] Hospitals do not afford the means of ascertaining satisfactorily the weight of the heart in aged persons, for comparatively few old people die in them, and of these a large proportion labour under some form of cardiac disease; it is therefore very desirable that medical men having charge of public charitable institutions for the aged, should direct their attention to this point. From some observations which I have made, I have been led to suppose, that when entirely free from disease, the heart, so far from continuing to increase in weight to the term of life, undergoes a decrease in advanced age; and I have more especially noticed this to be the case in females, in whom the decrease of weight of the brain, in advanced life, is noticed at the earliest period and to the greatest extent. The opposite result arrived at by other observers, I cannot but suspect to have been due to organs not strictly healthy having been included in their calculations.

[1] Vol. ii., p. 1124.

[2] See observations by Dr Reid and myself, in Lond. and Ed. Journal, 1843, and 1846, and by myself in the Lond. Journal of Medicine.

Weight of the Heart in Phthisis, Bronchitis, and Disease of the Kidneys.

5. The weight of the heart in persons who have died of phthisis, is less than in those who have sank from other diseases; but the decrease of weight after death from that disease is usually not so marked as in persons who have died from other chronic affections, unconnected with disease of the lungs.

This inference is deduced from the facts which appear in table 3, there being, however, an exception as to the relative weight of the heart in phthisis and in other chronic diseases, in females; but the latter cases are too few in number to form a satisfactory basis for generalization. Dr Clendinning inferred that the weight of the heart in phthisis is greater than in other diseases in which the organ is healthy. This conclusion is, however, opposed by the observations here collected. It will be seen, that, with the exception of the mean weight of the heart in phthisis, in females, falling slightly below the average in the very small number of chronic cases, the weight of the organ in persons dying of that disease is intermediate between its weight in acute and other chronic diseases. The observations in table 4 show, that the heart may require a very considerable increase of weight in cases of phthisis, even attaining nearly double its ordinary weight, or 15 oz. 8 dr., without being the seat of any valvular disease, or any source of obstruction existing in the aorta.

6. The correct inference, therefore, seems to be, that in phthisis, the difficulty in transmitting the blood through the lungs occasions a tendency to enlargement of the heart, which, though counteracted, to a certain extent by the emaciation, prevents the organ declining in weight to so great a degree as in chronic and emaciating diseases where no obstruction exists in the lungs.

7. The facts collected in table 4 show, that in chronic bronchitis the heart ordinarily acquires a considerable increase of weight. In two cases only of persons who had died of this disease did the organ possess its average weight in chronic diseases, while in 9, 8 males and 1 female, it exceeded the weight regarded as the extreme limit of health; and, in three of them, all of whom were males, it weighed from 15 to 16 oz. Though the number of cases contained in the table is so few, yet the results are too marked to be regarded as accidental.

8. The condition of the heart in cases of disease of the kidneys is also shown in table 4; and from this it will be seen that in 7 cases, or 4 males and 3 females, the weight of the organ was below the average in other chronic diseases; while in 11, 3 males and 8 females, it exceeded the average, attaining in one male the weight of 14 oz. 8 dr., and in 2 females of 13 oz. 12 dr., and 15 oz. 8 dr. The facts, therefore, confirm, to a certain extent, the observations of Dr

Bright,[1] that the disease of the kidneys has a tendency to produce enlargement of the heart, unconnected with valvular disease, or disease of the aorta. This increase of weight probably results from the difficulty in transmitting the blood through the smaller vessels and capillaries.

(*To be continued.*)

ARTICLE II.—*On the Development of the Blood and Blood-vessels*, being the Prize Essay for 1854 of the Edinburgh Harveian Society. By JAMES DRUMMOND, M.D., Edinburgh.

IN the following paper it is proposed to consider the structure and mode of formation of the blood and blood-vessels in the embryo of the different classes of vertebrate animals.

In the first section will be described—1st, The mode of development of the blood corpuscle; 2d, The origin of the colouring matter of the same; and 3d, The source of the serum and fibrin of the blood.

In the second section, the structure and mode of formation of the blood-vessels will be described.

SECTION I.—DEVELOPMENT OF THE BLOOD CORPUSCLES.

a Reptitia.—In this class, the frog, as being most easily procured, is the animal in which the development has been chiefly studied, although perhaps it is not so well adapted for this purpose as certain others of the same class.

In order to understand the development of the blood corpuscle, it will be necessary in the first place to describe the different structures which enter into the composition of the ovum of the frog. This, when newly deposited, presents the aspect of a small, dark, round mass enclosed in a jelly-like substance. When examined with a power of 250 diameters, it is found to consist of an external layer, of more or less round or angular cells, which are filled with granules of dark pigmentary matter, and are placed close together so as to constitute a single layer or membrane, within which is contained the vitellus. The latter is composed of the following structures: 1st, A number of granules varying in size from what appears like a mere point on the field of the microscope, up to the $\frac{1}{4000}$th or $\frac{1}{5000}$th of an inch in diameter. The smaller of these bodies are generally round and exhibit, as described by Prevost and Lebert, the phenomenon of molecular motion. The larger are more or less angular, and commonly present a flattened oblong form. In their angular shape as well as in their general character, they appear to be more or less analogous to crystals, and have been termed by Vogt and others stearine plates (fig. 1, *a*). They are colourless and occur either free or included within a cell-wall. These particles are

[1] Guy's Hospital Reports, vol. i., 1836, p. 380.

occasionally seen to contain a smaller body in their interior. They swell out on addition of acetic acid, assuming often an oval or round shape, and then gradually fade away. Carbonate of soda does not produce any very marked effect upon them. Turpentine was added to them. They did not, however, appear to be changed by it. 2d, Bodies measuring from the $\frac{1}{4000}$th to the $\frac{1}{2500}$th of an inch in diameter. These are generally round, rarely oval in shape (fig. 1, *b*), are clearer and apparently more delicate than the bodies above described. In some of them are seen a few very minute granules; in others there are larger granules, similar to those floating free, and situated towards one side of the cell; again, in others, we find stearine plates. Sometimes they appear to be half-filled with these. They frequently contain one or two minute, spherical particles. 3d, Masses of a round, irregular, or oval shape, formed merely of granules similar to those described No. 1 around the bodies No. 2, and without any distinct investing membrane or cell wall (fig. 1, *c*). 4th, Perfect cells, consisting of a transparent external membrane, enclosing a body similar to No. 2, which seems to hold the relation of a nucleus, and also a quantity of granular matter (fig, 1, *d*). The cells in question present a more or less dark grey appearance, and have some resemblance to the bodies occurring in organs during inflammation—the compound granular cell. When isolated, they commonly present a round or oval shape, but when aggregated together they appear to be more or less angular. The granular matter they contain is sometimes present in such quantity, as to obscure the nucleus, or conceal it entirely from view. They vary considerably in size. In many, the contained granules are nearly all large, and present the angular character already described; in others, they are smaller and round.

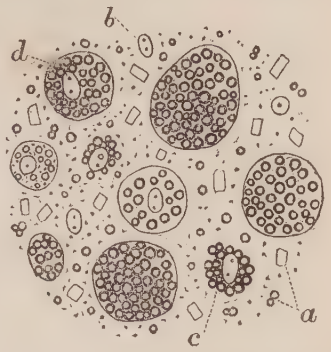

Fig. 1.—Corpuscles in the vitellus of frog's ovum. *a*, Round and oblong granules. *b*, Free nucleus. *c*, The same with granules aggregated around it. *d*, Perfect embryonic cell. 250 dia.

These bodies, which may be termed embryonic or primary cells, have been divided by Prevost and Lebert into two classes. The one is termed by them the vitelline globules, the other, the organo-plastic globules. The former, according to these observers, is spherical or oval; the spherical measure about 0·05 to 0·0875 of a millimetre in diameter; in those which are not spherical, the longitudinal diameter is about 0·125 of a millimetre, while their transverse diameter varies between 0·075 and 0·875. The granules with which they are filled belong chiefly to the larger oblong variety. The organo-plastic globules are perfectly spherical, measure from 0·02 to 0·03 of a millimetre in diameter, have a brownish-yellow colour, or brown colour, and contain a nucleus which resembles those in the vitelline globules, excepting that it is smaller, measuring from 0·0125 to 0·0175 of a millimetre. They also contain

granules and simple globules. The former term is applied by Prevost and Lebert to the smaller rounded granules already described, while the term *simple* globules is given by them to the larger angular granules, or so-called stearine plates.

The vitelline globules correspond, according to these authors, to the mucous or vegetative layer of the germinal membrane in the bird, while the organo-plastic correspond to the serous layer of the same. The former are destined to be developed into the tissues composing the organs of vegetative life, while the latter go to the formation of those which constitute the organs of animal life.

The distinction which has been made by these authors does not, however, appear to be very well founded. The cells which exist in the abdominal portion of the tadpole, and which appear to be destined for the formation of the organs of vegetative life, are certainly larger in general than the cells in those parts from which the organs of animal life are formed. It must be observed, however, that among the former there are cells as small as any of the so-called organo-plastic globules, and amongst these again there are cells present which have a diameter as great as that possessed by any of the vitelline globules. Moreover, the cells which go to the formation of the organs of animal life, and which, therefore, correspond to the organo-plastic globules of Prevost and Lebert, are not always spherical as these observers maintain, many of them being oval in shape. The other marks of distinction, as given by these authors, are in like manner not well characterised.

It seems, then, much better to employ the term embryonic or primary cells to designate these bodies without attempting to separate them into two distinct classes as Prevost and Lebert have done. The first blood corpuscles resemble the embryonic cells in every respect. They are round, do not possess any colouring matter like that which exists in the perfect blood corpuscle, and are filled with granules similar in every respect to those contained in the embryonic cells; when the blood corpuscles and embryonic cells are mingled together, it is impossible, for the first few days, to distinguish between them, and it is only by examining the former as they circulate in the vessels of the living animal that we are enabled to ascertain that they are blood corpuscles. By and by, however, the blood corpuscle may be distinguished from the embryonic cell by the circumstances of its presenting a slightly yellowish tinge.

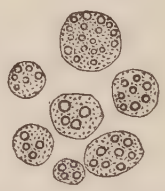

FIG. 2.—The blood corpuscles of a tadpole two lines and a half long. 250 diam.

In tadpoles measuring about three lines in length, the branchiæ are already pretty well developed, and the circulation in them established. In these, therefore, we have an opportunity of examining the corpuscles as they circulate in the vessels, and in this way are certain that the bodies we are examining are really blood corpuscles. They are spherical, possess a delicate membrane or cell wall, and

contain a number of round and oblong granules like those already described. In many these exist in such number as completely to fill them, in others they are fewer in number. Besides these granules the blood corpuscles contain a fluid which possesses considerable viscidity. Sometimes when the blood corpuscles are treated with water, a hernia-like protrusion may be seen projecting from the side of one or more of them, formed apparently by this fluid.

In each of the branchial tufts there are two vessels, in one of which the corpuscles run in the direction of the periphery, in the other they are directed inwards towards the heart; the two vessels become continuous at the extremity of the branchial tuft forming a vascular loop. They admit the blood corpuscles only in single file. The latter do not for the most part present any approach to the oval or elliptical form which they have in the full-grown tadpole or in the perfect frog. The nucleus is not always very distinctly seen.

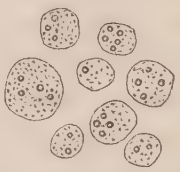

Fig. 3.—The blood corpuscles of a tadpole further developed, showing the gradual disappearance of the granular matter. 250 diam.

As regards their behaviour with reagents, acetic acid dissolves the cell wall or at least renders it invisible. Citric and tartaric acids produce the same effect upon them as acetic acid.

In tadpoles somewhat older there are still seen many corpuscles presenting the same characters as those above described. In most of them, however, the contained granules are of smaller size and fewer in number than those contained in the corpuscles of younger animals. The blood corpuscles vary considerably in size, some being nearly twice as large as the others; the largest measure about $\frac{1}{1000}$th of an inch in breadth and about $\frac{1}{800}$th of an inch in length. The smaller measure about the $\frac{1}{2000}$th of an inch in their long diameter. In many, a round or oval finely granular nucleus is distinctly visible even without addition of reagents, and sometimes two such bodies may be seen in a single corpuscle.

Many of the corpuscles present the same spherical shape as before. Others have a more or less oval or elliptical shape. The latter are generally smaller than those which present the spherical form, and are often more or less pointed at their extremities. They contain several small granules similar to those in the spherical corpuscles. The oval or elliptical corpuscles approach in character those of the fully formed tadpole. All the corpuscles present a more or less yellowish hue, deeper in some than in others. In the oval or illiptical corpuscles, the colouring matter is more abundant than in the varied ones. Besides the bodies thus described, there are sometimes observed corpuscles which in their character resemble, more or less, the white blood corpuscles which

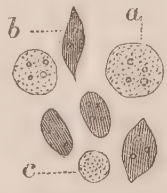

Fig. 4.—The blood corpuscles of the tadpole still further developed; *a*, Primary blood corpuscles; *b*, Corpuscles belonging to the second set; *c*, White or colourless blood corpuscles. 250 diam.

are seen more distinctly in the blood of tadpoles farther advanced.

In tadpoles, measuring about six lines in length, the tail has become much more transparent, so that the circulation can now be observed in it, as well as in the branchiæ. The blood corpuscles, which have been described as presenting an oval or elliptical shape, are now much more numerous than those belonging to the round or spherical variety. In many of them there are one or two granules, like those contained in the corpuscles of the previous animal. Such of the corpuscles as present a spherical shape also contain more or less granular matter. The elliptical or oval corpuscles have the same colour as those in the perfect animal.

If we watch the circulation, as it goes on in the branchiæ or tail of the animal at this period, we observe much more distinctly than before the presence of bodies analogous to the white or colourless corpuscle of the perfect frog. They are round for the most part, colourless, faintly granular, and have a diameter of about $\frac{1}{2800}$th of an inch, some of them, however, being considerably larger, while others again are smaller. They contain a distinct, round, or oval nucleus. Sometimes these bodies may be seen to have a somewhat oval shape, and appear slightly coloured.

In tadpoles farther advanced than the above, the chief change which is found to have taken place, is the more or less complete disappearance of the original round or spherical corpuscles. The blood corpuscles are now almost all elliptical or oval, and few of them contain any of the granules described as existing in those of young embryos. There are also present white or colourless corpuscles, like those already described. Occasionally some of these bodies appear of a more or less oval shape, and have a slight yellow tinge. They are probably transition forms, between the coloured and colourless corpuscle.

FIG. 5.—The blood corpuscles nearly fully formed in the tadpole; *a*, Coloured blood corpuscle; *b*, Colourless corpuscle; *c*, Intermediate stage between coloured and colourless corpuscles. 250 diam.

In yet older animals, in which the extremities have appeared, the elliptical blood corpuscles seem to be less pointed at their extremities than before, and present more the perfectly oval character of those in the fully formed frog. The relation between the coloured and colourless corpuscles is also much the same as in the perfect animal.

From what has been stated, it appears that, during the development of the frog, the blood corpuscles present at least two distinct phases.

1*st*, The corpuscles which first appear are spherical, granular, and colourless, agreeing in their characters with the embryonic cells. The granular matter at first contained in these gradually disappears, while, at the same time, colouring matter is formed so that at length they present the aspect of more or less smooth, round, coloured cells.

During the clearing up of these cells it is the larger granules—the so-called stearine plates—which disappear first.

2*d*, Succeeding these we have a series of corpuscles formed, which present a distinctly elliptical shape and yellow colour, and contain few or no granules.

It remains to inquire, in what relation the blood corpuscles of the second set stand to those of the first set. Are they merely the former, altered in shape and other characters, and in this way derived directly from these? Or are they new structures, which have been developed, independently of those of the first set?

In regard to this question, Prevost and Lebert believe that the first blood corpuscles are derived from that portion of the embryonic cells to which they give the name of organo-plastic globules: that these become gradually coloured, their granular contents disappearing, while, at the same time, they assume an oval or elliptical shape. In this way the second set of blood corpuscles are, according to these authors, derived immediately from the first set.

Vogt, on the other hand, maintains that the perfect blood corpuscles, corresponding to what has been above described as the second set of corpuscles, are not derived directly from the embryonic cells, but are their nuclei. At first, according to Vogt, the blood corpuscles cannot be distinguished from the cells, of which the different organs are composed. They are round, and have molecular contents. The nucleus is at first seen as a clear diaphanous vesicle: its contents are at first hyaline and perfectly fluid, but afterwards become granular. The cell wall of the primary blood corpuscle disappears, and now we find in the blood only spherical cells, which present a feebly yellowish colour, and are considerably smaller than the primary blood corpuscles, and contain more or less granular matter. In proportion as the embryo advances the granular matter disappears, and they become quite transparent. At this period a fine shadow begins to appear in the interior of the blood corpuscle. This corresponds to the first stage in the formation of the nucleus. The blood corpuscle itself is round, more or less flattened, and presents a pale yellow colour. According to Vogt, the blood corpuscles only assume the elliptical form towards the termination of embryonic life. In support of the view, that perfect blood corpuscles are the nuclei of the original embryonic cells, he states, that the size of the nucleus of the embryonic cell corresponds with that of the perfect blood globule in the tadpole. Again, according to him, the nucleus of the first blood corpuscle is when it is first observed, very distinct: afterwards the blood corpuscles are seen to be smaller, and contain only a nutritive fluid, no nucleus being seen in them. If, then, the perfect blood corpuscles be formed by a contraction and alteration in form of the embryonic cells or primary blood corpuscles, we ought, according to Vogt, to observe the nucleus in them throughout.

In regard to Vogt's observation, Prevost and Lebert state, that they have been able to trace the primary embryonic cell (organo-

plastic globule) in all its stages, into the perfect blood corpuscle of the tadpole. They have never, on the other hand, observed anything in the development of the blood corpuscle in the tadpole which indicates the disappearance of the original cell wall, and the liberation of the nucleus, or the formation of another body within the latter, such as Vogt's view implies must be the case.

As regards the smaller size of the perfectly formed blood corpuscle, as compared with that of the primary cell, Prevost and Lebert say, that the difference in this respect is very inconsiderable. According to their measurements, the primary blood cells measure 0·02 to 0·03 of a millimetre in diameter; their nuclei vary from 0·0125 to 0·0175. Afterwards when the granular matter was nearly absorbed, the blood corpuscles presented a diameter of 0·0175 to 0·0225. In those which presented an oval shape, the length was 0·0225 to 0·0275, the breadth 0·0150 to 0·0125. The animal in which these measurements were made was but little developed. In embryos, after the appearance of the limbs, and in which the circulation was complete, and the blood globules perfectly formed, they found that the latter measured 0·02 to 0·225 in length, and 0·0125 to 0·150 in breadth. Their nuclei measured 0·0075 to 0·01. From these measurements, the difference in point of size between the primary blood-cell and that which is perfectly developed does not appear to be very great, and may, according to those observers, be due to the absorption of the contents of the former. It would also appear from these measurements, that the perfectly developed corpuscle differs very much in point of size from the nucleus of the primary or embryonic corpuscle. In regard to the observation of Vogt, that after the disappearance of the membrane of the original embryonic cell, no nucleus is observed in the blood corpuscle, and that at a farther stage the nucleus again appears in them, they remark that they have been able to distinguish the nucleus throughout all the stages of the development of the blood corpuscle.

The difference in point of size between the primary and secondary blood corpuscles, appears to be certainly greater than the measurements of Prevost and Lebert suppose. We do not, so far as I can remember, find that any cell structure, while being developed into higher tissue, becomes smaller than before, the tendency being, on the other hand, always to growth—to become larger, at the same time that it becomes altered in shape. The oval blood corpuscle in the tadpole, on the other hand, appears, as has been just stated, to be considerably smaller than the original embryonic corpuscle. And this, therefore, is opposed to the view that the one is immediately derived from the other. Those of the first set, however, often pass so insensibly into those of the second, that it is very possible some of the latter may be derived from the former. In regard to Vogt's view, that they are the nuclei of the primary blood cells, it is in accordance to a certain extent at least with what is known in regard to the development of the blood in the higher animals. Moreover,

it is quite true that the nuclei are not always very distinctly seen in the blood corpuscles of the young frog. The difference, however, between the nucleus of the primary or embryonic cells and the blood corpuscles as regards size as well as other characters, are such as to render it improbable that the one is formed from the other. The different stages of the transition from the one to the other, such as Vogt describes, cannot be seen.

We have already seen that so soon as the perfect oval corpuscles appear in the blood, there also appear at the same time a number of round pale granular bodies corresponding in shape, size, etc., to the white blood corpuscles of the perfect animal. These also present, in general, corpuscles which appear to be intermediate between the coloured and colourless corpuscle, and which, therefore, may be regarded as transition stages from the one to the other. The most probable view, then, in regard to the origin of the second set of corpuscles in the frog is, that while a portion of them may be formed by a direct transformation of those of the first set, by far the greater number of them are derived from the white or colourless corpuscles. This view of the formation of the perfect or second set of blood corpuscles in the tadpole accords with what must take place in the perfect frog.

b. Aves.—At a very early period in the development of the chick, even before the forty-eighth hour of incubation, we find blood corpuscles formed, which differ but little from perfect blood corpuscles. Mingled with these, however, are cells which evidently point to a stage when the blood corpuscles, as yet, consisted only of embryonic cells similar in their nature to the first set of blood corpuscles in the tadpole. In blood from the vessels of the area varculosa in the chick on the third day of incubation, we find the following structures:—1st, Round or spherical cells, measuring from the $\frac{1}{2000}$th to the $\frac{1}{1500}$th of an inch in diameter. They possess a delicate, transparent cell wall, and contain a number of spherical particles, which refract the light strongly, and are evidently of a fatty nature (fig. 6, *a*). In many of these cells, the nucleus is not very distinct. Many of them are colourless, others present a slightly yellowish tinge. 2d, Bodies similar to the above in shape and size, but which contain few or none of the refracting particles, and are more distinctly coloured (fig. 6, *b*). 3d, Bodies measuring from the $\frac{1}{3000}$th to the $\frac{1}{2500}$th of an inch in diameter, generally round and colourless, and containing more or less granular matter, the particles, however, being in general finer than those occurring in the cells first described. They resemble

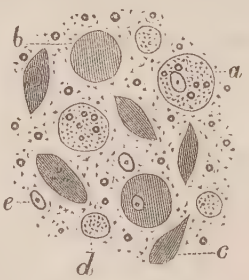

FIG. 6.—Blood from the area vasculosa of the chick, forty-eight hours after incubation; *a*, Large colourless cell; *b*, Large coloured cell, both belonging to the primary set of blood corpuscles; *c*, Coloured oval corpuscle, belonging to the second set of blood corpuscles; *d*, White or colourless blood corpuscle; *e*, Coloured granula with central spot. 250 diam.

the white or colourless cells of the blood (fig. 6, *d*). A few of them present a more or less oval form, and contain fewer granules than the others. 4th, Oval blood corpuscles, presenting a distinct yellow colour, flattened, and containing small oval nuclei (fig. 6, *c*). The latter appear when seen within the corpuscles, as a dark shadow. When the blood-corpuscles are seen upon their edge, the nucleus is observed as a small projection on either side of the corpuscle. Addition of acetic acid dissolves the cell wall, setting free the nuclei, when these appear as small oval finely granular bodies. The greater portion of the blood corpuscles just described agree in their general characters with those in the perfect animal. In a few of them we find two or three bright refracting particles, of spherical form, similar to those contained in the cells, No. 3. Lastly, There are almost always present in the blood of the chick more or fewer free granules of various sizes from a mere point up to about the $\frac{1}{7000}$th of an inch in diameter. Some of these are slightly coloured, and present a small dark point in their interior (fig. 6, *e*).

Between the colouring matter contained in the blood corpuscles and that of the yolk there is a very great resemblance.

Occasionally in some of the corpuscles we find, after addition of dilute acetic acid, two nuclei similar to what Fahrner and Kölliker have described as occurring in the blood corpuscles of the fœtal mammal.

From this period onwards the only changes which take place are the gradual disappearance of the large cells above described, and the increase in the number of the oval coloured cells. Almost all the changes which take place in the development of the blood in the chick are so rapid that it is difficult to trace them. In general, most of the changes which take place before the appearance of the oval blood corpuscles seem to be completed before the 36th hour of incubation.

FIG. 7.—The same, fifteen days after incubation; *a*, Blood corpuscle of second set; *b*, Colourless corpuscle; *c*, Oval, granular colourless corpuscle, a transition stage between the two former bodies; *d*, Granule with central spot. 250 diam.

In blood from the chick on the 15th day of incubation, there are very few, sometimes none, of the large cells described 1 and 2 present. It consists almost entirely of the corpuscles 3 and 4, with a number of free granules similar to those already described. There are, however, not unfrequently present bodies which have the same oval shape as the blood corpuscles, but are either colourless, or very slightly coloured. They do not, moreover, present the distinct, flattened appearance of the blood corpuscles. They also contain an oval granular nucleus, and sometimes a few granules (fig. 7).

Some of the oval coloured blood corpuscles contain a granule or two of a bright refracting appearance.

During the development of the blood in the chick, it seems prob-

able that there are two sets of corpuscles just as in the frog; the one set corresponding to the large cells described 1 and 2, derived from the embryonic corpuscles; the other formed partly from these, partly probably in the liver, but chiefly from the white or colourless blood corpuscles.

c. Mammalia.—In blood taken from the umbilical vein of an embryo calf about $1\frac{1}{2}$ or 2 inches in length, the following structures were found:—1st, Bodies presenting a more or less spherical shape, measuring from the $\frac{1}{2500}$th to the $\frac{1}{2000}$th of an inch in diameter, and perfectly colourless. They contained one, sometimes two nuclei, besides finely granular matter. The nuclei measured from the $\frac{1}{4000}$th to the $\frac{1}{3500}$th of an inch in diameter, were generally round, and finely granular. 2d, Bodies agreeing in size and shape with the above, but differing from in presenting a smooth surface, and in being more or less coloured. These also contained one, and sometimes two nuclei. 3d, Corpuscles presenting the same characters as the blood corpuscle in the animal after birth, but smaller and more spherical than discoid in shape. 4th, Bodies presenting the characters of the nuclei described as contained in the corpuscles 1 and 2 (fig. 8).

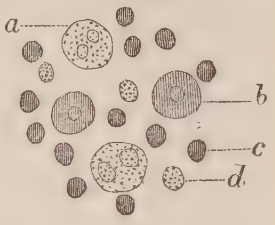

Fig. 8.—Blood from the umbilical vein of an embryo calf, two inches long; *a*, Large colourless cell; *b*, Large cell coloured—both corpuscles of the first set; *c*, Coloured corpuscle of second set; *d*, Free nucleus. 250 diam.

Between the large colourless and coloured cells 1 and 2 there are corpuscles present which seem to be transition stages from the one to the other; so also between the free nuclei and coloured blood corpuscles 3.

In blood taken from the umbilical vein of an embryo sheep about 3 inches in length structures similar to those above described were found.

In blood from the umbilical vein of an embryo calf 12 inches long the large coloured corpuscles, as well as the corresponding pale ones, were very few in number. The blood corpuscles consisted entirely almost of the bodies 3, and of white blood corpuscles. The latter were present apparently in much greater numbers than in the adult animal. They often adhered together in clumps.

Blood from the heart presented the same characters.

The younger the embryo the more numerous are the bodies 1 and 2. They appear to be analogous to the primary set of corpuscles in the frog and bird, etc. Those containing two nuclei are evidently identical with the bodies which have been described by Kölliker and Fahrner.

The bodies 3 correspond to the second set of corpuscles or perfect fœtal blood corpuscles.

The free nuclei seem to be structures in process of being converted into the corpuscles belonging to the 2d set or perfect blood corpuscle.

In blood from the umbilical vein of a calf $1\frac{1}{2}$ feet in length none of the bodies 1 or 2 were present.

The greater number of the blood corpuscles present the same characters as those in the adult animal, with this exception, that they are smaller, and appear less distinctly discoid in form. In the latter periods of embryonic life, the large cells described 1 and 2, gradually disappear, the round coloured corpuscles alone remaining. It is commonly stated that the blood corpuscles of the fœtus are distinguished from those of the adult by their greater size, as well as from the circumstance of their possessing nuclei. This, however, is true only of the first set of corpuscles, occurring in the very early periods of embryonic life. Those of the second set (Nos. 3 and 4), which form the great mass of the blood corpuscles during the greater portion of embryonic life are, as we have stated above, of smaller size than those in the adult, and do not contain any distinct nucleus. As has been shown by the researches of Harting, the different constituents of the fœtal organs are all of smaller size than the corresponding parts in the adult; the Harveian systems in the bones, for example, are much narrower; so also the elementary fibres of muscle are more than one-third finer than those in the perfect animal. Hence, then, it is only to be expected that the perfect fœtal blood corpuscle should also be of smaller size than those of the adult. According to Harting, the blood corpuscle grows very little during the last two-thirds of embryonic life. They grow very rapidly, however, shortly after an independent respiration has been established, and soon attain the size which they have permanently in the adult animal.

In regard to the development of the blood corpuscle in the different classes of vertebrate animals, different views, as may be supposed from what has been already stated, are entertained. They may be reduced to the following:—

1. The blood corpuscles are derived from the cells of the vitellus. These become gradually coloured and flattened, at the same time that they become altered in shape and lose their granular contents. Such is the view which is given by Schultz, from an examination of the development of the blood corpuscle in the frog. The same view has also been taken by Lebert and Prevost.

2. From the observations of Fahrner and Kölliker, the first blood corpuscles in the mammalian embryo also are formed in the manner above mentioned, from the cells of the mucous layer of the germinal membrane, but afterwards increase by a process of fissiparous generation.

3. Vogt, again, as has been already stated, while he believes that the first blood corpuscles are derived from the embryonic cells, maintains that the perfect blood corpuscles, those, namely, which we have described as the second set, are not those of the first set altered in shape and other characters, but are the nuclei of the same. The cell wall of the primary blood corpuscle disappears, its nucleus is set free,

and becomes developed into the perfect blood corpuscle. The nucleus which is seen in these is, according to Vogt's view, a secondary formation, a nucleolus, in short.

4. Another view in regard to the origin of the blood corpuscles, is that of Macleod and others, who hold that they arise from minute granules.

5. From the observations of Reichert and Kölliker, it appears that a portion at least of the blood corpuscles in the embryo are formed in the liver. According to Weber, when the vitellus is withdrawn into the cavity of the abdomen at the end of embryonic life, the fatty matter which it contains is absorbed by the blood-vessels and carried to the liver, where it is partly converted into bile and also in part into blood corpuscles.

There seems to be no doubt that the bodies which we have described as the first set of blood corpuscles are merely a portion of the embryonic cells, derived, according to most observers, from the mucous layer of the germinal membrane. On this point nearly all observers are agreed.

It is chiefly in regard to the origin of the second set of blood corpuscles that the differences of opinion exist—Schultz, Prevost, Lebert, and others, supposing that they are the first set of corpuscles altered in shape; Vogt, etc., believing them to be the nuclei of these. In addition to what has been already stated in regard to these two views, we will merely add that, granting the blood corpuscles of the first set are converted into those of the second in either of the two ways mentioned, the number of the corpuscles so formed must be very small indeed compared with the number actually existing in the blood. Moreover, the corpuscles of the second set increase greatly in number during the entire period of embryonic life, and long after the primary corpuscles have disappeared. Those, therefore, which are formed after the disappearance of the corpuscles belonging to the first set must have a different origin; they cannot be formed from these bodies, inasmuch as they no longer exist in the blood. From the observations of Kölliker and Fahrner, it is true the increase of the blood corpuscles in number might be ascribed to the fissiparous division of previously existing corpuscles. It must be observed, however, that it is chiefly or exclusively to the corpuscles of the first set that their observations apply.

We have already stated that, simultaneous with the appearance of the blood corpuscles of the second series, the white or colourless corpuscles also appear. Moreover, it is generally possible, in the blood of the chick and frog, to observe bodies which seem to be transition stages from the one to the other. Hence, then, although it seems probable that a portion of the coloured corpuscles of the second set are derived from those belonging to the first set, and are probably also formed in the liver, the great mass of these bodies seems to be derived from the colourless blood corpuscles. As regards the rela-

tion in which these two structures stand to each other, it seems to be different in the higher and lower classes of vertebrate animals. In the latter the colourless corpuscle appears to be itself developed into the coloured, the nucleus of the former remaining as the nucleus of the latter. Such is the case in the frog, fish, and bird. In mammalia, again, it appears to be the nucleus only of the white or colourless corpuscle, which is converted into the coloured.

From all that has been stated in regard to the development of the blood corpuscle, it seems to follow :—

1. *That there are two sets of blood corpuscles in the fœtus of vertebrate animals.* Those belonging to the *first* set are round, granular, nucleated, and colourless. After a time they become more or less coloured, their granular matter at the same time disappearing to a great extent. Those of the *second* set differ from the corpuscles of the first set, in being coloured, in their smaller size, and in containing but little or no granular matter. In the lower vertebratæ, in addition to the marks of distinction above stated, they differ from those of the first set in being illiptical or oval, and in the mammal they differ from those of the first set in not containing any nucleus.

2. *That the former set of corpuscles are derived from the embryonic corpuscles, and probably increase by fissiparous division, others being also derived from the liver.*

3. *That the corpuscles belonging to the second set may in part be derived from those of the first set, as well as from the liver ; the greater number of them, however, being formed from the white or colourless corpuscles.*

(*To be continued.*)

ARTICLE III.—*On the Causes of Colour-Blindness.* By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.

(*Continued from page* 107.)

3. *Relation of the Colour of the Choroid to Colour-Vision.*

IF the proposition be well founded, that the colour of the internal membranes of the eye must affect its perception of colours, then the choroid, which is the most fully coloured of the tunics, and the one most liable to vary in extent and depth of coloration, must have a very important influence on colour-vision.

In the normal human eye, the matter colouring the choroid and posterior surface of the iris, is rather a *pigmentum fuscum*, than *nigrum*, and is described as possessing a reddish-brown, a sepia, or tobacco-brown tint. The amount of pigment and its particular shade, vary in different individuals, according, especially, to age and race. In the young it is much darker than in the aged, and it is more abundant and deeper in tint in the dark than the fair races of mankind.

Vision is held to be in all respects most perfect, when *ceteris paribus*, the *pigmentum nigrum* is darkest, and by absorbing the rays which have traversed the transparent retina, prevents the interfering influence of cross lights within the chamber of the eye; but I shall presently have occasion to show that this opinion calls for qualification.

In the eyes of the aged, where the pigmentum nigrum becomes very pale, and in those of the albino, where it is in great part or altogether awanting, there is manifest reflection of light from the choroid;[1] but, in truth, by Helmholtz's arrangements, the choroid of every healthy human eye can be shown to reflect much light; and as this light in moving towards the pupil, traverses the retina, including the yellow spot, as well as the blood-vessels, it must (even if originally colourless) become coloured in its passage. The great distinction, accordingly, which has been drawn between the mirrorless *camera obscura* of the human eye, and the brilliant *tapetum lucidum* which, like a concave metallic mirror, lies behind a large part of the retina in the eyes of many of the lower animals, is, in reality, much less marked than it has been held to be. Without, indeed, the aid of any optical apparatus, the human eye can be seen to glare and flash as that of a cat does; and from a very early period the observation has been made, that the eyes of certain persons "shine in the dark." Dr Esser has collected several striking examples of this as occurring in albinos, but it is not confined to them.[2] The late Professor Jameson of Edinburgh states, that he had remarked a luminous property like that of the cat's eye, "in the eyes of several individuals, principally females;"[3] and Mr Cumming, before Helmholtz published, had shown, "that in the eyes of all persons, where the pupil is tolerably large, a very decided reflection from the bottom of the eye may be observed by placing the individual under trial at a distance of ten feet from a single gaslight, and directing him to look a little on one side, when a strong glare becomes visible to any one standing almost directly between him and the light. In some persons this glare is exceedingly brilliant, like that from burnished brass; in others it is fainter."[4] Similar observations were made at a later period by Brücke;[5] and before either of these experimenters had recorded his results, Dr Mackenzie had stated that: "In one instance, Sir David Brewster saw a reflection of a bright red colour, with a purplish tinge, from the bottom of the eye of a boy about ten years of age. In a girl at the Glasgow Eye Infirmary, I noticed also a purple reflection."[6]

[1] Mackenzie's Physiology of Vision, p. 218.

[2] On the Luminousness observed in the Eyes of Human Beings, etc. Edin. Phil. Journal, 1827, p. 164.

[3] Op. Cit., p. 301.

[4] Medico-Chir. Trans. Lond., vol. xxix. p. 283. Quoted in Todd and Bowman's Physiological Anatomy, chap. xvii. p. 51.

[5] Müller's Archiv., 1847, p. 225.

[6] Physiology of Vision, p. 218, 1841.

The light, which in so many ways can be shown to be reflected from the interior of the eye, is of course thrown back in part only from the choroid. But that this membrane contributes largely to the reflection, may be shown by the colour of the light which issues from the eyes of those animals which are provided with a *tapetum lucidum*, and from the colour of the pupil of the albino. In both cases the tint *corresponds* to that of the choroid. It could not be expected to be strictly identical, for the choroid being behind all the membranes but the sclerotic, the light it reflects must traverse them and have its colour altered in its passage, so that besides the influence of the *yellow spot* in colouring this light, we may always expect an addition of red from the blood-vessels. After making due allowance for this, however, we shall find the proposition hold good, that the pale choroid of the aged man, the red choroid of the albino, and the silvery green tapetal choroid of the sheep, reflect respectively a yellow, a red, and a green light.

It comes, then, to be a matter of interest to inquire, what influence such coloured light will have on the vision of colours: and as the eyes of those animals which have shining *tapeta*, exhibit coloured reflection more strikingly than the human eye normally does, I shall refer first to them, as well as to the human albino where there is so great an abnormal reflection of coloured light, before considering the reflecting power of the choroid in ordinary eyes.

The *tapetum lucidum* presents such an appearance in bright light, as we might expect in a piece of *metallic velvet*, *i.e.*, velvet woven from the finest and most polished capillary threads of gold or silver wire. It is thus described by Todd and Bowman:—"In many quadrupeds and fishes the inner surface of the choroid, in its posterior part, has a brilliant lustre, owing to the presence of a thick layer of wavy fibrous tissue, peculiarly arranged, outside the choroidal epithelium (here colourless)."[1] The appearance of this *tapetum lucidum* varies in different animals, as will be seen from the following description by Cuvier, which contains the fullest account I have been able to find of these variations. It leaves unnoticed, however, a large number of creatures.

After referring to the Ruyschian membrane (the inner surface or layer of the choroid) as more or less covered with a dark varnish (*pigmentum nigrum*), he continues:—"The bottom of the Ruyschian membrane is covered only with a very thin layer of this varnish, through which its colour, which varies singularly in different species, is perceived. In man and monkeys it is brown or blackish; in hares, rabbits, and hogs, of a chocolate-brown; but the carnivora, the ruminantia, the pachydermata, the solipeda, and the cetacea, have bright and shining colours in this part. The ox has it of a beautiful golden green changing into celestial blue; the horse, the goat, the buffalo, the deer (*cerf*), of a silvery blue changing into

[1] Physiol. Anatomy of Man, chap. xvii. p. 23.

violet; the sheep of a pale golden green, sometimes bluish; the lion, the cat, the bear, and the dolphin, have it of a pale golden yellow; the dog, the wolf, and the badger, of a pure white bordered with blue. This coloured part of the *Ruyschian membrane* is named 'le *tapis*,' [tapetum]. It does not occupy the whole of the bottom of the eye, but only that part which is not penetrated by the optic nerve."[1] In another part of this *leçon*, Cuvier refers to the *ray* (which is the only fish mentioned by him) as having the bottom of the eye of a "beautiful silvery colour." It may be added that the eyes of more than one species of shark present the same appearance, and judging from specimens preserved in spirits, where the *tapetum* looks through the semitransparent retina like tinfoil, and when the retina is removed, like dulled silver, I infer that in the living eye, the reflecting surface resembles this metal when purest and most polished, in colourlessness and brilliancy.

How far the enumeration given above exhausts the diversities of colour observed in the *tapeta* of different creatures, must be regarded as uncertain, since whole divisions of the animal kingdom are unrepresented in the list. The recorded diversities are after all, small. In fishes the tapetum may be compared to a concave mirror of bright silver. In other animals it is such a mirror coated as it were with a transparent blue, green or yellow varnish; but except a tinge of violet, it is never red. Assuming the tapetum to act as a reflecting speculum in the living eye (as I think it will presently appear it certainly does), then a reason can be seen for its colourlessness in fishes, inasmuch as the medium in which they live is itself in mass coloured, and absorbs a very large amount of the solar and sidereal light which falls upon it, so that at considerable depths there is comparative darkness, and the faint twilight which prevails will certainly be reflected with less loss by a colourless tapetum than it would be by a coloured one. Moreover, the pale phosphorescent light of the sea, which must be regarded as to a considerable extent a substitute in its darker regions, for the celestial light of which they receive so little, has so feeble a luminous intensity, that its illuminating power would be much lessened by reflection from a coloured surface, and a silvery white tapetum can turn to best account its faint rays.[2]

[1] Leçons d'Anatomie Comparée, (xii^e^ leçon); 2^de^ Edition, t. iij., p. 418, 1845.

[2] In addition to its tapetum a fish has another *moveable* ocular mirror in its golden or silvery iris. The external appearance of a fish's eye, with the incontractile brilliant iris surrounding the large crystalline, exactly recalls the appendage attached to microscopes for condensing light on opaque objects, called from its inventor a Lieberkuhn, and consisting of a circular metallic speculum, with an aperture in the centre occupied by a lens. In truth, however, the majority of fishes are altogether living mirrors; the metallic brilliancy of their skins or scales making them powerful reflectors. Those who have noticed to what an extent, in a dark night, the phosphorescence of the sea, although fainter than early twilight, will render visible fishes in the water, at a considerable distance from the observer, in virtue of their high reflecting

That the tapetum of no animal's eye is in its normal condition red, appears sufficiently accounted for by two considerations:—1. *Red* light of feeble intensity impresses the retina less than faint light of any other colour (ante p. 314), so that if the tapetum acts as a reflector, and is most serviceable when the quantity and intensity of the light entering the eye are smallest, the worst tint it could possess would be a red one. 2. The transparent blood-vessels ramifying on the retina, and in other parts of the eye in front of the tapetum, colour the light which it reflects through them, so that there is in all animals, an unavoidable reddening of the light issuing from their eyes, and a corresponding diminution of its illuminating power. With a red tapetum this diminution would be still greater.

So far, then, as our knowledge goes, the metalline tapetum is frequently white, sometimes yellow, but in the majority of land animals either blue or green; and I am now to consider the question what influence will the emission of, *ex. gr.*, blue or green light from a living animal's eyes have upon its perception of colour? It will conduce to perspicuity to take a single unquestionable example of such emission of coloured light from the living eye, and no more striking or familiar instance of this could be wished than that furnished by the common cat, whose eyes proverbially "shine *green* in the dark." The tapetum of this animal is yellow, but the light reflected from it acquires a green colour before leaving the eye. This is not a vulgar fancy. Dr Esser, who made a great many experiments on the luminousness of cats' eyes both before and after death, mentions that when a few rays of sunlight were allowed to fall obliquely on the face of a living cat whose eyes were in a straight line with his own, he "observed a most beautiful green light;" and in twilight "a yellowish green." The eyes of a cat just beheaded shone far more vividly when its face was turned to the sun, than those of the living animal, in consequence of the permanent dilatation of the iris after death, allowing a larger pencil of light to enter and leave the eye. The light then emitted "resembled," says Esser, "the most beautiful green fire."[1]

power, must have been struck by the many beautiful provisions which have been made for economising to the utmost the small amount of light which reaches, or is generated within, the depths of the sea.

[1] Edinburgh Philosophical Journal, 1827, pp. 164, 167. My friend Mr William Swan, at my request, examined with Helmholtz's speculum the eyes of his cat which is an unusually large and powerful male, with the following result:—"The colour of the tapetum, or that of the light reflected from it is what I should call an apple green, sometimes passing into nearly pure yellow. The former colour I have often seen when looking at his eyes without the intervention of the speculum."

I have found no difficulty in making similar observations on the eyes of a dog, who, by a little coaxing and bribing, was persuaded to look in such a direction that the light of a gas flame could be reflected into his eyes. The light returned from them was generally a yellow like that of illuminated brass, but in certain positions of the eye-balls, it became a full crimson,

Seeing, then, that a cat's eyes certainly emit green light in particular circumstances, how far will its judgments of colours seen by that light be affected by this? Some preliminary difficulties must be disposed of before this question, involving in considerable part that of the relation of a coloured choroid to the vision of colours, can be considered.

In the first place, there is still some reluctance to come to a unanimous conclusion regarding the source of the light which renders the eyes of many animals luminous "in the dark." The popular belief, vaguely acquiesced in by some naturalists is, that the eyes of the cat and of other creatures shine by a light of their own, which is the more conspicuous, the more absolute the darkness which it encounters. But the experiments of Esser,[1] and of Benedict Prevost,[2] incontestably prove that in utter darkness the eyes of all the higher animals very soon cease to be luminous,[3] and that within certain limits they may be made to shine at the will of the experimenter by allowing light to fall upon them.

It is possible that the vitreous and other humours, in the eyes of animals, are phosphorescent, in the way transparent bodies like the diamond are, which, when transferred to total darkness after exposure to light, shine for a time. But if the eye is thus endowed, its power to phosphoresce is very slight, and not less transient; and it is certainly as a reflector of light incident on it from without, that the tapetum mainly confers luminosity on the eye, of which it is a part. The point is important to my discussion, inasmuch as it shows that the coloured light issuing from the eyes of animals possessed of tapeta, has in great part been reflected from the very bottom of the eye, and has thus completely traversed its transparent media, before its emission; so that every portion of the retina is liable to be impressed by it.

The action of the tapetum is thus sufficiently manifest; but many able physiologists have found great difficulty in assigning a function

when, as I presume, the larger blood-vessels acted either as reflectors or transmitters of the light.

I mention these particulars because they show that a series of very interesting observations could be made by means of specula, not only on the domesticated animals, but also on the less controllable inhabitants of our zoological gardens, as to the amount and colour of the light which their eyes (whether provided or not with tapeta) emit. Any animal with its head at rest and its face fronting the observer, but especially those with tapeta, could be thus examined with the assistance of a mirror by daylight, and a moveable flame by night; and it is only by such observations that the quality of the light which the eyes of all creatures give out can be ascertained. The colour of the tapetum, does not, as some have supposed, alone determine the colour of the light which leaves the cornea; and where there is no tapetum it is still more difficult to predict what the colour of the issuing light should be.

[1] Op. et loc. cit. [2] Ibid, p. 297.

[3] I do not know how the case stands with insects, such as moths whose eyes shine in the dark; but whether from mere reflection or a true phosphorescence does not appear to have been certainly ascertained.

(or at least a useful one) to it, and there seems no agreement regarding this point. As these difficulties stand in the way of conclusions I wish presently to urge, they must first be disposed of.

Benedict Prevost, after showing that the shining of a cat's eyes is not phosphoric, continues:—"It has also been pretended that it serves as a light to the animals which possess it, and that it assists them in seeing, and guiding themselves in the dark. But the place which the reflectors occupy is reasonably a matter of astonishment; for it is not the light which proceeds from the eye to an object that enables the eye to perceive that object, but the light which arrives in the eye from it."[1] Dr Mackenzie quotes, with approval, this observation, adding the more important comment:—"The purpose served by the reflection of light by the tapetum is not understood. Reasoning *à priori*, we should say it would render the eyes weak and impatient of light."[2] Todd and Bowman observe,—"This *tapetum lucidum* must act as a concave reflector, causing the rays of light to traverse the retina a second time, and thus, probably, increasing the visual power, particularly when the quantity of light admitted into the eye is small."[3] This opinion, Dr Mackenzie thinks "not likely to be just," founding his objection on the absence of a tapetum from the eyes of nocturnal birds, which can see very well in what seems to human eyes absolute darkness.[4]

Prevost's observations seem scarcely called for. If any one certainly has imagined, that the mere lighting up of an animal's eyes would confer upon them the power of vision in darkness, although the light issuing from those eyes was not returned to them from the objects which it rendered visible, the fallacy of his notion deserved to be exposed. But those who held such a view can have had no clear idea of the relation of vision to light from external sources, and it is unnecessary to discuss minutely their opinions. Prevost, however, in dismissing these, appears to think that he has disposed of the question—Does the light issuing from an animal's eyes assist in rendering objects visible to it? and, finding no manner of use for this light, gravely proposes the singular doctrine, that the use of the tapetum is continually to empty the eye, especially during the day, of the superabundant light, which, if not sent off, would lessen its delicacy of visual perception! It is plain, however, that the light thus expelled from the eye, whenever it fell upon a reflecting surface placed in the axis of vision, would in great part be thrown back into the eye, and render visible the object which reflected it. Instead, therefore, of its being a pretence, as Prevost would have us believe, that such an animal as a cat is helped to see by the light issuing from its eyes, it does not admit of doubt that, whether it wills it or not, it cannot help seeing by such light, in so far as it is returned to these organs by the objects on which it falls.

[1] Edin. Phil. Journal, 1827, p. 302.
[2] Physiology of Vision, p. 220.
[3] Physiology of Man, etc., chap. xvii., p. 23.
[4] Physiology of Vision, p. 221.

I am at a loss to understand how this function of the tapetum should recently have been so much disregarded. It seems to have fallen out of notice among our physiologists and natural philosophers, although not sharers of Prevost's views; yet I can conceive no reason for this but the supposed feeble intensity of the light which is emitted from the eyes of animals. It is feeble assuredly, compared with sunlight, or even candlelight, but it is bright compared with darkness, and seems the brighter the deeper the darkness. The light of a cat's eyes is not inferior to that of a glow-worm in intensity: yet the light of the latter is seen from a considerable distance, and close at hand, will enable one, whose eyes have been rendered sensitive by darkness, to distinguish minute objects.

What light of such intensity will do for our eyes, it will do for those of a cat, and none the less because it is returned to eyes from which it was given out. It is true, that if the primary source of the light reflected from the tapetum, and thereafter returned to the retina, were some luminary of great brightness such as the sun, or incandescent lime, or charcoal, the low intensity of the returned light would prevent it from impressing the retina; but in intense light, the tapetum is not needed, and, as will presently appear, does not come into play; and, in faint light, where the difference between the luminosity of the primary rays falling on the tapetum and reflected from it, and that of those returning to the eye, is slight, the retina will retain sensitiveness to both. An eye dazzled by sunlight does not perceive candlelight; but candlelight does not paralyse the retina to the faint phosphorescence of a fish which has begun to decay; and a much nearer approach in the relative luminous intensity of two lights may occur without depriving the eye of the power of discerning both. Of this, one proof specially pertinent to the topic under discussion will suffice.

In subjecting one's eye to the scrutiny of Helmholtz's speculum held in the hand of another, the light of a bright gas flame is thrown into the eye, and reflected from the retina and the choroid behind it (as it would be from a tapetum), through the pupil; but though there is the sense of dazzling from the flame, this does not prevent less brilliant objects from being seen distinctly. These are perceived chiefly by the feebler rays of the flame falling upon them; and I refer to the fact here, simply to illustrate the simultaneous visibility of lights of unequal luminosity; but I notice, in passing, that the light issuing from one's eye, in such an experiment, is brighter than some of the rays of the gas flame, and if reflected from external objects into the eye, would certainly render them visible.

The older physiologists seem fully to have realised this truth, and to have gone beyond its recognition to speculations on the final cause of the existence of a tapetum in the eyes of certain animals. Monro, and as Cuvier states, "d'autres avant lui,—believed that the tapetum of the ox is green, in order to represent more vividly to it the

colour of its natural food."[1] And it was probably a similar opinion that led John Hunter to state that, "in the cow, in sheep, deer, horses, and I believe, *in all animals feeding on grass*, there are in the same eye, certain portions of it [the tapetum] white, and others of *a fine green colour*."[2] Monro, at least, and those at one with him, certainly held, that the green light issuing from an ox's eye was reflected back to it from the grass on which it fed, and made it more conspicuous to the animal. This is a particular example of the general conclusion which I wish to urge—that from the eyes of most animals possessed of tapeta, there issues (especially in twilight or partial darkness) coloured light, by which, in part at least, they see objects before them, and necessarily of a different tint from that which such objects present to the eyes of animals without tapeta. No creatures certainly are more free from colour-blindness, or more keenly sensitive to the difference between red and green, than bulls, rams, and turkeys, which are equally roused to madness by the spectacle of the former colour, and equally composedly regard the latter. Yet the same red rag, or green blade of grass, must present a different tint to each; for the ram's tapetum is much greener than the bull's, and the turkey has no tapetum at all. These are examples of a *chromatic equation* in animals, depending on the colour of the choroid, which will prepare us for similar though less decided differences, due to the same cause, in the human eye; but to this I shall return. Before, however, passing to another subject, there is a probable (I would say, indeed, a certain) use of the tapetum in some animals, which, as it appears to have entirely escaped the attention of naturalists, I briefly notice, although it has no direct connection with the vision of colour. Cuvier observes, in reference to Monro's opinion, that its green tapetum assists the ox in finding its green food, that "this explanation does not apply to other species," as assuredly it does not. A cat preys on nothing green, and can receive no special help in finding its food from the light of that colour which issues from its eyes, although it will, simply as light, assist it in surprising its victims. But if such light guides the cat to the mouse, *it warns the mouse of the cat.* There is mercy to the victim as well as to his pursuer, in the endowment of the latter with a brilliant reflecting choroid. The owl, which has no mirrors in his eyes, and owes his acute sight to his large pupil and highly sensitive retina, can steal unperceived in twilight, on his prey; but the flashing green eyes of the lion, the leopard, and the tiger, must often have given timely though unintended warning of their approach to their anticipated victims; and the shark probably cheats himself occasionally of a meal, through the unconscious announcement of his approach, given by the appalling glare of his immense eyes. But this is a digression.

[1] Leçons d'Anatomie Comparée (leçon xij.); 2de edition, t. iij., p. 419.
[2] Catalogue of Museum of R. Coll. of Surgeons in London, vol. iii. p. 162.

Having then considered the relation of the tapetum to the reflection of light through the pupil on external objects, we are now to look at its relation to the reflection of light from side to side, within the chamber of the eye, with a view to inquire how far vision will be affected by such cross reflections, especially as occurring in the human eye.

The observation of Dr W. Mackenzie (already quoted), that *à priori* reasoning leads us to infer, that the addition of a tapetum to the eye would render it "weak and impatient of light," has great force in it, and has probably been acquiesced in hitherto. A good deal of irregular reflection must occur from a surface so unequal as that of the tapetum is, and the lateral and anterior portions of the retina will receive those scattered rays. It certainly appears, at first sight, as if the resulting multiplication of cross lights could not fail to irritate the retina and confuse the distinctness of vision.

This conclusion, however, I believe, has mainly been drawn from the acknowledged condition of vision in human albinos, where all the evils supposed inseparable from the presence of a tapetum certainly show themselves. But the eye of an albino (animal or human) is, in two important respects, unfavourably circumstanced, compared with that of a cat or other shining-eyed animal. In the first place, the albino's eye has no *pigmentum nigrum* on the lateral and anterior portions of the choroid, any more than on its posterior portion; whereas the tapetum occupies only the latter region, and the rest of the choroid is thickly covered with pigment, and increasingly so anteriorly. In the second place, the iris of the albino is free from pigment behind, and nearly so in front, so that it is transparent, and when contracted to the utmost, offers as large an area for the entrance of light as when it is fully dilated; whereas, in a cat's eye, *ex. gr.*, the iris is virtually opaque, and, when contracted, allows only a very small amount of light to enter.

It is further to be noticed that, except in fishes, and to some extent in the marine mammalia, the area covered by the *tapetum lucidum* corresponds to but a small portion of the bottom of the eye, whereas the arrangement in the albino is equivalent to a conversion of the entire choroid, including the back of the iris, into a reflecting tapetum. Thus John Hunter states, that the tapetum "is *always* placed at the bottom of the eye, in the shape of a half-moon, with the circular arch upwards, the straight line or diameter passing almost horizontally across the lower edge of the optic nerve. . . . . This shape is peculiar to the cat, lion, dog, and most of the carnivorous tribe; in the herbivorous, the upper edge being irregular."[1]

Moreover, limited as the tapetum is, only a small portion of its surface is brought into action as a reflector, unless when exposed to faint light. This is particularly the case in feline animals, where the elongated pupil is at right angles to the tapetum, and narrows

[1] Catalogue of Museum of R. Coll. of Surgeons in London, vol. iii., p. 165.

in sunlight to a small chink, allowing only a thread of light to enter; but it is sufficiently striking also, in the eyes of herbivorous animals, where the pupil stretches as a horizontal slit, parallel to the tapetum, but not opposite to it when the iris is contracted. In the albino's eye, on the other hand, only the closure of the eyelids defends the reflecting surface of the choroid from the access of light. The fullest contraction of the iris is but equivalent to the drawing down of a red window-blind: it alters the quality of the light entering the eye, but not the extent of surface on which it falls.

We must thus set aside altogether the albino's eye, as it presents conditions greatly differing from those of the luminous eye possessed of a tapetum: it is with the *normal* human eye that the animal's shining eye should be compared; and it is this analogy which gives special interest to the discussion.

If, then, we look at the circumstances under which the choroidal mirror, or tapetum lucidum comes into play, it appears that it acts only when the light falling upon it is feeble, and the iris is dilated. It is in permanent action, accordingly, only in fishes, in which the iris is immobile, and the eye is always exposed to a subdued light. In land animals, such as the cat, it is, as it were, thrown out of action except in twilight; and the wide-spread belief, even among the educated, that the cat's eyes shine in utter darkness, is the best proof that the feebler the light, the more fully does the tapetum act.

In the *first* place, then, the light falling upon the tapetal surface of the choroid is of low intensity, and therefore easily absorbed by dark surfaces, when reflected upon them. *Secondly*, As the greater number of the rays of light falling on the tapetum will simply retrace their course when reflected from it, they will pass out of the dilated pupil as they passed in, without impinging on the membranes within the eye. *Thirdly*, Those irregularly reflected rays which do not follow this course, will nevertheless in greater part have a direction forwards, so that they will fall upon the back of the iris, or the more anterior lateral portions of the choroid—the ciliary processes, and the ciliary body—and be absorbed by the pigment, which is thickest and darkest there. *Fourthly*, Of those rays which are reflected from one point of the shining tapetum to another, only such as fall upon the seat of perfect vision (which occupies a very limited area) can materially interfere with the distinctness of visual perception, and they must be very few.

The arrangement, in short, closely resembles that of a *camera obscura* arranged to take a daguerreotype on a silvered plate, with this exception, that the highly reflecting metallic surface corresponding to the tapetum is flat, not curved, so that reflection from one point of the surface to another, which is most to be dreaded, cannot occur. This difference allowed for, it is still manifest that, if reflection from the silvery surface does not prevent the production

of a perfect image on it, the similar reflection from the tapetum should not prevent the production of a perfect image on the retina, which lies like a thin film in front of it.

The presence, then, of a mirror in the back of an animal's eye, appears to be in no respect incompatible with distinct vision; on the other hand, I think there can be no doubt that it greatly enhances it; and I urge this belief because, after the results obtained by Cumming, Brücke, Helmholtz, and others, it does not admit of question that between the *mirrored* animal's eye, and the *mirrorless* human one, there is only a difference of degree. A cat's eye does not more certainly reflect light, than the eyes of every one of us do; and if his sight is disturbed by such reflection, ours must be also. Yet what we call in our own case "perfect vision," is vision thus disturbed, if disturbed it be, and the important question suggests itself, "Does not choroidal reflection increase instead of diminishing the perfection of sight?" According to the current theory of vision it should not improve it. This teaches that the light which has traversed and impressed the retina is thereafter absorbed by the pigment of the choroid, lying behind it, and the more rapid and complete this absorption is, the more perfect, *ex hypothesi*, the picture on the retina. In a word, the whole eye being maintained a *camera obscura*, it is desirable that its posterior wall, or surface, should be *pars obscurissima*. I find no fault with this as a hypothesis; it is probably realised in those sharp-sighted nocturnal animals, such as the lemurs, which have no tapetum, a uniformly tinted choroid, and a very sensitive retina, but it does not apply to the human eye, or to that of a large number of animals. "In those animals," says John Hunter, "where the *pigmentum nigrum* is black, it is pretty uniformly so through the whole; but in those where it is lighter it is not so uniform. The *lightest part is always at the bottom of the eye*, becoming darker gradually forwards, and in such it is often quite black, viz., from the termination of the retina to the pupil; or if not black, it is there much darker than any where else. This is generally the case in the eyes of the human subject."[1]

Later physiologists do not appear to have looked so particularly into this matter as the great surgeon and comparative anatomist did, but all are agreed as to the increased thickness and darkness of the pigment on the posterior surface of the iris (*uvea*), and the internal surface of the ciliary body, as well as between the folds of the ciliary processes. One great object of the uvea unquestionably is to render the iris opaque, and prevent it from *transmitting* light, but it also deprives it of the power of *reflecting* light, and the internal surfaces of the ciliary body and processes must have an equally low reflecting power. The wisdom of this arrangement is apparent. Seeing that, whether for a beneficial end or not, light is reflected from the bottom of the eye towards the pupil, had the uvea possessed a high

[1] Catal. Mus. R.C.S., London, vol. iii., p. 133.

reflecting power, it would have returned the light which fell upon it to the bottom of the eye, and thereby have infallibly disturbed vision. The seat of perfect vision is opposite the pupil, so that if it rapidly contracted, the increased surface of the iris would not merely intercept many of the rays which passed through the dilated pupil, but would, if the posterior surface of the iris acted as a reflector, send them back upon the spot of clearest vision, and confuse the image there. The occurrence of this in the eye of the albino is doubtless one of the causes of his imperfect sight.

There is thus, at least, provision made against vision losing in distinctness from deep-seated reflection within the eye, but I venture to suggest that it *gains* in distinctness from it. Let us look first at an eye with a tapetum, where the reflection is greatest. The first effect of this tissue (which I shall further assume to be colourless), acting as a mirror, will be to return the rays of light which fall upon it, *through the retina.* I have already quoted Todd and Bowman's opinion, that this "will probably increase the visual power," and Dr Mackenzie's objection to this conclusion, because quick-sighted night birds have no tapetum. The majority of physiologists take no notice of the matter, and Todd and Bowman's statement is certainly vague. Can it be doubted, however, that what is equivalent to two rays of light falling upon the retina will produce two impressions? We send a capillary sunbeam through the retina in one direction, and instantly return it through that membrane, a little diminished in intensity, in the opposite direction: if it determined a sensation in its first passage, what is there to prevent its doing so in its second? If, for simplicity's sake, we suppose exactly the same points of the retina to be traversed by the incident and the reflected ray, then (unless the luminous intensity of the incident ray was so great as by its passage to exhaust the sensibility of the retina) the reflected ray will repeat somewhat less powerfully the impression made by the incident one. The difference will be as great as there is between a sound and its echo, but not greater.

On this view of matters, the tapetum, especially in twilight, will serve the important purpose of making every perceived ray of light *tell twice* upon the retina, so that the sensation it produces will either be increased in distinctness, or in duration; and probably in both.

The human eye has no tapetum, and the light reflected from its interior, comes unquestionably in part from the surface of the retina; but the pale surface of the choroid behind the retina also acts as a mirror, and must serve the same purpose as the tapetum in the eyes of the lower animals. The effect of this choroidal reflection on our vision, must be the same as that of tapetal reflection on the visual perception of animals. All optical images will be twice depicted on the retina, and every luminous sensation will make a deeper and more lasting impression upon us, than it otherwise would have done. The pale colour of the choroid at the bottom

of the eye, is thus as beautiful a provision for effective vision, as its dark colour in front.[1]

If those conclusions are well-founded, their application to colour-vision is direct. The tapetum in a multitude of animals is coloured, and from this it follows that the impression made upon the retina by the reflected ray, must differ from that produced by the incident one. Thus, the incident ray in all cases being white; in the felidæ the reflected ray will be *yellow;* in the herbivora, it will be *blue* or *green;* and with both we may contrast the albino, where the white incident light will in great part be reflected as *red* light. Colours cannot appear the same to eyes so endowed. The feline, the herbivorous, and the albino eye, must contribute respecting a yellow, a blue or green, and a red tint, to every object they perceive, and must judge differently of the same colour.

There must be similar, though smaller differences, in the normal human eye, for the choroid varies much in tint in the different races and in different individuals. In the Museum of the London College of Surgeons, there is a considerable collection of preparations from the eyes of fair and dark persons of the same and of dissimilar races, specially intended to illustrate this, and also that the colour of the choroid corresponds to that of the hair. John Hunter thus comments on the subject:—"There are few of the human species that we can say are perfectly white. They rather pass from the black into the brown, red, and even light yellow; and we find the *pigmentum*, ... varying through all the different corresponding shades. In the African negro, the blackness of whose hair and skin are great distinguishing characteristics, the pigmentum is also very black. In the mulatto, who has not the skin so dark as the African, but the hair nearly as black, this pigmentum is of a shade not quite so deep, yet still it does not approach so near to the middle tint as the skin, rather following the colour of the hair. In people of a swarthy complexion, as Indians, Turks, Tartars, Moors, etc., we find the hair always of a jet black, and this substance of a much darker brown than in those that are fair. In people remarkably fair, ... we find this pigmentum following the colour of the skin and hair; being in some of a light brown, and in others [albinos] almost white, according to the colour of the hair in such people."[2]

[1] The objection to such views, drawn by Dr Mackenzie from the absence of a tapetum in nocturnal birds, is valid against those who hold that only by an internal mirror can vision by dim light be secured. I do not advocate this opinion, for, plainly, great dilatability of the pupil, and exalted sensitiveness of the retina, will secure this power for a mirrorless eye; and physiology abounds with examples of similar results secured by dissimilar causes. I would notice, however, that there is an anatomical objection to the occurrence of a tapetum in the eyes of birds, in the occupation of its site by the singular organ called the *marsupium*, or *pecten*, which is supposed to take part in the adaptation of the eye to vision at different distances.

[2] Catal. Mus. R.C.S., Lond., vol. iii., p. 163.

There are thus (excluding the albino) variations of colour in the human choroid from pale yellow to the darkest brown, which must, more or less, be communicated to the light reflected through the retina, from the membrane behind it, and affect the perception of colour. Apart from individual differences in the judgment of tints occasioned by this, there must be constant national ones. The dark and the fair races will present a different "chromatic equation;" the advantage, so far as normal perception of colour is concerned, being probably with the dark-eyed, in whom the deeply-tinted choroid will reflect less coloured light to add its impression to that of light from without, than will be the case with the paler choroid of the fair races.

ARTICLE IV.—*Contributions to Teratology.* By A. MERCER ADAM, M.D., Physician to the Dumfries and Galloway Royal Infirmary, and Member of the Physiological Society of Edinburgh.—(*Continued* from vol. xviii. p. 405).

NO. VII. CONGENITAL LUXATION OF THE FEMUR.

IN the beginning of February 1853, Professor Simpson kindly gave me a malformed fœtus for dissection, which had been sent to him by an obstetrical practitioner for minute examination. On inspection I ascertained that congenital dislocation of the femur existed on both sides; and so interesting were the appearances presented by the dissection, that I consider the following account and delineations of them not unworthy of a place among these "Contributions."[1] For, although this congenital displacement is by no means uncommon, opportunities of examining it in the dead body are, as acknowledged by Dupuytren,[2] comparatively rare, inasmuch as it is merely an infirmity, and not a disease necessarily abbreviating human life.

External appearance of the Fœtus.—I learned that the child had been prematurely still-born, and from its size I considered its intra-uterine life to have ended about the beginning of the eighth month of gestation. The head, trunk, and upper extremities were perfectly normal in their development; but the lower part of the body presented all the characteristic marks of congenital luxation of the femur. The limbs—considerably shortened and atrophied—were disproportioned to the size of the trunk, and obliquely placed in relation to it. They were also rotated very much inwards, and both

[1] Professor Simpson did me the honour of exhibiting my dissection to the Edinburgh Medico-Chirurgical Society, at their meeting on 16th February 1853. (See *Monthly Journal*, vol. xvi. p. 567). I subsequently exhibited the preparation to the Edinburgh Physiological Society. (See *Transactions of the Society* in *Monthly Journal*, vol. xvi. p. 470).

[2] "Les occasions de determiner, par l'ouverture des corps, la nature de cette singulière espèce de luxation sont fort rares."—*Clinique Chirurgicale*, p. 82.

feet were inverted by talipes varus. The femur admitted of very little motion; and on both sides its trochanters formed prominent projections above the site of the cotyloid cavity. The knee-joints were as immobile as if they had been anchylosed. The back could be bent abnormally between the dorsal and lumbar regions.

Dissection.—On both sides the following appearances presented themselves:—

The muscles of the thigh and gluteal region were very much atrophied and contracted, especially where they surrounded the hip-joint. By the retraction of the atrophied and shortened rectus femoris muscle, the patella and head of the tibia were drawn up over the condyles of the femur, thereby rendering flexion of the knee-joint impossible. The head of the femur, displaced upwards and outwards, lay on the dorsum of the ilium, where it had partially

Fig. 1.

Fig. 1.—Dissection showing the displacement of the femur, and the condition of the ligamentum teres.

Fig. 2.

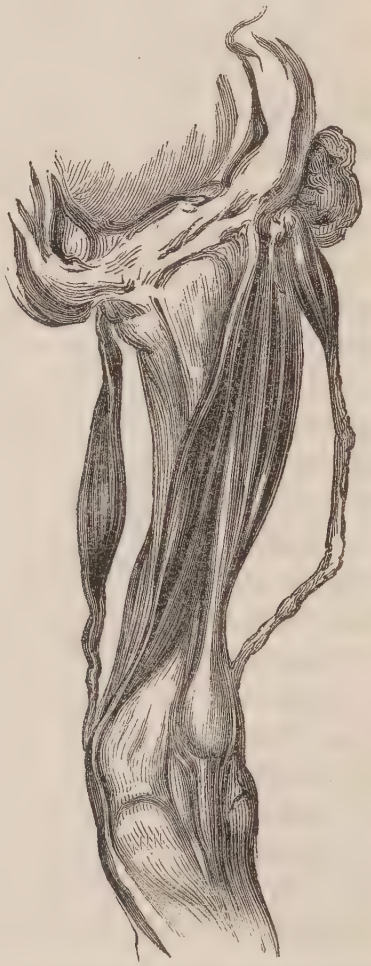

Fig 2.—Dissection of the other limb, showing the atrophied condition of the muscles, and the retraction of the patella.

formed for itself a new cartilaginous acetabulum. The true cotyloid cavity was of the normal size, but slightly irregular in its conformation. The axis of the cervix femoris was abnormal, the neck being

placed nearly horizontally in relation to the shaft of the bone, instead of in its natural oblique direction. The capsular ligament of the hip-joint was lax; and the ligamentum teres was thicker, longer, and stronger than is usual in fœtal life.

On comparing the fœtus with others of the same age, I found the breadth of the pelvis to exceed the normal standard, and the external surface of the ilium to be more than usually convex.

Between the dorsal and lumbar vertebræ a false joint was formed, strengthened by abnormal fibrous development.[1]

Remarks on the foregoing Case.

In congenital luxation of the femur, the displacement, as in the preceding case, occurs generally on both sides. Baron Dupuytren, who first accurately described the pathology of this affection, remarks that he only observed it confined to *one* side, in two or three out of twenty-six cases he had examined;[2] and, according to Melicher,[3] in four out of nine cases witnessed by Chelius, the luxation was unilateral. The experience of Mr Adams of Dublin[4] coincides with this; and the comparative rarity of one joint alone being implicated, constitutes, in his opinion, an important diagnostic mark of the affection.

The site of the displaced bone in the present case is the one usually met with in congenital dislocation. In malformed children the femur has occasionally been found luxated directly upwards, so as to lie immediately in front of the anterior inferior spinous process of the ilium; and it has also been observed to be congenitally displaced upwards and forwards, so as to rest on the ileo-pectineal eminence. Melicher[5] made several experiments on the dead subject with the view of ascertaining in what direction the femur is most easily displaced in fœtal life. By luxating the bone artificially in infants of various ages he found that the dislocation downwards and backwards into the ischiatic notch was more easily caused than any other variety. He experienced most difficulty in producing luxation upwards and outwards, only succeeding in two cases out of thirty in which he attempted it; and he was quite unable to effect displacement either into the foramen ovale, or upon the pubis. He is of opinion that these experiments warrant the conclusion that in congenital luxations, the head of the femur generally emerges from the cotyloid cavity at its anterior and inner margin, and that, thereafter, by a second dislocation produced by muscular action, it

[1] The dissection here described was given by me to Professor Goodsir (to whom I take this opportunity of publicly expressing my warmest thanks for the advice and assistance I received from him during many of my Teratological investigations), and may now be seen in the Anatomical Museum of the University of Edinburgh.

[2] Loc. cit.

[3] Dr L. J. Melicher, *Die Angeborne Verrenkungen.* Vienna, 1845.

[4] Cyclopædia of Anatomy and Physiology, vol. ii.

[5] Op. cit.

becomes tilted upwards and outwards upon the dorsum ilii. The displacement in the case under notice was, as we shall afterwards show, the one which would be most easily produced, by the influence of the contracted gluteal muscles, in the intra-uterine position of the fœtus.

Another point of great interest in this case, was the condition of the round ligament. It was found to be not only elongated, but thicker and stronger than is usual in fœtal life. These appearances seemed to confirm and illustrate the views entertained of its functions by Professor Goodsir, who regards it as a purely *suspensory ligament*, destined, sling-like, to prevent the whole pressure of the trunk from being thrown upon the head of the femur. The condition of the round ligament in this case is all the more remarkable and interesting, when we reflect that it was observed in a joint which had never been called into action, and that it evidenced an ingenious provision made by nature to counterbalance the effects of the displacement; for, had this child lived, nearly the whole weight of the body, owing to the head of the bone not being in the acetabulum, must have been supported by the ligamentum teres, which, accordingly, was formed of unusual strength, to fit it for this function.

I am not aware of any author who, in treating of congenital luxation of the femur, has specially directed attention to the condition of the ligamentum teres, or who has noticed elongation and thickening of it as peculiar to the displacement. Dupuytren[1] says that occasionally the ligament may be seen, but considerably changed. Elongation of the ligament has certainly been observed in dissections of the *adult* subject. Dr Heine,[2] of Canstatt, in a dissection he made of the lesion in a patient, æt. 40, found the round ligament wanting, and its place of insertion into the femur covered with cartilage. M. Bouvier[3] exhibited to the French Royal Academy of Medicine, a preparation of the congenital dislocation of the femur, taken from a woman, æt. 53. He described the ligamentum teres as "much elongated, flattened, and partly confounded with the capsule of the joint." But the dissections of Heine and Bouvier, as well as most of those executed by Dupuytren, having been made upon *adult*, not upon *fœtal* bodies, the appearances of the joint described by them cannot, with justice, be regarded as demonstrative of the truly *congenital* condition of the structures concerned in this deformity, for it is easy to understand that the parts would undergo considerable alterations during the lifetime of the individual, becoming more and more changed, by spontaneous adaptation to the abnormal circumstances in which they were placed. But appearances of the round ligament, analogous to those I have described have been observed in infancy; although, so far as I am

[1] Loc. cit.
[2] Uber Spontane and congenitale Luxationen, Stuttgard, 1842.
[3] Bulletin de l'Academie Royale de Médecine. February 1841.

aware, only by one author. Parise,[1] in a case of congenital luxation in an infant ten weeks old, describes the ligament as being "*long* and on the stretch;" and, in another dissection of the displacement, in a child aged six days, as being "*longer than ordinary.*"[2] I am strongly inclined to believe that the increased length and strength of the ligament, observed in cases examined in adult life, are merely its congenital conditions exaggerated by dint of usage; and that more extended future researches will demonstrate these appearances to be present at birth in nearly every case of the displacement.

In order to ascertain how far the present case goes to elucidate the etiology of this congenital luxation, it is necessary, very briefly, to allude to the various views which have been held in regard to its causes. These displacements have been considered due to,

1.—*Mechanical Violence during Gestation.*

Hippocrates and the older authors were of opinion that this luxation occurred *in utero* traumatically, from the operation of external mechanical causes, such as falls, blows, and muscular contractions, or preternatural smallness of the uterus. But we now know that this displacement is very unlikely to result from these causes alone; and, moreover, that luxation is rarely, if ever, found in cases where such injuries have been received by the mother during pregnancy. Pathologists have therefore ceased to regard mechanical violence as a cause of pure congenital dislocations.[3]

2.—*Defective Organization of the Early Embryo.*

Dupuytren and others consider congenital displacements to have existed, as such, from the earliest period of intra-uterine life, and to be due to an aberration of the *autotrophy*, or power of self-development inherent in the fœtus. But this view seems untenable and inconsistent with what we know of embryo-genesis.

3.—*Diseased and Abnormal conditions of the Hip-Joint.*

a. Defective Development of the Acetabulum.

Breschet conceived congenital luxations to be the result of irregular or insufficient formation of the cotyloid cavity. This explanation is by no means satisfactory; for, in many instances (as in the present case), the acetabulum is found to be of normal size. Thus, in three cases examined by Parise, the cavity was found[4] to be of

[1] Archives Générales de Médecine. Tome xiv. p. 439.

[2] Ibid. Loc. cit., p. 546.

[3] Undue violence on the part of the accoucheur, during parturition, may undoubtedly occasion luxations in the fœtus, although this rarely occurs; but such obstetric injuries cannot, strictly speaking, be called congenital affections. In the above remarks, therefore, I have avoided making any allusion to mechanical violence exerted by the obstetrician.

[4] Loc. cit.

ample size and regular conformation, and in one of the cases (to which allusion has already been made), occurring in a child ten weeks old, the head of the bone, displaced upwards and outwards, rested on the crest of the acetabulum, where it had formed for itself a cartilaginous depression, which communicated with a very large and normal cotyloid cavity.

b. Morbus Coxarius.

This has often been assigned as one of the causes of intra-uterine dislocation of the femur; but the theory is not supported by dissections, which show the disease to be comparatively a rare one in the fœtus. Instances are on record, however, in which congenital luxation is said to have resulted from this affection; but these are very few in number, and Melicher[1]—the latest and best authority on the subject—says that he never saw a case of truly congenital luxation occasioned by it.

c. Hydrarthrosis.

We have already, more than once, made reference to results stated in a paper written by M. Parise, in volume xiv. of the *Archives Générales de Médecine*. The object of this admirable monograph was to prove that dropsical effusions into the cavity of the acetabulum were the usual and principal causes of spontaneous dislocation of the head of the femur. He found that artificial injections of the hip-joint had the effect of causing this luxation; and he reasoned analogically, that a preternaturally great amount of synovial secretion would produce, pathologically, what he had done experimentally. That hydrarthrosis may be an occasional cause of congenital luxation of the femur, I do not doubt, but that it is by any means a constant or uniform accompaniment of the lesion, appears to me highly improbable. Indeed the testimony of Parise himself seems to destroy the exclusiveness of his theory; for, out of 332 cases, in which he examined the joints of new-born infants, during the time he was house-surgeon to the *Hôpital des Enfans Trouvés*, at Paris, he only found *three* in which he could distinctly trace congenital dislocation to this cause.[2]

d. Hypertrophy of the Haversian Gland.

Parise and others believe that the displacement may also result from an increased growth of those adipose folds, generally found within the synovial membrane of the hip-joint, which have been somewhat erroneously denominated "Haversian *Glands*," but which are, in reality, merely vascular accumulations of fat-cells. In support of this theory, these authors have related cases of congenital

[1] Op. cit.

[2] It was in the dissection of one of these cases that he observed the elongation of the round ligament to which I have already alluded.

luxation, in which there existed visible hypertrophy of these masses; but the assertion that, in these cases, the displacement was caused by the enlargement of the Haversian glands appears to me a most unwarrantable application of the *post hoc ergo propter hoc* line of argument. The increased growth of the adipose substance in these instances seems to be rather the *result* than the *cause* of the luxation; for, had the head of the femur remained *in situ*, there is every likelihood that its presence and pressure would have restrained that hypertrophy, which occurred exuberantly as the sequence of its displacement.

e. Muscular Retraction.

In the great majority of cases I believe congenital luxations to be occasioned by the influence of muscular retraction. Guerin was probably the first who suggested this view of the etiology of the affection; and in 1840 he thus wrote:[1]—"Congenital luxations of the hip are, like talipes, torticollis, and spinal deviations, the product of primitive muscular retraction; and the kind of the dislocation depends upon the direction in which the retraction happens." Melicher and Dr Carnochan of New York—the two most recent writers on the subject—have since adopted the opinions enunciated by Guerin; and the appearances presented by the dissection I have described, seem to corroborate the views entertained by these authors regarding the etiology of the displacement. We know that many pathological conditions are produced by muscular retraction, whether dependent upon spasm or upon shortening and atrophy, both in fœtal and in extra-uterine life. The hip-joint is, of all the articulations, the one most liable to congenital luxations; for the position assumed by the fœtus *in utero*, where it lies with the legs flexed on the abdomen, is the one of all others which predisposes the head of the femur to slip out of the acetabulum, and to mount upwards and backwards, as in this case, upon the dorsum of the ilium. It is easy to understand, therefore, that if muscular force should be exercised upon the head of the femur, while the fœtus lies in this position, that the bone would be displaced from the cotyloid cavity. The atrophy and shortening of the muscles, in this case, sufficiently account for the luxation which occurred. In some cases, however, displacement may exist without any visible abnormality of the muscular system. The retraction, in such cases, is probably dependent upon that super-polarity of the nervous system to which infancy is so prone, which may result from either a perverted condition of the cerebro-spinal centres, or from some obscure irritation, local or remote, communicated to them, and thence transmitted by reflex action, to the parts wherein the morbid action is developed.

[1] Gazette Médecale de Paris, 1840, No. IV.

ARTICLE V.—*Two Cases in Midwifery, with a Description of the Internal Surface of the Uterus after Delivery.* By J. CHISHOLM, M.D., Ratho.

Case of Puerperal Convulsions without Albuminuria.

EARLY on the morning of the 8th of October 1852, I was requested to visit Mrs ———, æt. 38.

The account given by her friends was, that she had been delivered of a healthy child at the full time, about thirty hours ago; the labour, her seventh, had lasted only two hours, and she had called in no medical attendant. She had continued quite well until about six hours ago, when she was seized with convulsions, during the night, while the rest of the family were in bed. This attack lasted for a few minutes, and was followed by stupor, from which she gradually emerged, but about four hours afterwards she had a second attack, which lasted upwards of half an hour; this time the convulsions were so severe, that four people were unable to hold her still; and now, one hour and a half after the convulsions have ceased, she is lying in a state of profound coma, with stertorous breathing, and a bloody and frothy mucus issuing from the mouth; she is quite insensible to pinching, or loud sounds, excito-motory movements of the legs are readily enough produced when the soles are tickled; the pupils are slightly dilated and contract sluggishly when a lighted candle is held near them; there is no squinting.

The skin is everywhere slightly anasarcous, but this is most marked upon the arms and under the eyelids, where it pits distinctly on pressure.

The respirations are very laboured, and eighteen per minute. The pulse is sixty, full, firm, and distinct. The tongue is slightly furred, and severely bit at three different places. The bowels have not been moved since her confinement; there is no unnatural hardness or distension felt over them.

The uterus feels firmly and equally contracted, and of about the ordinary size after delivery; the lochial discharge still continues, though it has always been scanty; the mammæ contain no milk.

The urinary bladder is empty. She is reported to have passed urine freely and frequently enough since her confinement.

I put one drop of croton oil on the back part of her tongue, ordered her hair to be removed, and leeches to be applied to the temples; she is also to have sinapisms applied to the calves of the legs; visited her again after a space of four hours, and found her gradually emerging from her coma; she now answers questions when spoken to, with a loud voice. She has not been purged, and she has not passed any urine; I now made her swallow one compound colycynth pill, containing 1 minim of croton oil. Ordered her to take a dose, of a mixture containing sulphate of magnesia and vinum colchici, every two hours, till freely purged, and to have cold applied to the head. Saw her again in the evening, and found her slowly improving; she had been freely purged, and I drew off with the catheter ℥xij. of urine; she is now to take, instead of the colchicum with the sulphate of magnesia, a mixture of vinum colchici and sp. æth. nit.

The urine was carefully examined, and, much to my surprise, was found to contain no albumen, its specific gravity also was good, being 1027. After standing, it threw down no deposit, save a small quantity of cast-off epithelium from the urinary bladder.

October 9th.—During the night she had three returns of the convulsions, but none of them severe or long continued; she now complains of headache, and is rather drowsy, but still seems better than when seen last night; she has passed about ℥xxv. of urine within the last twelve hours, and it still contains no albumen, its specific gravity is 1020. The anasarca is almost quite gone.

October 13.—She has been improving rapidly since last report; she has had no complaint except of her tongue, and of various parts of the body, which had been bruised and strained during her struggles; the anasarca is quite gone, and

the urine, which has been several times examined, was always found perfectly normal, when care was taken to obtain it free from the uterine discharges. She has now no recollection of her illness, and even remembers nothing of the birth of the child, which took place twenty-four hours before the first appearance of convulsions or coma.

The most curious circumstance connected with this case was the anasarca; it was, however, probably merely accidental, for puerperal convulsions do occur without being accompanied by albuminuria, and we sometimes see anasarca without being able to detect albumen in the urine, or any organic disease to account for it. This patient had never been subject to fits formerly, and has continued quite well till now.

Case of sudden Death on the seventh day after Delivery.

On the night of the 12th of January last, I was called to attend Mrs M—— in labour; she was a feeble, delicate-looking woman, æt. 33, and previous to this, her sixth confinement, had suffered much from general debility.

She had been about four hours in labour, and the membranes having ruptured at the very commencement, the liquor amnii had been escaping slowly ever since; the pains were reported to have been less severe lately than they were at an earlier stage of the labour; on examination I found that it was an arm presentation, the back of the fœtus being turned towards the abdomen of the mother, the left elbow protruded into the upper part of the vagina, and the body of the child pressed closely on the brim of the pelvis. The os uteri was pretty fully dilated, and the pelvis well formed and capacious. After having emptied the urinary bladder, I slowly introduced my left hand, and having succeeded in laying hold of both the feet of the child, experienced no difficulty in turning; this was effected much more easily than had been anticipated, for a considerable quantity of the liquor amnii still remained in the uterus. Some delay occurred before the expulsion of the head was effected, and the child, a fine, full-grown male, was still-born. The placenta was separated naturally a few minutes afterwards, and the uterus remained firmly contracted, not more than the ordinary amount of hæmorrhage took place; the woman was rather exhausted, and we administered some spirits along with an opiate.

January 13.—She had a sound and refreshing sleep after her opiate, and she now feels quite as well as she used to be at the same length of time after any of her former labours.

The lochial discharge is moderate in quantity and the uterus feels firmly contracted; she has suffered a little from after-pains, but they are not at all severe; she has passed urine freely and without pain. All the other functions are natural.

She was visited again on the 14th, 15th, and 17th, and progressed favourably, without any particular symptom, except, that on the night of the 14th, she suffered a good deal from a pain in the left side; this, however, entirely disappeared after a dose of castor oil on the morning of the 15th.

On the morning of the 19th, I was requested to see her as soon as possible, as she had been seized with a violent pain in her back. On my arrival, I found that she had died shortly after the message had been sent; the account given by the friends was, that while sitting up in bed, after having taken her breakfast as usual, she was seized with a pain in the back, which made her scream out; shortly afterwards she fell back fainting, and having made a very imperfect rally from this, soon fainted again and expired.

Sectio Cadaveris.—Twenty-six hours after death, body generally, but more especially the lips and face, unusually pale; at the posterior parts of the body very slight post-mortem sugillations, rigor mortis scarcely commenced, subcutaneous fat small in quantity, muscles pale and soft.

Pericardium contains about two drachms of pale straw-coloured serum, two whitish opaque spots of a small size on the posterior surface of the heart.

Heart, not contracted, normal in size and shape, contains small soft semi-decolorised clots in all its cavities; valves all normal; tissue rather softer and paler than usual, but the colour is uniform throughout. The muscular substance was subjected to a searching microscopic examination, and found to be strictly normal; coronary arteries normal. The aorta and some of the larger arteries were found to contain traces of atheromatous deposit, but not more than is common at her age, and it was not in an advanced state.

Pretty firm adhesions over the apices and posterior margins of both lungs. Both lungs present a considerable extent of emphysematous tissue along their anterior margins; there are also portions of collapsed and atrophied tissue at various parts of both lungs; mucous membrane of larger bronchi normal.

Near the centre of the greater omentum, there is a thickened, puckered portion, it is of cartilagenous hardness, and contains a quantity of dark matter infiltrated into its substance. This has all the characters of an old hæmorrhage undergoing absorption. Immediately under the peritoneum, which is raised up to a considerable extent, and in contact with the upper part of the left kidney, there is a large clot of dark grumous blood, having a semi-fluid tarry consistence, it extends upwards for three or four inches, and inwards beyond the mesial line; its margins are, for the most part, distinctly circumscribed by healthy tissue, and do not present any traces of inflammatory action. The clot is easily turned out of the cavity it has formed. No ruptured vessel can be found. The clot weighs probably eight or ten ounces.

On both sides over the brim of the pelvis, upon the posterior surface of the urinary bladder, and in both the lateral ligaments of the uterus, the sub-peritoneal tissue has a dark, livid, bruised appearance; when cut into, it is found not to extend deep, a bloody serum exudes from the cut surfaces, and no clots can be discovered at this part. This ecchymosis is gradually lost in the surrounding healthy textures, and nowhere presents a distinct or well-defined border.

Liver—soft, pale—its cut surface having a mottled aspect, tissue friable, and presents all the ordinary characters of fatty liver. On microscopic examination, the cells are found to contain an excess of fatty granules. Specific gravity 1030.

Uterus, size of a cocoa nut; peritoneal surface quite smooth and normal, thickest part of walls upwards of half an inch; the cavity contains a small quantity of tenacious mucus; portions of the uterus were laid aside for further examination.

All the other abdominal organs are pale, but present no abnormal appearance. Head not examined.

Description of Internal Surface of Uterus.

As already mentioned, portions of the uterus were laid aside for more careful examination, but from some accident, that portion over which the placenta had been attached was not secured.

The other portions were covered with a thick tenacious mucus. This, when examined microscopically, was found to contain much mollecular and granular matter, a few pus corpuscles, and about as many red blood corpuscles. There was also a considerable proportion of nucleated cells variable in size and shape, but all more or less closely resembling the normal epithelial cells of the uterus; these were most numerous when a scraping was taken from the internal

membrane with some degree of force. None of the muscular fibres could be obtained by scraping unless the tissue were torn up.

The internal surface itself was of a pale rose colour; when examined with the naked eye or low powers of the microscope it seemed quite smooth, and this smoothness was equally apparent when it was examined under water; it did not present that ragged aspect we must have found if the muscular tissue had been laid bare. There were numbers of minute oval and circular depressions studded very regularly over the internal surface. These were distinctly visible to the naked eye, and the largest of them might have admitted the head of a small pin. Little more concerning these depressions could be made out with the microscope, but we have found similar appearances constantly presented by the mucous membranes of pregnant uteri among the lower mammalia, and no one can for a moment doubt that they are the openings of the follicles of the *mucous membrane*.

One portion was dried, and when thin sections of this were placed under the microscope, it was found that a structure, assuming the appearance of a narrow clear line, existed internal to the muscular fibres. Another portion was macerated in water, and after it had lain for three weeks, a membrane could be moved separately upon the muscular tissue, and small flaky portions of it could be raised up with the forceps.

We cannot speak confidently concerning the state of the mucous membrane over the site of the placenta, but from various examinations of the placenta as it is expelled at the full time, we are led to believe, that in all, or almost all cases, if the uterus were examined soon after delivery there would be found portions of those placental tufts which have pierced the decidua, remaining in the maternal sinuses.

ARTICLE VI.—*On a Case of Sudden Death.* By ANGUS J. MACKAY, M.D., Edinburgh.

A. T., æt. 32, by occupation a postman, had been drinking very hard on the evening of Thursday, 1st December 1853. He was taken home drunk at six o'clock, and put to bed. At that time he was so far sensible as to be able to speak a few words to his wife. At eleven o'clock that same night he was shifted from one bed to another by a policeman, for some reason or other which I did not learn. He was then quite insensible, and the only evidence of his being in life was his breathing.

His wife was advised to send for a doctor, but she thought that there was no occasion, as she imagined that a few hours' sleep would put him all to rights. Consequently, she did not do so; but went to bed confident that he would be quite well next morning, and, I dare say, composing a lecture to give him on his most unchristian behaviour!

At three o'clock on Friday morning she awoke, and was alarmed at finding him cold and motionless. She immediately got up and sent for the nearest medical man, Mr Brown of Dublin Street. Mr Brown instantly went to his house, but

on his arrival there found him quite dead; and, from the coldness of his body, concluded that he must have been so for several hours.

This man had for many years been a sober, steady, industrious person; during the day going about delivering letters, and at nights working at his trade —that of a tailor. Gradually, however, he had got into the habit of drinking, and latterly he had become a confirmed drunkard. This unfortunate habit may have been occasioned by his place of residence, which was with his father-in-law, who keeps a spirit shop.

On Saturday forenoon I accompanied Mr Brown and Dr M'Gregor to an examination of the body. To this examination the friends made no objections. The following was the result :—

External Appearances.—No emaciation. No unusual discolouration of the integument, excepting slight blueness about the neck and shoulders, which might easily be accounted for by the position in which he was lying. The face pale. The features perfectly composed and tranquil. Rigor mortis considerable.

Chest.—Pericardium healthy. Heart normal, excepting slight comparative thickening of the walls of the left ventricle, and of the roots of the columnæ carniæ. It contained about *six* ounces of blood, both sides containing pretty nearly the same quantity. The lungs were both emphysematous and slightly congested, and the left was firmly adherent, consequent on a previous pleurisy.

Abdomen.—Peritoneum healthy. Stomach distended with half-digested matter, which had no odour of alcohol or any thing unusual. The mucous membrane was of a pale ashy hue, with here and there slightly congested patches. There was no obstruction of any kind. The liver was greatly enlarged, but a section presented its normal characters. There were no gall stones. The kidneys were quite normal, and the bladder contained nothing but urine.

Head.—The membranes of the brain were quite healthy and moist. The surface of the cerebral hemispheres was slightly congested, and on section presented rather more red spots than usual, but nothing of any consequence. There was no appreciable quantity of fluid in the ventricles or base of the brain, and the texture of the organ was of its usual consistence.

This concluded the examination; but we brought away with us for analysis the stomach and its contents, and part of the brain. These were given to Dr Douglas Maclagan, who carefully analysed them, but could find no traces of poisoning.

Remarks.—Now, of what did this man die? This is no easy question to solve. Before the sectio, Mr Brown and Dr M'Gregor had almost made up their minds to find disease of the heart, and in this idea I participated. But, as I have mentioned, the heart was normal. The suspicion of poisoning, which we afterwards entertained, was done away with by the result of chemical analysis.

The best way to come to a decision upon the question is, in my opinion, shortly to run over the different kinds of death, or modes of dying, when by comparing each of them with the case before us, we shall be able to fix upon that one which most resembles it.

I shall go over the different modes of dying as they are given by Dr Watson, in his excellent work on the Principles and Practice of Physic.

The *first* form of death which he mentions is that by "anæmia," which he describes as being caused by a want of the due supply of blood to the heart. "The circulation fails, not because the heart has lost its power of contraction, but because blood does not arrive in its chambers in sufficient quantity." On dissection after death

the heart is found empty and contracted. It is evident that this cannot have been the mode of death in this case, for the heart was found full and not contracted.

Second, Death by "apnæa." At first sight we would be apt to suppose that this was the mode of death, as suffocation often happens in intoxicated persons from a piece of meat, etc., becoming accidentally fixed in the glottis. But, though in the dissection we neglected to examine this part, and consequently I am unable to state *from observation* that there was no such obstruction, yet *from the history* we may safely conclude that such was not the cause of death. For, mark you, he was put to bed at six o'clock, and at that time he spoke; after that hour he tasted nothing, and he was seen by a policeman at eleven o'clock, when no symptoms of suffocation were observed. And the fact that he died during the night without awakening his wife, is almost sufficient of itself to do away with this idea. The appearance of the features, too, after death, oppose this view; for, instead of a flushed and turbid face and distorted features, the countenance was pale, placid, and tranquil.

Third, Death by "coma." We naturally suppose that this was the mode of dying, we almost instinctively suspect apoplexy in such a case as this. But there was no amount of congestion of the brain, and as death by coma is, at its termination, exactly the same as death by apnæa, the arguments which I have used against the one apply to the other also—no appearance of suffocation, no very marked irregular or stertorous breathing, and no turgidity or lividity of the face.

And now we come to the last form of death, viz., that by "asthenia." This is exactly the converse of death by anæmia; death takes place, not by a deficiency of the stimulus—the blood—but by a total failure of the heart's contractile power. On dissection after this kind of death, "each chamber of the heart is found to be filled with its proper stimulus upon which it has been unable to contract."

Now, looking back at the appearance of this man's heart, we find it exactly agreeing with this description. It contained about six ounces of blood, each side containing pretty nearly the same quantity. I am inclined to believe, then, that failure of the heart's contractile power was the cause of death.

Alcohol, like most other stimuli, produces three different effects—or its effects are divided into three different stages. The *first* is slightly stimulant, the pulse is increased in frequency and it is fuller and stronger, the ideas are brighter and more vivid. The *second* stage is that of over-excitement, characterized by incoherence of talk, etc., and a man in this state is said to be "tipsy" or "elevated." The *third* stage is that of depression, characterized by loss of voluntary motion, weakness of pulse, insensibility, etc., and a man in this state is said to be "dead-drunk." This state of depression is generally followed by one of reaction, the system gradually recovering its normal tone.

Now, it appears to me, that, in this case, the depression was too

great to admit of reaction, not only were the voluntary muscles paralyzed, but the involuntary also; the respiratory muscles did not act properly, hence the engorgement of the lungs, and the muscular structure of the heart was paralyzed too, hence the quantity of blood found in that organ. But it may be said, surely, if such was the cause of death, alcohol would have been discoverable in the contents of the stomach, or in the brain. I cannot account for this not being the case. And it may also be said, that the congestion of the brain, slight as it was, disproves this idea, for if the man died of depression, surely the brain would not be engorged.

Now, there might perhaps have been a slight amount of apoplexy coexistent with the asthenia, but still not sufficient to do away with the asthenia as the cause of death. For, observe the natural and usual *result* of the *second effect* of alcohol is cerebral congestion, giving rise to all the various phenomena of over-excitement; and the natural *result* of the *third effect* of alcohol is cerebral depression, causing faintness, nausea, and insensibility. Now, in this case, it would appear that the effect of the alcohol ran rapidly on, so as, as it were, to paralyze the heart by *shock*, before the congestion caused by the second stage had time to subside.

Part Second.

REVIEWS.

Balneologisch Verslag van het Badsaisoen 1853, door P. M. Mess, Med. Chir. et Artis Obst. Doctor, dirigerend Bad-Arts bij de Zeebadinrigting te Scheveningen.

The Bathing Season at Scheveningen, in 1853. By Dr Mess, Superintending Physician.

Hygeia has scarcely a gushing fountain within the confines of Europe, beside which the native of Great Britain may not occasionally be found lingering, either on the score of the health which he anxiously strives to gain, or on that of the *ennui* which he as anxiously strives to lose. Recognised at once, wherever he presents himself, and addressed instantly in broken English before he has time to reveal his own too prevailing destitution of any but his native speech, he is every where received with a welcome which, we fear, depends now less upon his reputation for liberality, than upon the innate courtesy of the foreigner.

The truth is, that many Englishmen (and here we respectfully intimate to those of our readers who are members of the Scottish

Rights Association, that we do not desire the Scotchman to be included) are too apt to display a haughty reserve, somewhat disproportioned to their peculiar merits, and a suspicion of being cheated abroad, which is but indifferently in correspondence with the standard of purity at home, such as do not certainly deprive them of that kind of esteem which is founded on respect, for that the force of national character insures to them, but strip them of much of that which is founded on affection. The Scotchman, on the other hand, is usually far more cosmopolitan in his habits and manners; and adapts himself with infinitely greater tact to the rules of the particular form of civilization with which he may be brought in contact. If he evince shyness, it is that of timidity and not of pride, and a short familiarity is sufficient to sun it into heartiness. It is thus, we can assure our readers, and we speak upon a tolerably wide experience, that the Scotchman, whether as an occasional visitant, or as a continuous resident on the Continent, becomes more readily part and parcel of the existing social system, than his equally estimable, but somewhat more exigent, brother of England.

But whether we congratulate our countrymen, in the stricter sense, on this inherent happiness of disposition, which renders them at once staunch patriots at home, and gracious citizens of the world abroad, or deprecate a tendency of an opposite description in our valued neighbours, we may rest assured that the tide of foreign travel will ever continue to roll backwards and forwards from amongst us, and with ever increasing torrents as the channels become wider and less obstructed. When our railway officials can add to the speed of their trains the security and commodiousness of those on the Continent, and can find leisure to frame arrangements with consideration which they design to enforce with courtesy, and when improved harbours and steam-vessels add farther ease and safety to our other means of transit, it seems scarcely possible to assign a limit to the current which must pass, hitherward and thitherward, between us and the various, even the remoter, states of the Continent. The tourists who now, in search of health, leave Harrogate and Buxton for Baden and Carlsbad, may then also quit Scarborough and Hastings for Scheveningen or Heligoland, or may seek for tideless waters at Doberan or at Venice. Partly with a view to this possibly extensive migration of our sea-bathers, and partly with the object of laying before our readers a slight sketch of the prevailing arrangements of a foreign watering-place, we have selected the report of Dr Mess as the ground-work for a few remarks.

Scheveningen, one of the most frequented of the sea-bathing places of the Continent, lies on the coast of Holland, open to the full sweep of the waves of the German Ocean. The beach is composed of a fine, compact sand, abounding with minute fragments of shells, but without a single protruding rock, or even a pebble, to wound the naked foot of the bather. So gentle are the gradations

of descent towards the depths of the sea, that there is no hour of the tide, whether of flux or of reflux, at which the beach is not accessible; so that the choice of a period for taking the bath can be usually regulated with the precision which, in many cases, is so requisite for its beneficial employment. At a short distance inland lies the agreeable city of the Hague, with its numerous attractions: the intervening space being principally occupied by a fine tract of woodland, skirted by lines of villas, and intersected by paths and drives, which afford easy access for walks or for carriage exercise, with the advantages of quiet retreat, and of shade or shelter. The village, or town of Scheveningen itself, is one of considerable dimensions; and its extended main street, with which the visitor will chiefly concern himself, is lined by a series of neat residences, the larger portion of which are open for hire as lodgings during the season. The scientific expounder of the qualities of any place of resort for the invalid, rarely condescends to add to his few details of physical geography or of climate, any information as to the scarcely less essential points of accommodation or of cost, by which the infirm wanderer may be enabled to judge at what possible sacrifice of comfort or of means he is to proceed in search of alleged advantages, which may be so far counterbalanced by other conditions as to be rendered questionable. Our lighter and less dignified pen may attempt, in some measure, to supply this defect. Good lodgings, consisting of from two to three apartments, usually, however, of very moderate dimensions, may be procured in Scheveningen at from 50 to 100 guldens, or from about four to eight guineas a month; this sum including attendance, and one or two other advantages. As to meals, it is usual either to dine at a public table, or to have the dishes sent in from a cook-shop, the charges at which are not exorbitant. Breakfast and tea will be prepared within the lodgings, and the price of provisions is sufficiently moderate to ensure these meals, with bread, butter, and eggs, at the cost of a few pence. Many excellent wines, including port and sherry such as rarely grace our tables, may be had at half their price in this country.

But it is on the margin of the beach, at a distance of about ten minutes' walk from Scheveningen, that is to be found the principal bathing establishment of the place; an institution which has been founded by government, and the details of which have been devised with a perfection which is not unworthily carried out in practice. The main edifice contains about sixty apartments, devoted to the various objects of the institution; and such has been its success, and the approval of the public, that a large addition of forty new apartments has been found requisite, and is already resolved upon. Part of the existing accommodation consists of a handsome dining-hall, a tea-room, and a reading-room, the latter of which is supplied with a fair proportion of English and Continental newspapers. The baths within the building are either of hot or cold sea-water, and it is usual to take a few of the latter before proceeding to bathe in the

open sea; a precaution which may be often both safe and necessary. The warm baths are charged singly at a gulden; and ten may be subscribed for at seven guldens, or somewhat less then a shilling for each. There is also provision made for different descriptions of medicated baths, as with the *sapo medicinalis*, sulphuret of potass, the mother-water of Kreuznach, carbonic acid gas, etc., etc., as well as for douches, ascending and descending, of different degrees of force. Visitors are accommodated with apartments within the building, the charges for which vary according to the nature and extent of what is required. A neat room, looking towards the terrace and the sea, and measuring some 18 feet square, with bed and other requisite furniture, may be had for about twelve shillings a week. Others, of a more handsome description, cost from eighteen to twenty-four shillings; while sitting-rooms, more or less large and elegant, but which Continental usages render only necessary for the high in rank and affluence, cost from twenty-four to fifty-eight shillings a week. Breakfast within the establishment costs a shilling, and dinner at the table d'hôte half-a-crown; the fare at the latter being sumptuous and plentiful. Lastly, to close our chapter on economics, for which we do not apologize, because we really account such information valuable, and but too rarely accessible, the sea-baths, with a commodious machine drawn by a horse, are charged at the rate of twenty-five for a pound sterling; while, with a smaller machine, drawn by an attendant, the charge is threepence for each bath.

Scheveningen has few resources of its own for amusement, besides those which we have already noticed; though we ought not to omit the frequent attendance of good bands of music from the Hague, which usually attract along with them considerable numbers of the higher class of citizens, and constitute the centre of a gay and animated scene, where groups, enjoying refreshments, are seated round tables, thickly clustered on the open terrace in front of the establishment. But the prompt and easy access which Scheveningen enjoys with the principal cities of Holland, contributes largely to its advantages as an agreeable place of sojourn. The Hague has its fine collections of objects in art and science; its elegant and richly stored bazaar; its theatre; its clubs; and its lively concerts and promenades in the *Haagsche Bosch*. The fastidious critic, who might turn from Paul Potter's famous Bull in the *Mauritshuis*, with some regret that an artist of such infinite spirit did not possess equal refinement, will dwell upon the "Anatomical School" of Rembrandt with all the interest of an amateur, and possibly with the sympathy of a professional brother of the Nicolas Tulp whom it so admirably portrays; and will recognise, in this great work of the master, a skill which could combine at will the highest triumphs of the Low Country schools, ennobling at once with the dignified and thoughtful expression of Vandyke, and animating with the bold yet careful outline of Vanderhelst, while still throwing over all the breadth and

repose which he knew so well how to reconcile with the most marked effects of light and shade in his own inimitable style. Leyden, too, has its natural history museum, the magnificent result of the science and labours of Temminck, which may be reached in an hour's journey; and, in little more than twice the time, we are transported among the treasures of Amsterdam. To those who seek retirement, the shady paths through the wood adjoining Scheveningen are ever open, or the firm beach presents its level footing; and here the prospect is ever diversified by the long line of fishing sloops drawn up along the shore, or gliding out seawards under sail, or returning with their freight, when a new variety presents itself in the busy animation of the fish sales. Attractions so varied and so numerous render Scheveningen a place of very considerable resort, the number of sea-bathing visiters usually amounting to several thousands annually. Of these the greater portion are from Germany; and it is rare that a few princes, or scions of royalty, are not included. Independent of the government institution, there is a second and minor establishment, in which part of the guests are accommodated.

The report of Dr Mess, which is that of a careful and judicious practitioner, need not detain us by many comments. Its author is already favourably known to the profession, through a recent treatise on fractures and dislocations,[1] which we have frequently had occasion to consult with advantage, and of which we have understood that a second edition has already been demanded. In his discharge of the duties of the office with which he has been entrusted by the Dutch government, he very properly places in the foreground the study of the effects of the sea baths, and of a coast residence with the modification of habits which it implies, considered simply in themselves; but he gives ample proof of his ability to sustain these, where necessary, by the use of other remedies, as well as of his capacity to give a just direction to his efforts of the latter description, through the application of the most approved methods of diagnosis. In furtherance of his aims, careful meteorological observations are made daily under his direct superintendence; and the temperature of the air and of the sea is regularly notified on a placard fixed on the beach for the information of the bathers. The rules which Dr Mess judges fit to enforce do not appear to differ materially from those which are generally adopted by British practitioners. He has good cause, however, to complain of the ignorance on such topics of certain of his continental brethren, whom he has known to send patients to the coast with written directions, enjoining that the first bath in the sea should not be prolonged beyond half an hour, and that no after bath should exceed an hour; or that the immersions should be repeated twice daily, retiring to sleep immediately after each; or that the terror, so often evinced by children, should not be temporized with, but quelled at once by for-

[1] Handboek over de Beenbreuken en Ontwrichtingen. Leiden, 1853.

cibly plunging beneath a wave. Against the latter ineffectual cruelty the author not only remonstrates justly, but he gives his directions of the opposite description with that knowledge of the juvenile character which is so often little regarded, yet which constitutes so powerful an agent in the therapeutics of the nursery. His remarks upon the prevailing faulty education of children, especially among the higher classes, and the vicious efforts of parents and preceptors to induce a hazardous precociousness, by which not a few grave manikins and wifikins ("*deftige oude mannetjes of vrouwtjes*") came under his charge, to expiate the fault of their premature aspirations towards manly lore and wisdom, are well deserving of notice as illustrating the system of society on the Continent, and not unworthy of attention among ourselves. We more than concur in his advice, that those of tender years should not remain more than a minute, or, at the utmost, two minutes in the sea; and that, instead of being instantly carried home, only partially rubbed dry, and often with shivering frame, and extremities tipped with lividity, they should be allowed to run or roll in some warm and sheltered hollow of the downs, until reaction from the bath had restored heat to the surface. In all cases where we have had reason to doubt the prompt occurrence of reaction, we have ourselves usually restricted the bath to a single immersion, and therefore to a period of only a few seconds. In adults we have frequently farther secured our object, by causing the patient to drink a cup of warm coffee shortly before entering the sea; or, where the current of the tides, as at Scheveningen, admitted a choice of periods, to select that at which the sun's rays had already exerted some considerable influence. The intervals at which the baths should be repeated will be best determined by their results. Towards either extreme of youth or of age, where the animal heat is scanty and the reaction weak, it will be best to abstain from them altogether, and substitute the tepid, or, at most, the cold in-doors bath in their place.

We could have wished to have dwelt longer on this interesting topic, and to have considered more closely the results of our own experience in relation to that of the author. Like every other heroic remedy, sea-bathing has its perils as well as its triumphs. Occasionally of the utmost efficacy in chronic rheumatism, its inconsiderate use may be attended with the greatest danger; and we have known the sufferer from rheumatic metastasis upon the heart its instantaneous victim. If a risk is to be incurred, we would urge here the necessity of a close discrimination between a rheumatic affection of the intercostal muscles, and of the deeper seated and more important organs; a precaution which we have known to be neglected. In all apoplectic tendencies the use of the sea-bath should be wholly interdicted: but neither is this rule always remembered, though it may be nowhere controverted. In anæmia, chlorosis, strumous affections, protracted periods of convalescence, certain forms of dyspepsia, etc., there is no practitioner who has not

had reason to congratulate himself and his patient, on the excellent effects of a residence on the coast in restoring the bloom and vigour of the system. This residence, however, is frequently not sufficiently lengthened. There are many cases in which a stay, either interrupted or continuous, of from six to twelve weeks may be requisite, before we can hope to attain its full advantages. Yet, in this country, somewhere about half these periods is usually judged sufficient.

We recollect to have seen, in a Norwegian medical journal,[1] a paper on the water-cure by Dr Sandberg, founded partly on his own observations during a fortnight's visit to the establishment of Marienberg, near Boppard, in Rhenish Prussia, but chiefly on the results of the practice there, as presented in the consecutive annual reports for 1846, 1847, and 1848, by Dr Hallmann, its superintendent. These results surprised us at the time, both from their own quality, and from the candour which could permit such revelations to emanate from among the haunts of charlatanry. Thus of 16 cases of rheumatism, 7 are reported as cured; of 12 cases of dyspepsia, 1 is cured only; of female complaints, including hysteria, chlorosis, irregular catamenia, etc., there were 22 cases, and 1 cure; of hypochondriasis, without perceptible organic basis, the cases were 15, and the cure 1; of chest complaints there were 12 cases, and no cures; and of spinal irritation, 11 cases, and 1 cure. On the entire amount, the cures are stated at 13·14 per cent. only; and nearly one half of the patients, marquises, counts, and barons included, with similar dignitaries, *sequioris sexus*, left the institution confessedly wholly without benefit.

And through what an ordeal they had to pass,—of sour milk, and bilberries, and sweating, and whole baths, and half baths, and frictions, and douches, and wave baths, and seat baths, and Neptune's belts, and cold affusions, and other horrors! A few such shocks as this, and Priessnitzism may well beg to be saved from its friends; without, however, acquiring any right to adopt the rest of the apophthegm, that it will take care of its enemies. Assuredly, our good old water-cure on the sea-shore could boast of better results than these, and conceded them on gentler solicitations.

The Baths of France, Central Germany, and Switzerland. By Edwin Lee, Corresponding and Honorary Member of the Medical Academies of Vienna, Madrid, Turin, and Naples, etc. Third Edition. London, 1854. Small 8vo, pp. 207.

Notes on Spain; with a Special Account of Malaga and its Climate. By Edwin Lee, etc. London, 1854. Small 8vo, pp. 144.

Nice, and its Climate, with Notices of the Coast from Marseilles to

[1] Norsk Magazin for Lægevidenskaben, B. v., p. 181. Christiania, 1851.

Geneva. By EDWIN LEE, etc. London, 1854. Small 8vo, pp. 167.

A Hand-Book to the Peak of Derbyshire, and to the Use of the Buxton Mineral Waters; or Buxton in 1854. By WILLIAM HENRY ROBERTSON, M.D., etc. London, 1854. Small 8vo, pp. 227.

Moffat, its Walks and Wells. By WILLIAM KEDDIE; and *Report on and Chemical Analysis of its Mineral Wells, etc.* By JOHN MACADAM, F.R.S.S.A., etc. Glasgow, 1854. 12mo, pp. 163.

WE have grouped these works together because they interest, for various reasons, the peripatetic wanderer after health. The present is the season for baths and tours, and there are few medical practitioners who have not occasionally felt embarrassment in answering the inquiries of a patient, as to what water he should drink, what baths he should take, what route he should pursue. Mr Lee's works are very agreeably written, and give all the information that can be desired on the subjects of which they treat. He is evidently more predisposed to reside in a foreign than in an English watering-place, especially in winter, on account of the facilities afforded by the greater amount of fine sunshiny weather, for taking out-of-door exercise. He also considers the objects of interest which several of them possess to be advantageous, by which the patient's spirits are cheered, instead of being depressed by the aspect of cloudy skies, and the in-door monotonous sort of life which an invalid usually leads in England. Many of them, he adds, present no resource in themselves, and are rendered still more dull by the absence of society, and the unsocial, exclusive spirit in which each family is almost restricted to its own circle. Need it then be wondered at, that with the present facility of travelling, so many persons should avail themselves of the opportunity of escaping from the dreary English winters, and of enjoying the variety presented by change of scene, agreeable society and sunshine, which they meet with at little more than half the expense at several of the places of Continental resort? For pretty much the same reasons, he seems to prefer the German to the French baths.

"To the majority of British visitors, who seek more for recreation, than for a renovation of disordered health from the waters, the French baths will be found, in general, less attractive than those of Germany, especially near the Rhine; the accommodation in most of them is inferior; there are comparatively few tables d'hôte, and those not very animated, dinners being frequently sent to families from a *traiteur*. There is also little general society, as the French when *aux eaux* rarely associate with those whom they have not previously known, or to whom they are not specially introduced. A musical band does not play at drinking hours, or at any other stated period; the public salons are but little frequented as a lounge, there being nothing to attract, except an occasional concert, and gaming not being allowed. With the exception of those in the Pyrenees, the baths in France lie at a distance from each other; and the surrounding scenery is less calculated to excite interest than that of most of the German baths, of which several of the most frequented (as

the Rhenish) are situate near to each other, and an interchange of visitors is constantly taking place between them."—Pp. 3, 4.

Agreeing as we do with Mr Lee, that much of the salubrity of these places of resort depends on external circumstances, such as pleasing society, fine weather, amusements and relaxation from business, we must still remember that every one is not calculated to derive pleasure and advantage from Continental travel. The man *au fait* at the language and customs proceeds without difficulty, but they who know nothing of either, experience continual perplexity, irritation, fatigue, and disgust—feelings which are not conducive to health or happiness. We have long formed the conclusion, that with regard to foreign travelling, and watering-places, the character of the individual must be studied, and especially his circumstances as to means, his education, taste, knowledge of foreign languages, energy of mind, temper, fastidiousness, etc. etc.

From the following passage on the influence of climate in consumption, we are glad to find that Mr Lee has arrived at the same general result as ourselves, namely,—

"That the effects of climate in counteracting the operation of the causes most instrumental in the production of tubercular disease, should be estimated not merely with reference to the action of this agent upon the organs of respiration, but likewise as respects its general influence on the system—a principal advantage which invalids derive from a winter's residence in a favourable climate being, that they are enabled to take daily and sufficient exercise in the open air; which, by causing free expansion of the lungs, by improving the functions of digestion, and exciting those of the skin to greater activity than would be the case in persons who remain in-doors, as also by inducing a more cheerful tone of mind, tends materially to rectify the abnormal condition of the blood, which gives rise to the deposit of tubercle, and by this means, better than by any other, to obviate the consequences of such an abnormal condition, when they have not been allowed to proceed too far; and that this indication will be best fulfilled by selecting those places for the temporary residence of invalids which present the greatest facilities and inducements for taking out-of-door exercise."—Pp. 166, 167.

Buxton, owing to the liberal patronage of the Duke of Devonshire, the skilful superintendence of Sir Joseph Paxton, and the medical supervision of Dr Robertson, has become one of the most perfect watering-places in the country. We are told that

"The state of Buxton now, with its noble and extensive ranges of baths, supplied with all the accessories which art and ingenuity and science can furnish,—with its many and various pleasure-grounds, and promenades, and plantation walks, and ornamental shrubberies, some of them being immediately contiguous to the principal buildings, all within easy access, and all thrown open freely and gratuitously to the public,—with its park of more than a hundred acres, laid out and planted for ornamental and building ground, from plans by Sir Joseph Paxton,—with its surrounding hills, clothed with plantations of thriving trees, wherever plantations are desirable, either for the purpose of shelter, or of beauty,—with its dry soil, and tempered mountain air, and mountain climate,—this, the Buxton of the year 1854, ought to be thus compared with the place as it was, thirty or forty years ago; with the place as it was in the year 1838, when I first published an account of Buxton and its waters, and even with Buxton as it was only three years ago." But he adds, "Much as has been done from time to time for the improvement of Buxton, and the

advantage of its visitors, the increasing reputation of the mineral waters, and the increasing resort of the invalided, have always preceded and justified the extensions and improvements. It is undeniable, that the character and success of this important watering-place have been singularly independent of any circumstances but the marvellous efficacy of its healing waters."—Pp. 23, 24

Moffat also is a watering-place rapidly rising into importance; regarding which, every information may be obtained from the able little work of Messrs Keddie and Macadam.

Part Third.

PERISCOPE.

PRACTICE OF PHYSIC.

DR HALLER'S OBSERVATIONS ON TYPHUS, COLLECTED IN THE MILITARY HOSPITALS OF VIENNA, FROM 1849 TO 1853.

In 1849 the epidemic of typhus in the military hospital at Gumpendorf commenced spontaneously in two of the surgical wards, in which there were a great many patients with mortifying wounds, and with gangrenous buboes. In the space of eight days all the patients in these wards were attacked by the fever. Haller thinks that this circumstance shows, 1st, that typhus, under certain conditions, may become developed spontaneously, quite irrespective of contagion; 2d, that the effluviæ of putrescent animal substances render the atmosphere in which they are diffused prone to the development of the typhus poison; 3d, that it tends to confirm the zymotic hypotheses of Liebig.

In 1849, intermittent fever was very prevalent before the outbreak of typhus in the military hospitals, and it was observed that in proportion as the intermittent diminished the typhus increased; that when the typhus was raging at its maximum the former disease had quite disappeared; and that as the typhus decreased in severity the cases of intermittent began again to return. In regard to these circumstances Haller remarks, 1st, that the causes of malaria and typhus poison are totally distinct; the former arising from the putrefaction of vegetable substances, and of the lower animals, the latter from the putrid emanations of the higher animals; 2d, that there is no instance known of a place where intermittent fever was endemic spontaneously developing endemic typhus or typhoid miasmata; 3d, that Denis has shown by experiments that the malaria is generally connected with an atmosphere commixed with sulphuretted hydrogen, and that, on the other hand (according to Liebig, Simon, and Scherer), the typhus miasma is due to an ammoniacal condition of the air. Hence he concludes that the typhus which so often succeeds intermittent fever in the army, does not depend upon the same, but upon a distinct set of causes.

Haller regards exanthematous and abdominal typhus as merely modifications of the same morbid affection, and not as essentially distinct and separate in their nature.

That the contagious principle of typhus is lighter than atmospheric air, and always ascends, this author ascertained satisfactorily, not only by ozonometrical experiments, but also by direct and careful observations. Ozone, when admitted into a fever-ward, was ascertained to become first lost in the upper regions of the atmosphere. Moreover, when the under stories of an hospital were filled with typhus patients, those in the upper stories were always observed to become infected, when there was a communication between the air of the two stories. On the contrary, when only the upper stories contained cases of

typhus, the patients in the under part of the house enjoyed perfect immunity from the disease, although there was atmospheric access between them both.

He observes that *dark*-coloured materials of clothing, etc., are more prone to absorb the contagion of typhus, and to convey it to other individuals, than those which are *light*-coloured. He found that among troops wearing dark-coloured uniforms it more frequently happened that new cases of typhus entered the hospital after a convalescent patient had rejoined his *corps*, than among those wearing light or white-coloured uniforms. A similar observation was made by Stork, who found that in dissecting rooms dark-coloured clothes acquired the cadavoric odour sooner, and were deprived of it less easily than light-coloured garments. He ascertained by experiments that the absorption of odours by coloured bodies is regulated by the laws which govern the absorption of light, and that the power of attracting odours is regulated in all bodies by the degree and intensity of their dark colour.

Diagnosis of typhus.—The principal diseases with which typhus may be confounded are :—

I. *Meningitis.*—This is apt to occur when the patient is brought to the hospital in a state of insensibility, with dry tongue, meteorism, diarrhœa, and often chronic hypertrophy of the spleen. Dr Haller places great diagnostic value on the appearances presented by the urine in the two diseases, and the following are the differences which he affirms are observable in that secretion :—

In Meningitis.	*In Typhus.*
High spec. gr. (1028-35).	Low sp. gr. (1017).
Abundance of urea and uric acid,	Little urea; no increase of uric acid.
Uroerythrine,	No uroerythrine.
Phosphates not decreased,	Phosphates decreased, or wholly absent.
Trifling increase of the alcoholic extract.	Very considerable increase of the alcoholic extract.

II. *Pneumonia.*—In severe pneumonia where the whole lungs are hepatized, or when it is combined with meningitis or cerebral hyperaemia, it may present all the external characters of typhus. Physical examination of the lungs is the only means of diagnosis we possess.

III. *Miliary Tuberculosis.*—This disease often, from beginning to end, so closely resembles typhus, that they are confounded together. Although Haller specifies no pathognomonic symptoms as characteristic of miliary tuberculosis, he indicates one mark, whereby, in cases of doubt, a distinction may be made between them. This consists in the presence or absence of *albuminuria*. In all the cases of miliary tuberculosis which the author has seen (9 in number), the urine has contained no albumen ; but in typhus albuminuria is by no means infrequent.

IV. *Acute Intestinal Tuberculosis.*—This may also simulate typhus, especially when conjoined to serous effusion within the cranium. But this affection generally occurs synchronously with pulmonary tuberculosis, with which typhus is seldom found connected. In those rare cases, however, where intestinal tuberculosis occurs, without any indication of pulmonary disease, the following marks serve to distinguish it from typhus :—The skin is pale and shrivelled, not turgescent and hyperaemic as in typhus ; the belly is not swollen, but deeply sunk and depressed ; the swollen and infiltrated mesenteric glands may often be distinctly felt by the hand ; the stools are foul, purulent, and painful, and contain various constituents, absent in the others, as the phosphates of ammonia and magnesia, and whitish clouds, etc.

V. *Pyœmia* can only be distinguished from typhus by an inquiry into its exciting causes—as wounds, operations, etc., where these are not known, the diagnosis is impossible. In the autopsy of a fatal case of this kind, which, during life, presented all the characters of typhus, with an icteric skin, and which was diagnosed as continued fever, none of the traces of typhus were found, save an acute atrophy of the liver.

VI. *Acute Bright's disease*, with uraemic symptoms, might be mistaken for a case of typhus; but the rapidly succeeding enlargement of the extremities, as well as the albuminuria, with fibrinous tube-casts, easily afford us means for accurate diagnosis.

TREATMENT.

1. *Large doses of Calomel.*—The author tried ℈j doses of calomel in twenty-three cases. The advocates of this system place great dependence upon it when it is commenced during the first few days of the fever; but at this time the diagnosis of the typhus is uncertain, and therefore Haller only administered it in unmistakeable cases of typhus, where the characteristic maculæ were visible on the skin. His experience is certainly not in its favour; for of the twenty-three cases thus treated, only eight recovered, and fifteen died—a mortality equal to 35 per cent.; while the average mortality during the course of the whole epidemic was only 17 per cent. On examining, *post mortem*, the bodies of patients thus treated, numerous large ulcers were found in the patches of the peyerian glands.

Haller found that in patients treated with large doses of hydrargyrum, one of the usual symptoms of typhus, viz., tympanitis, never supervened, after one or two scruples of the calomel had been taken. This result he attributes to a purely chemical process—the sulphuretted hydrogen gas which occasions the meteorism, being decomposed by the calomel, and a sulphuret of mercury being formed. He alleges that the preparations of iron, especially the carbonate, act in this manner also, while they possess none of the disadvantages of the other mineral. By the use of chalybeates he found the tympanitic state of the abdomen completely disappear, and the evacuation of copious green stools take place; a sulphuret of iron having been formed by chemical action, thereby decomposing the sulphuretted hydrogen.

2. *Venesection.*—Haller found that most of the cases of so called fever which were successfully treated by bleeding, were not true typhus, but merely cases of hyperaemia of the brain, meningitis, or pneumonia with typhoid symptoms. In true typhus he found the mortality increased by it. Even in those who recovered he observed that the nervous symptoms occurred sooner, with more intensity, and were of longer duration; that the maculæ spread over the whole body, and were of a darker colour than usual; that convalescence was very protracted, and that the patients continued for a long time very anaemic. On examination after death of the bodies of those treated by venesection, every organ was found to be, more or less, in an anaemic condition.

3. *Sulphate of Quinine.*—Haller administered this remedy in numerous cases, in doses varying from the medium to the maximum in magnitude; but he never observed any beneficial effects produced by its use, either upon the course and duration of the disease, or upon any of its symptoms.

Treatment recommended.—As the best indications for the treatment of typhus, this author recommends, 1st, The internal administration of dilute mineral acids —especially the muriatic and sulphuric; 2d, Ventilation, and regular admission of fresh air into the apartments of the sick; 3d, Careful and frequent sponging of the whole surface of the body with vinegar and water; and 4th, A copious supply of fresh water to drink. All other specifics or remedies for typhus he condemns as superfluous, if not as positively injurious.—*Wien. Med. Wochenschrift*, 42–45, 1853.

VIRCHOW ON BLUE PIGMENT IN THE URINE, AND CHROMATURIA IN GENERAL.

As an appendix to his former papers on the urine, Virchow has published a short article describing more specifically the characters of blue urinary pigment, of which he gives the following as the synonymes:—Uroglaucin, (*Heller*); cyanourine, (*Braconnot*); urocyanine, (*Martin*); purpurine, (*Golding Bird*).[1]

[1] Virchow is here in error; Bird's purpurine is a *pink*, not a blue deposit.—*Translator.*

Virchow has only seen blue pigment existing in the urine, when voided, in one case; it is generally obtained by allowing the urine to stand exposed to the air, or by boiling it with the mineral acids, especially with the muriatic. It is gradually precipitated as a violet, brownish, or blackish sediment. It is not universally found to be crystallized; indeed this condition of it appears to be always more or less transitory. Too long boiling, or application of the acid without heat, frustrate crystallization. He found the appearance of the crystals to be exactly analogous to that originally described by Heller, and subsequently figured in Plate VI., fig. 5, of Otto Funke's Atlas of Physiological Chemistry, viz., small, fine needle-shaped crystals, of various lengths and irregular forms, seldom occurring isolated, but generally in groups, composed of two, three, or more clustered together. Their smallness has hitherto prevented their correct crystallographic determination. They are easy soluble in ether and alcohol. From their solutions in these re-agents, Virchow, like Funke, failed in obtaining colourless crystals, and got merely an amorphous bluish deposit. Mayer, however, obtained tolerably large blue crystals by *slowly* evaporating the solutions.

Virchow is unable to state upon what particular condition of the urine the formation of this pigment depends; but, like other colouring matters, it is usually found in urine rich in uric acid. It cannot be separated from the uric acid either by mineral acids or by ammonia, but solely by means of repeated solution in alcohol and ether. The distinguishing characteristics of urine containing uroglaucin are, 1st, the readiness with which it undergoes *acid* fermentation; and 2d, its giving no reaction when Trommer's copper test for sugar is applied. The pigment seems to enter into a combination with the copper, which partly obstructs, and partly resembles the sugar-test. When sugar is added to such urine, and Trommer's test is applied, there occurs no precipitate, but merely an intense yellowish-brown colour. Rayer, Bird, Martin, and Shearman, have shown that urinary pigments are found in greatest abundance in diseases of the lungs, liver, and spleen; and Virchow conjectures that there exists a certain inverse ratio between the separation of colouring matters by the urine and the bile. Where the biliary secretion is diminished, the urine contains a larger proportion of colouring matter, yielding uroglaucin, so that the latter seems to be a sort of surrogate for the biliary pigment. The colouring matter of the bile does not exist normally as such in the blood, but it appears first in the liver; and Virchow thinks it is probably formed from some other blood-pigment—perhaps from the "Haemaphäin" of Simon?

Under the term "chromaturia," Virchow would designate all abundant excretions of urinary pigment. It is generally accompanied by not only an increased amount of uric acid, but an alteration in the constitution of the blood and the tissues. Both chromaturia and the uric acid diathesis are often combined with albuminuria, and the minor grades of nephritis. The seat of the formation of the uroglaucin, as of the other pigments, lies in the blood rather than in the kidneys.—*Virchow's Archiv. für Pathol. Anat., Bd.* vi. *Hft.* 2.

MIDWIFERY.

DISCUSSION ON M. DEPAUL'S REPORT ON THE TREATMENT OF DISPLACEMENTS OF THE UTERUS BY INTRA-UTERINE PESSARIES, AT THE ACADÉMIE IMPÉRIALE DE MÉDICINE DE PARIS.

(*Continued from page* 171).

M. AMUSSAT said that the idea of introducing bougies (*tiges*) within the cavity of the uterus, first suggested itself to him in 1826, on seeing M. Recamier use the uterine sound for diagnostic purposes. Among the first cases in which he used the pessary was that of a woman with anteversion; he introduced a pessary with an ivory stem into the uterus, and allowed the patient to go about with the instrument in situ. Inflammation supervened, and the patient died.

Deeply grieved in consequence of this mishap, he tried to remedy all displacements by extra-uterine instruments. He employed for this purpose conical pessaries, which resembled ivory cupping-glasses, but these were found productive of so much pain, that he had to abandon their use. For thirty years he had used both intra and extra-uterine instruments. (Here M. Amussat exhibited to the members of the academy his various instruments—some made of ivory and steel, and others of india-rubber, elastic gum, etc.) He was discouraged, however, by the general results of instrumental treatment. He had heard of many spontaneous cures of uterine displacements, resulting from local inflammations and ulcerations having produced adhesions between the uterus and vagina, and this led him to imitate nature, and to attempt their cure by gently cauterizing the posterior part of the cervix, that by the adhesions formed during the cicatrization of the sore, the displacements might be radically and permanently cured. The successful results of this mode of treatment had far surpassed his anticipations. Relaxation of the vagina and procidentia of the uterus often rendered the application of the caustic somewhat difficult; but by putting the fingers into the rectum, and simultaneously using the sound, the position of the uterus could be sufficiently maintained during its application, and by a pessary thereafter. He related several successful cases illustrative of this mode of treatment in cases of retroversion. He had not found cauterization of the anterior lip of the cervix to be beneficial in anteversion. In such cases he used a cup-shaped pessary, (*en gobelet*), to support the uterus. In conclusion, he believed the indiscriminate use of pessaries for the cure of displacements to be prejudicial; he considered the employment of intra-uterine instruments as dangerous in some instances, and as productive of merely temporary benefit in the great majority of cases; but he characterised his own method of treatment as free from all danger, and as permanent in its results—except after pregnancy.

M. MALGAIGNE again spoke at great length. He agreed with M. Hervez de Chégoin, in considering that uterine displacements occasioned great pain, and also engorgement, and that these disappeared after the replacement of the organ. But he affirmed that it was impossible to recognise engorgements of the uterus apart from, and independent of, displacements. He commented, with some severity, on the statements made by Gibert, in which that gentleman had underrated the importance of uterine affections, and attributed their symptoms to general pathological conditions of the system. Referring to what had been said by M. Dubois, he alleged that neuralgia of the uterus did not appear to be a very common complaint, for although he *had* seen cases of it, they were very few in number; but he bowed deferentially to the experience of Dubois. He maintained that the pain occasioned by pure displacements of the uterus, was removable by mechanical means; and that the recumbent posture caused its cessation. This circumstance he regarded as pathognomonic of the affection. He looked upon the speculum as of little use in the diagnosis of displacements, as he had known such cases when examined by means of it, diagnosed as ulcerations of the cervix, and treated needlessly with cauterants. M. Huguier had alleged that displacements, when trifling in amount, occasioned no pain, but herein he was in error. The suffering occasioned by displacements was by no means regulated by their extent, but solely by the special susceptibility of the patients in whom they occurred. Great prolapsus might occasion no pain, while trifling deviations of position might be quite intolerable. M. Cazeaux had said, "Congenital inflexions are seldom the cause of pain, while those occurring accidentally are always the source of suffering, which is sometimes very acute." He asked M. Cazeaux for some proof of the existence of congenital inflexions, and by what means he distinguished them from the others. He could conceive of no way except saying to the patient, "Madam, have you had this inflexion since your birth?" After passing a high compliment on M. Velpeau, he made some remarks on certain portions of his address. Velpeau had stated his belief that the displacements occasioned pain in the round and

broad ligaments, which he said were filled with nervous filaments derived from the great sympathetic, and continuous with the mesenteric nerves. This doctrine Malgaigne condemned as unphysiological. He did not agree with Velpeau in thinking that "no experienced practitioners could overlook these conditions." The present discussion was the best proof of the possibility of such mistakes. As to the hypogastric belt, which had been originated and greatly used by Velpeau, he affirmed that it could always be borne by patients who had simply uncomplicated displacements; and that where it was not borne well at first, it seldom was so even after usage for some time. *Can uterine displacements be replaced?* Undoubtedly, for he had seen three cases of retro-flexion cured spontaneously. There was no difficulty in the replacement of so mobile an organ; and in ordinary cases, where there were no adhesions, the instrument when introduced into it, replaced it instantaneously, and without any effort. He could not see the advantage to be gained by allowing the instrument to remain, (as proposed by Simpson, Valleix, and others), within the uterus, for days, weeks, or months. What was the purpose of this? To give tonicity to the ligaments? They were not placed in their normal direction. If it was in order to allow the intestines (which were normally interposed between the uterus and the rectum, and keep it *in situ*) time to slip behind the organ again, it was unnecessary, as this was the work of a moment. He was of opinion that the instrument for replacing the uterus should not be allowed to remain within it more than a few minutes at the utmost. *When the uterus is replaced can we maintain it permanently so?* That was much less probable, for the organ having once been displaced, was very prone to become so again. He believed that some complete recoveries had been effected, but that in *no* case was a cure certain. Seeing, then, that a cure was so very uncertain, and palliation alone could be hoped for, he considered it was neither prudent nor reasonable that we should subject our patients to the risk of danger and death. He concluded thus:—"I have only one word more to add, and it is this:—If unfortunately I had to treat my wife or my daughter for an uterine displacement, I should not choose the most dangerous of two palliative measures, and most assuredly, I should not subject them to the use of the intra-uterine instrument."

M. Ricord desired to make a few remarks on the treatment of certain hyperaesthetic conditions of the cervix and vagina, which were sometimes consequences or complications of various displacements of the uterus, with or without some other morbid condition. There were many cases of displacement where the pain was alleviated or removed by lying in a certain position. He considered that much of M. Valleix's success was due to his displacing and isolating the uterus from its painful position, without making pressure (*sans prendre un point d'appui*) on the sensitive hyperaesthetic parts. For a serious case of this nature, in which all the resources of therapeutics had failed in the ablest hands, and in which *considerable benefit* had been derived from the use of M. Valleix's instrument—he had invented a new instrument, whereby the uterus was isolated, supported, and maintained in a suitable position, without the instrument entering the cavity of the womb, or touching the vagina. Its use was quite painless and free from bad consequences in cases where the cervix could be touched by an instrument or by the finger of the accoucheur. This instrument consisted of a forceps, which was introduced by the aid of a speculum, and the points of which seized the cervix. By means of a ring, moved by a screw, the uterus could be raised to any desired height; and the whole apparatus was fixed by means of the plate (*plastron*) of M. Valleix's instrument. In conclusion, he considered, with MM. Paul Dubois and Velpeau, as well as with M. Huguier and with M. Malgaigne in his first speech, that the mode of treatment practised by Simpson and Valleix was productive of great benefit, when applied to cases requiring it, and that consequently it should not be rejected from practice.

M. Robert said he rose with some hesitation to address the Academy, but having been a member of the commission for whom M. Depaul acted as reporter, he considered it right to state wherein he differed in opinion from M. Depaul.

He did not, with M. Depaul, consider that the influence exercised by uterine displacement on the health of females, had been at all exaggerated. In his opinion, there existed two classes of deviations: the one comprising such as were primitive, and resulted from accidental and occasional causes; the other, such as were secondary, occasioned by different lesions of the uterus, more especially of the body of the organ. The former class of cases were observed to occur thus:—"A woman in good health makes a sudden or violent effort, or falls on the breech; she feels at the time a crack and a painful sensation in the lower part of the abdomen. From this time she suffers more or less pain; menstruation becomes deranged, and is either too scanty or too profuse. She consults us, and we find retroversion; the uterus is painful, tumified, etc." He observed that such cases should not be neglected, as they did not always remain uncomplicated, and functional disorders, as well as organic lesions, were liable to result from them. The latter class of cases followed chronic metritis, uterine catarrh, and inflammation of the cellular tissue surrounding the uterus. They occurred after protracted labours, and especially after miscarriages; and were observed in females of lymphatic temperament, in whom uterine congestion occurred without signs of phlegmasia. The fundus of the uterus was generally at fault in these cases; and was often so voluminous and heavy that it swayed backwards and forwards. In retroversion, suffering resulted from the interrupted circulation in the uterus, from the obstruction offered to the passage of fæces, and from the pressure of the organ on the sacral nerves. He expressed astonishment at what had been said by Depaul and Velpeau as to the ease with which displacements could be diagnosed. He had studied diseases of the uterus carefully during the twenty years which he had been surgeon to the Hôpital de Lourcine, and he still often found great difficulty in diagnosing displacements. He considered the pathognomonic sign of displacements adduced by Velpeau and Malgaigne—viz., freedom from pain in the recumbent posture and return of uneasiness on rising—applicable only to cases of anteversion complicated with procidentia (*abaissement*), and not to retroversion.

He had for a long time been in the habit of introducing dilating bougies into the cavity of the uterus, and never saw any bad effects; and he had also tried to replace the organ by means of an instrument like that shown by M. Velpeau, but without success. He had also employed Simpson's pessary. He first used it in a case of retroversion which had resisted all other treatment. No evil result occurred during its use, but, on the contrary, the catamenia, which had been absent more than three months, returned in normal quantity on the fourth day after its application. It was worn for a period of six weeks, and during this time, a second menstrual epoch occurred, without any pain or inconvenience. At the end of the six weeks it was withdrawn, and he then found that the uterus had regained its normal position. The retroversion returned, however, and the pessary was again introduced. The patient left the hospital, and went about in Paris for ten or twelve days without experiencing any pain; but feeling it uneasy after that time she applied for its removal. Since then he had never seen her. He used Simpson's instrument in a second case of retroversion, but as it induced a continual discharge of blood, he was obliged to withdraw it on the tenth day. In a third case of retroversion, occurring in a woman of sanguine and irritable constitution, he used this pessary. He had to withdraw it, however, at the end of three hours, inasmuch as it caused severe uterine pains, accompanied by vomiting. He tried it again three times, at intervals of several days, but without success. He had therefore abandoned the use of the instrument. Although injurious results had followed the use of this instrument, he admitted that it was proved by incontestible facts that in very many cases, where all other measures had failed, this mode of treatment had been productive of complete and lasting success. He thought that it would be premature to exclude this instrument from practice, but he considered it very desirable that its mode of action was better understood, and above all, that we knew accurately all its indications and counter-indications.

This knowledge would render its employment more efficacious, and would diminish the number of accidents resulting from its use.

M. Depaul then replied at great length, and maintained that his report had been impartially prepared. M. Huguier had said that intra-uterine pessaries should be used in "those exceptional cases of displacement, giving rise to serious symptoms, which resist other means of treatment." If displacements occasioning severe symptoms were rare, how much more so must be those cases which resisted treatment? He could fearlessly affirm that such cases were hardly to be met with during the course of a long career. As regarded the use of pessaries for emmenagogue purposes, he could say that cases of amenorrhea affecting the life and health of women were very rare, and that those which resisted ordinary medical treatment were still more exceptional. He believed that the sanguineous discharge provoked by the introduction of these instruments was *not* the catamenial fluid, but a true uterine hæmorrhage, which had been mistaken for menstruation. As to the influence of displacements upon the health, he believed that statistics, collected from a number of rigorously observed cases, were necessary to set the question at rest. He still remained convinced that there was great exaggeration in the opinions of the partizans of mechanical replacement; and he thought that he had proved this from their own observations. He had never denied that displacements or flexions might occasion sterility, and he believed this to be due to an alteration both of structure and of form. Were uterine displacements incurable? No one had affirmed this absolutely. There were a great many displacements, especially congenital inflexions, which resisted all treatment, but there were others which were easily removed. The first question he always asked himself when called to treat such cases was, "Is the tissue of the organ sound?" If not, he tried to remedy the disease before the displacement. He did not mean absolutely to assert that uterine displacements were of no consequence, or that they caused no accidents. He had seen enough of these diseases to know that it was not so. He agreed with M. Hervez de Chégoin that many displacements resulted from diseases of the uterus, but he did not consider, with him, that cases of uncomplicated displacement, producing distressing symptoms, were at all common; he believed them to be very rare, but he did not deny their existence. It was far more usual to find displacements—flexions and versions—existing to a considerable extent, without in the least affecting the health, or leading the women in whom they occurred to suspect their existence. As regarded the introduction of intra-uterine pessaries, he might mention that M. Michon had not only abandoned their use, but had expressed scruples about introducing foreign bodies even into the cervix, inasmuch as he had seen peri-uterine inflammation and the formation of abscesses follow the introduction of a hollow sound. He agreed with the principles of treatment laid down by M. de Chégoin, and considered that mechanical treatment should never be resorted to until all other pathological conditions, existing simultaneously, had been removed; that we should not confine our attention *solely* to the local affection, but by all suitable means endeavour to remedy the general health, and those derangements of innervation, nutrition, and circulation which were so common in such cases. He believed further that, in certain cases, it might be advantageous to associate with the local treatment of some uterine affections, the use of mechanical means, which by fixing (*immobilisant*) the organ without irritating it, might aid in removing chronic inflammations, of a partial or general character. He protested against the injustice of M. Cazeaux's accusation of having quoted only the unfortunate, and suppressed all the successful cases; he had drawn his data from all the cases he was cognizant of; and he knew of no really successful ones. He appealed to the statistics of M. Gosselin which he had quoted, for the truth of his assertion, that many uterine affections—even the most serious—might exist without giving rise to any uneasy symptoms. Displacement was a simple unimportant deformity. M. Cazeaux had alluded to catarrhs, granulations, ulcerations, neuralgias, etc., co-existing

with displacements. In such cases these lesions should be attacked before the displacements; and in sixty cases he had treated on this principle there were only three in which any bad symptoms remained after two or three months. He advanced a series of arguments and statistics, at great length, to prove the fallacy of the assertion—made originally by MM. Boulard and Verneuil, and quoted in the speech of M. Cazeaux—that anteversion was the normal condition of the uterus in children and women who had never borne children. He had shown by his researches that intestinal expansion, in the dead body, might raise up the organ, and so deceive observers. But he admitted that there was a *predisposition to anteversion* at all periods of life. He regretted that M. Cazeaux had not stated, for the enlightenment of MM. Huguier, Valleix, Nonat, and others, the "proper precautions" which rendered uterine catheterism safe. He did not deny the usefulness of the uterine sound (*hystéromètre*); he considered that it was wrong to make the use of it general, and thought its employment should be confined to the *few* cases in which it was indispensable. With considerable asperity Depaul next attacked the assertion which had been made by Cazeaux, that there was no such thing as inflammation of the tissue-proper of the uterus; and he quoted cases and authorities to disprove it. He alluded to the statement made by M. Dubois, that the uterus was never permanently replaced by the use of instruments, and asked if, in the event of this statement being true, it did not militate against the use of intra-uterine pessaries. After the cases cited by M. Dubois and himself, he thought there could be no doubt of the fact that these instruments were useless. ("*Les redresseurs ne redressent pas.*") He believed that a modified form of the pessary might perhaps, as Dubois said, relieve uterine neuralgia; but he did not hesitate to express his opinion, that the evil results and fatal cases caused by the intra-uterine pessary ought to exclude it from use in the treatment of all serious uterine phlegmasiæ. He next alluded to M. Malgaigne's speech; and considered as very inconclusive and unsatisfactory the test he gave for the diagnosis of displacements, viz., cessation of pain on assuming the horizontal posture. He also objected to Malgaigne's detestation of the speculum, and of vaginal examination in such cases. He had accepted an invitation from M. Malgaigne, on the 2d July, to go to the Hôpital St Louis, to see two cases there which had been cured by M.'s hypogastric belt. They were said by Malgaigne to be cases of simple displacement—the one a case of anteversion, the other of retroversion. On examining the patients manually and by means of the speculum, he found considerable ulceration of the cervix to be present in both cases; in the first case there existed, in addition, slight anteversion and considerable prolapsus, in the other there was no displacement whatever. He agreed with all that had been said by his learned master, M. Velpeau, in praise of the high scientific character and general probity of M. Valleix; but he dissented from the opinions expressed by Velpeau as to his practice. If Valleix had had cases proving the successful use of intra-uterine instruments, he was astonished that he had not published them. Velpeau had attributed most of the evil results of pessaries to errors of practice and diagnosis, and to want of expertness and ease in manipulation. He could not see wherein lay the difficulty of introducing a bougie into the uterine cavity, and could not imagine practitioners like MM. Valleix, Nélaton, Huguier, Aran, etc., being deficient in proper precautions or expertness. What always astonished him was that the uterus should be so tolerant of instrumental interference, when it was recollected how easily it was irritated by the presence of a small foreign body, and how the presence of a simple clot or polypus within its cavity induced violent contractions of it, which only ceased when the offending substance was expelled. There was a great difference between these foreign bodies and an intra-uterine pessary formed of solid, inflexible material. It was said that the end of the stalk should not touch the fundus uteri; but it was forgotten that this organ had, properly speaking, *no* cavity; and, however short might be the instrument introduced, it acted on the organ by all the parts of its surface, and espe-

cially by its point, against which the uterus was continually being impinged, either by its own inherent force of contractility, or by the impulsions given to the organ by muscular movements and the pressure of the intestines. M. Velpeau had concluded his speech by expressing a hope that M. Valleix would decide the question as to whether these instruments should or should not be used. M. Valleix had already done so ; but his opinions were dangerous, and therefore he (M. Depaul) could not accept them. He concluded by laying before the academy the following conclusions made in the name of the commission, and carried by a majority, with the exception of the third, in which M. Depaul said he was at variance with his colleagues :—

1st. *The cases reported to the academy by MM. Broca and Cruveilhier, along with numerous others on record, prove that the use of the intra-uterine pessary may often occasion serious accidents, and sometimes even death.*

2d. *In the rare cases in which this instrument has appeared productive of advantageous results, it is not proved that it had always acted by replacing the uterus.*

3d. *In some exceptional cases where uterine displacements (déviations) have occasioned serious functional derangements, and where all known therapeutic measures have been tried in vain, the application of the intra-uterine pessary may be tried as a last resource.*

4th. *The commission has the honour to propose a vote of thanks to M. Broca, for his very interesting communication which has originated the late debates ; and also to M. Cruveilhier.*

The President put the *first* conclusion to the vote.

M. Velpeau.—This conclusion appears to me to have been adopted ; but it is too severe. Instead of saying "the use of the intra-uterine pessary may *often*," I would substitute the words, "may *sometimes* occasion," etc.

The President.—It is proposed to suppress the adverbs altogether. It should be read :—"The use of the pessary *may* occasion," etc.

The President then put the first conclusion to the vote, with the word "*often*" suppressed. It was adopted in this modified form.

The *second* conclusion was carried unanimously, without any modification.

The *third* conclusion was then read, and M. Gibert demanded its suppression.

M. Velpeau.—I insist on its being allowed to remain.

M. Cazeaux.—I am convinced that the use of the intra-uterine pessary is always useless, and sometimes dangerous. I cannot adopt this conclusion.

The President.—It contains the words, "as a last resource." For this purpose we may try anything (*laughter*).

Several members proposed the suppression of the conclusion, and the President proposed to put it to the vote.

M. Jobert requested the suppression of the words, "as a last resource" (*comme dernière ressource*).

M. Velpeau supported this amendment.

The conclusion thus modified was put to the vote, and was rejected.

[The proposed commentary of Dr Simpson will constitute an original communication, to be inserted in our next No.]

Part Fourth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

Meeting X.—*March* 25, 1854. Dr Bennett, President, in the Chair.

Dr Matthews Duncan was added to the committee on cancer already appointed.

MILK FROM THE MALE MAMMA.

Dr Cobbold demonstrated under the microscope a specimen of milk from the

mammæ of a male child, nine days after birth. The milk presented the usual microscopical characters; but the globules were larger and less numerous than usual. There were no compound granular cells.

Dr Matthews Duncan referred to Natalis Guilliot's researches, which proved the normal presence of milk in the mammæ of children shortly after birth.

ON A MALFORMED TROUT.

Dr Cobbold exhibited to the society a malformed trout which had been captured with the fly in the river Jed, near Jedburgh.

The chief peculiarity, viewed externally, consisted in the preponderant depth of its body as compared with the length, giving the animal an humpbacked appearance, and causing it to resemble very closely the common bream. Respecting the explanation of this anomaly Dr Cobbold offered the following remarks:—Part of the skin and lateral muscles of the left side of the body having been removed, the society will observe that we find the dorsal elevation to be entirely owing to the position and direction of the neural spines and interspinous bones, a considerable number of which instead of sloping backwards (as in the normal skeleton), have been tilted forwards so as to force upward some of the interspinous bones with their corresponding dorsal-fin rays. This curious phenomenon has evidently been brought about by the excessive development of some of the dorsal vertebræ at the expense of others, for at different parts of the spinal column there are two, three and four bodies of vertebræ compressed together, the irregular intervals between these undeveloped *centra* being occupied by normal vertebral segments—the unequal pressure thus giving rise to the change already described. On the same grounds it becomes very interesting to notice that we may be pretty certain, that in all the numerous and hitherto observed varieties of this genus, the same secondary causes have conduced to bring about analogous results. Keeping this in mind, we perceive that many so called specific differences are alterations of degree rather than of kind, and this holds more particularly in cases where the cephalic or first four vertebral segments have undergone extension or abrogation as has undoubtedly obtained in the case of many of our permanent varieties of *salmonidæ*.

ON FASCIOLA GIGANTICA.

Dr Cobbold exhibited a series of injected fluke-worms removed from the liver ducts of the giraffe formerly alluded to. The worms varied from an inch and a half to three inches in length, and were about one-third of an inch in breadth. The relative position of the viscera and their corresponding excretory orifices, resembled that of trematodes generally. These specimens were in every case injected from the mouth, but in some instances the injection had passed into a set of branching tubuli, which were smaller, superficial, more numerous, and readily distinguishable from the digestive cœca, which were disposed regularly, and lie deeper in the substance of the animal. Dr C. thought there could be little doubt that these superficial tubuli, by many considered vessels, constituted together an excretory organ or gland, inasmuch as in the present instance a gentle pressure on the syringe caused the injection to flow in a continuous jet from a small orifice situated at the centre of the caudal extremity. That these tubuli were in no way connected with the digestive cœca was proved by the circumstance that the filling of these superficial canals was always accidental, *i.e.* by rupture following over distension of the former, also leading to the idea that they were lacunary in character. On comparing them with the figures of the vascular system of *trematoda* given by *Blanchard*, one cannot fail to be struck with the similarity of the two sets of vessels, and Dr C. thought that we are justified in concluding them to be identical, an opinion in strict accordance with the views of *Van Beneden*, who questions the presence of a true vascular system in the trematode entozoa. At all events a distinction between the so-called vascular system, and the special excretory organ of trematoda has not yet been conclusively demonstrated. Considering

the large size of these individuals, and no description justifying the idea of their having been previously described, Dr Cobbold proposed to name this species, *Fasciola gigantica.*

Associated with the above, and scattered throughout the substance of the liver, were numerous cysts containing entozoa, some of which had undergone calcareous degeneration, especially those lying near the surface of the organ, and which were mentioned when describing the external characters of the liver as small hard prominences. Under low magnifying powers the entozoa exhibited appearances identical with those of the fully developed *cysticercus longicollis,* and all of them seemed to be of equal size and growth.

Some conversation here followed in reference to the researches of Siebold, Steenstrup, Nelson, Kuchenmeister, and others, after which Dr Cobbold concluded by noticing in addition to these cysticerci, the occurrence of three small cysts situated in the aponeurosis, investing the stylo-glossi and lingualis muscles. These presented the following microscopic appearances : — The investing capsule has two layers, the outer one consisting of areolar tissue, and the inner of flattened epithelium well marked. Within the capsule occurs a second cyst surrounded by an albuminous fluid ; this structure magnified 250 diameters, exhibits only a mass of small cellules of irregular form and size, in a few of which nuclei appear ; there are also a few indistinct fibres. The embryo or larva contained within this secondary cyst, and coiled upon itself, presents very much the appearance of an ammonite. On the external surface there is a single sucker, and the body presents a series of rings or segments ; a tube passes through the substance of the entozoan at the middle line, but this does not appear to communicate with the sucker observed without. The integument (mag. 250) presents the ordinary modifications of flattened epithelium. The substance of the embryo (mag. 250) consists of regular oval cells with one or more nuclei, but in the centre of the spiral near the apex of the coil, occur two large compound vesicles or ova, which can be readily seen when the section is examined with a pocket lens. Magnified 60 diameters these exhibit each a double cell-wall, enclosing in their interior four nucleated cellules, which again, under a still higher power, present distinct nucleoli.

CANCEROUS TUMOUR OF THE KIDNEY.

Dr W. T. Gairdner exhibited a large cancerous tumour in connection with the kidney. It had occurred in a child four years old, and had filled and greatly distended the abdomen. Some parts of the tumour were white and soft (cerebriform), other parts were rendered firmer and coloured apparently by extravasated blood. Small cancerous tumours were found connected with the lungs in the same case. Demonstrations of these tumours were made under the microscope, and presented rounded nucleated cellules, with little or no fibrous tissue ; but in some portions of the tumours, fusiform cellules were found in great numbers, similar to those which occur in the early stage of the formation of fibrous tissue.

Some discussion ensued, in which Drs Bennett, Gairdner, Matthews Duncan, and others, shared.

EDINBURGH OBSTETRICAL SOCIETY.

Session XII.

ON EARLY LIVE BIRTH, WITH CASE. BY DR KEILLER.

Dr Keiller exhibited a premature fœtus which was born *alive* in the fourth month, and made some remarks on the *medico-legal* relations and importance of such a case. The following are the particulars of the case as noted at the time of its occurrence :—

On the 17th June Dr Keiller was called to Mrs R. who was about to abort. She had miscarried about a year previously (20th July), when seven months pregnant, and now considered herself only in the fourth month, having

last menstruated on the 8th February, and quickened about a week ago (8th June). The pains were evidently expulsive, and on examination the distended membranes were felt protruding into the vagina. Dr K. shortly afterwards ruptured them, when the liquor amnii was forcibly and fully expelled, a foot immediately presented itself by which the extraction of the fœtus was speedily accomplished. The heart and vessels of the cord were beating vigorously, which induced Dr K. to allow the fœtal circulation through the still attached placenta to continue for sometime in order to observe the reflex movements of the limbs, face, and respiratory muscles which thereafter took place. At first these muscular reflex contractions were very marked. On touching the feet and hands the limbs were immediately drawn up and moved about. On blowing on the face the lower part of it was tremulously moved, and the mouth at each time opened; and three or four times an attempt to respire or gasp, accompanied by an apparently respiratory movement of the chest or thoracic convulsion, took place. The pulsations of the carotid arteries were also at first very distinctly observed. The umbilical pulsations gradually diminished in force and frequency, and when reduced to about ninety beats in the minute Dr K. cut the cord and allowed about a drachm of blood to ooze from its fœtal extremity, the heart's action immediately became quicker, and one or two thoracic convulsions afterwards followed. The mouth gaped repeatedly on blowing air on the features, the limbs gradually lost their reflex actions, and the heart's action, as seen against the thin walls of the chest, became more and more feeble; subsequently a few seconds elapsed between every observed pulsation, but more or less distinct movements occurred for nearly an hour from the time the fœtus was first expelled. On being weighed the fœtus was found to be exactly 9½ oz., in length it measured 8 inches, the placenta with attached cord weighed about 6 oz. The eyelids were adherent, the nose and ears closed, the mouth, however, being open; the membrana pupillaris was entire; on opening the chest the situation and appearance of the lungs and other organs were characteristic of its apparent age. The lungs in colour and volume resembled those of an early fœtus, and with the exception of one or two ecchymosed spots no colour or other evidence of developed air-cells were noticed, all the appearances indicating that no air whatever had ever reached the tissue of the lungs. The brain was afterwards minutely examined and also found to be characteristic of the apparent age, as were also the other fœtal organs.

Dr Keiller referred to the importance of such cases of early live birth, in regard to civil jurisprudence, but more especially to the questions, *Possessio Fratris*, *Tenancy by Courtesy*, *etc.*, to which the case he had now reported was in several respects related. It was frequently a matter of considerable importance to determine even the momentary existence of children at birth, and therefore the subject of *live birth*, and the evidence by which its occurrence is held to be substantiated, occasionally become of extreme consequence in a legal point of view, more especially in cases of contested lawsuits relative to the inheritance of, or the succession to, property. Dr Keiller directed the attention of the Society to the decisions that had from time to time been given respecting *what constituted live birth*, and referred to the more recent cases in civil jurisprudence in which the question of live birth was held to be established without any evidence of what was formerly demanded, *respiration or crying*. The mere muscular movements of the limbs, or the features, independent of any signs of respiration (as alone happened in the preceding case), having been ruled as sufficient evidence of a child being born alive, is therefore now held to be ample enough *proof* of what is termed in such investigations "live birth."

CASE OF TURNING FOR IMPACTION AT THE BRIM. BY HUGH SHARP, ESQ., SURGEON, CULLEN.

The following case was communicated to the society:—Mrs ———, æt. 37, at her ninth confinement was attended by Mr Sharp. Labour pains came on

suddenly while asleep, by the discharge of liquor amnii, at 1 A.M. As the os was contracted, a sedative was administered. The pains continued gradually to increase during the day, and at 4 P.M. the os was found fully dilated, and a large unyielding head presented at the brim. Very strong and frequent expulsive pains continued without any advance of the head until 10 P.M., when the patient's strength began to flag, pulse much increased in frequency, countenance anxious and flushed—a state of matters altogether different from any of her previous confinements, which were all comparatively easy, and of short duration. Mr Sharp then applied the long forceps without success, but ultimately succeeded in effecting the delivery of a large male child by the operation of turning, and which, though apparently dead when extracted, was resuscitated. The child weighed 11 lbs. 14½ oz. The mother and child did well.

CASE OF NON-MENSTRUATION. BY DR CUMMING.

Isabella K., æt. 21, when she first came under Dr Cumming's observation, had never menstruated. Dr C. had no record nor recollection of the ailments she had suffered from, before she became his patient—but when he was sent for, she laboured under symptoms simulating peritonitis. From these she recovered in two or three days, and was comparatively well for three weeks, when they recurred ; and it became obvious both from this and her previous history, that nature was attempting at each menstrual period to establish the regular discharge but failed—and that on failure of duly affecting the uterine organs, the *molimen* was expended elsewhere. It would be endless to detail the variety of forms and organs through which the *molimen* was made. Intense headache and abdominal pain were the most frequent causes of suffering, but she had vicarious discharges from the nose, eyes, and lungs at one time, a very peculiar eruption on the face at another, a hammer-like motion of one arm at another, and so on, till she exhausted almost every amenorrheal symptom on record. She had a great variety of treatment also, but nothing gave her more than temporary relief. With a view, however, to the discovery of the cause of the disease, and to its radical cure, an examination of the uterine organs was instituted. The vagina was of the normal length, and at the site of the os and cervix uteri, a very small tubercle was found with a depression in which a bougie was introduced. This Dr C. believed to be the os uteri, and it eventually proved to be so—but so minute were both the tubercle and the depression, that two medical friends who saw the patient along with him, and who had great experience in such examinations, maintained that he had *made* the aperture. The result of this examination was, that either the uterus was altogether wanting, or in a rudimentary condition, and nothing resembling the ovaries could be felt. After attending her from time to time for several years, Dr C. lost sight of her ; but somewhere about ten years from his first acquaintance with her, he was again sent for, and found her dying of phthisis. After her death, he procured with some difficulty a *post-mortem* examination, which was performed by Dr Handyside, and attended by Dr Simpson, and Dr White (of Buffalo, U. S.) The uterus was found to be half an inch in length, and the ovaries mere lines. Dr Cumming exhibited the uterus and appendages to the Society.

Dr Simpson mentioned, that he had seen a considerable number of cases of short or imperfectly developed uteri in the living subject. The imperfect development of the organ was ascertained, on examination by the finger, by the small and atrophic-like cervix uteri, and by actual admeasurement of the length of the cavity by the uterine sound. Instead of being 2½ inches in length, the cavity in such cases was only 2 inches, or more frequently only 1½ or 1 inch long. The subjects of this imperfect development were often well made and formed in other respects. But the malformation led to various functional defects, especially amenorrhea and sterility. The amenorrhea was usually persistent, and when a patient applied for medical relief, and who was already twenty-five or thirty years of age, this malformation would in a large propor-

tion of cases be found to be the organic cause. In some such cases of amenorrhea, there was great vascularity of the face and occasionally a most unconquerable form of acne. He had seen in some of these instances the wearing of an intra-uterine galvanic (or zinc and copper) pessary—gradually increased in size—followed by the best results, and even occasionally by cure of the amenorrhea. The uterus developed itself around such a foreign body, when it filled its cavity as it did around a fibrous tumour or an ovum.

THE DETERMINING CAUSE OF PARTURITION. BY PROFESSOR SIMPSON.

After stating and refuting the various theories that had been suggested on this point, such as the supposed origin of the act of labour in certain states of vital development or physical expansion of the fundus, body, or cervix uteri—in some supposed conditions of the fœtus, liquor amnii, or placenta, etc. Dr Simpson suggested that the loosening or decadence of the membranes, or membranes and placenta, from the interior of the uterus, constituted the determining cause of parturition; and that this loosening or decadence was itself the result of the effete degeneration of the structure of the decidua towards the full term of pregnancy. Various circumstances in obstetric physiology and pathology were stated in evidence of this view. It was so far also proved, experimentally, for we bring on labour artificially by imitating this process when we separate the membranes with the fingers or catheter; or when we inject tepid water into the cavity of the uterus, or, in other words, between the membranes and interior of the uterus—the latter a plan which Dr Simpson had followed in twenty or thirty cases.

SIMULTANEOUS EXISTENCE OF SMALL-POX AND COW-POX. BY PROFESSOR SIMPSON.

Dr S. stated the particulars of two fatal cases in which these two diseases co-existed. In both instances the two eruptions had appeared upon the skin about exactly the same time, and had each followed its natural progress,—seemingly unmodified by the other. In both cases there was a full and perfect vaccine vesicle; but yet the co-existing small-pox eruption was so great, and the attendant secondary fever so severe, as to destroy the patients. He pointed out the two general laws, that when these two diseases appear on the skin on the same day, or within one or two days of each other, the two affections usually, as in the preceding instances, pass through their natural courses, unaltered and unmodified, and unabated by each other; but, 2d, when one of the affections forestalls the other by a larger period, as by three, four, or more days, the eruption which appears last, whether cow-pox or small-pox, is usually more or less distinctly abridged and abated in its course, whilst the first, or earlier disease, does not undergo any change or curtailment in its own natural phenomena and progress.

Drs Weir, Thomson, Middleton, and others, reported similar cases. The following are the notes of Dr Middleton's case:—

A child, aged 5 months, was vaccinated on the 28th December 1852; the small-pox appeared on the 30th; the child died on the 10th January, the fourteenth day after vaccination, and twelfth from the appearance of small-pox. The vaccination vesicle began to inflame on the third day, and on the eighth it had attained its perfect form and size, with depressed surface. The eruption of the small-pox was confluent over the face and hands, but quite distinct over the feet and other parts of the body—not modified in any way. They were quite matured and full on the tenth day.

PROLAPSUS OF UTERUS DURING PREGNANCY.

Dr Graham Weir read the following communication from Dr Bone, assistant surgeon 97th Regiment Halifax, Nova Scotia:—

I was called, on Friday evening, 16th July 1852, to see Mrs Mulrain, aged 25, wife of a soldier, 97th Regiment, whom her husband reported to have met with an accident, and to be very ill. On my arrival at her lodgings I found her in bed, her countenance contracted, face pale, extremities cold, feet drawn upwards, pulse

quick. She informed me that when lifting a tub of water she felt a sudden pain in her back and side, and that something had given way within her, and that she was five months' pregnant. On turning down the bed-clothes a *tumour* was felt lying towards the right side and across the abdomen; on examination *it* was found to be projecting from the vagina, about the size of an infant's head, and covered with a mucous secretion, but not very painful on being touched.

Having passed my finger along the tumour I found the vagina filled up and distended by the mass; the os uteri could not be detected, the fœtus was without motion. There was no hemorrhage. Suspecting that the tumour was the uterus that had fallen down and was protruding, I asked if the same accident had happened to her before; she replied that after the birth of her first child the uterus had "come out," but not so much as at present.

Before my arrival the women in the room had put a bandage between her thighs, and tied it round her neck to support the tumour. Having lowered her head, and raised her hips with a pillow, I determined to try to reduce the mass by taxis. The left hand being oiled I seized the neck of the tumour and gently compressed it, and, by pressure directed upwards, endeavoured to replace the tumour. After a little resistance the protruded mass yielded, and returned inwards till it reached the promontory of the sacrum, and resisted all further efforts at reduction. The patient meantime moaning and complaining of great pain. I then discontinued the pressure, and applied cold cloths to the vagina and lower parts of the abdomen, and also dipped my hand in water, and rapidly applied it to the uterus within the vagina. The attendance of Mr Cay, the senior assistant surgeon of the regiment, was requested, and his permission asked to use chloroform. He was unwilling to sanction the use of it. After waiting some time, I renewed the pressure, and the uterus returned slowly to its place, when the finger came in contact with the os uteri. I afterwards examined the uterus through the parieties of the abdomen and found it upright. The woman was then turned on her back with the pillow still under her, and a bandage was applied above her hips and thighs, and as the vagina was relaxed I directed that some ice should be placed in it. I then gave her twenty drops of tincture of opium, and directed her husband to keep her for some time in the position in which she was then placed.

17*th July*.—She passed a quiet night—slept some time; but had vomited after I left her on the previous evening; no pain of the abdomen; the uterus in its place; *no movement of the child felt;* vagina nearly of its natural size.

℞ Olei. Ricini ℥ss.
Aqua Cinnamoni ℥j.

Fiat haustus statim sumendus. To take rice and milk.

Vespere 17*th July*.—Bowels freely moved by the oil used; soon after, the fœtus was felt freely moving.

18*th July*.—The child moves freely and vigorously. The patient feels quite strong, and was allowed to rise out of bed next day. Since then I have frequently seen her, and she continues till this date, 14th October, progressing favourably with her gestation.

That the uterus was prolapsed, and remained so for some time; that pressure was continued for about thirty minutes; that the uterus was replaced; and that abortion did not take place although such violence was used in returning the uterus—are facts that are in my opinion worthy of notice; for I cannot discover any record of the uterus being prolapsed and then replaced—the woman being pregnant, and gestation continuing afterwards.

Mrs Mulrain is short, flabby, of delicate constitution, and the mother of four children.

FATAL HEMORRHAGE IN CONNECTION WITH ABORTION, BY PROFESROR SIMPSON.

Various authors have described cases of fatal hemorrhage accompanying abortions. Dr Denman has given a drawing of the ovum in such a case still partially attached to the interior of the fundus. A fatal result from this

cause is, however, a very rare occurrence. Dr S. described a case to which he had been called some miles from town, where great hemorrhage had been going on for days, and the patient was already moribund. Dr S. removed with ease the ovum, which was impacted in the os uteri. The flooding ceased; but despite of the free use of stimulants, the patient sunk in two or three hours. An attempt was made at transfusion; but the surgeon failed to find a proper vein.

A conversation took place on the effects of the loss of blood in hemorrhages and in abortions. Several members mentioned cases demonstrating that immense loss of blood may be sustained in either conditions, without materially affecting the constitution, and showing that death rarely happened from such hemorrhages.

LIST OF GRADUATES

WHO OBTAINED THE DIPLOMA OF DOCTOR IN MEDICINE, IN THE UNIVERSITY OF EDINBURGH, ON THE 1ST AUGUST 1854.

*** *Those who have obtained Prizes for their Dissertations.*
** *Those selected for Competition for the Dissertation Prizes.*
* *Those commended for their Dissertations.*

Adlington, Robertus, Anglus. On Acute Pneumonia.
Aldridge, Russell, Anglus. On Tetanus.
Asher, Joannes Gordon, Scotus. On Inflammatory Diseases.
Barclay, Bruce, Thulensis. On Bronchitis.
*Bird, Robertus, Scotus. On the Physiology and Pathology of the Mind.
Birdwood, Georgius Christopher Molesworth, ex India Orientali. On the Origin of Ideas.
Borthwick, Alexander, Scotus. On Stricture of the Urethra.
Butterfield, Carolus Henricus, Bermudensis. On Rheumatic Fever.
Clegg, Gulielmus Jacobus, Anglus. On Puerperal Hæmorrhage.
*Collins, Franciscus, Anglus. On Animal Heat.
**Cowan, Michael Waistell, Scotus. On Exercise.
*Cross, Robertus, Anglus. On Hypochondriasis.
*Cullen, David, Scotus. On Alcoholic Liquors.
*Davenport, Carolus Jacobus, Anglus. On Dropsy.
*Dickson, Robertus Graham, Tripolensem On Ophthalmia.
*Ferguson, Fredericus Stuart, Anglus. Memoranda of Lectures on Clinical Surgery.
*Gibson, Gulielmus, Scotus. On Peripneumonia.
*Glover, Jacobus Grey, Anglus. On Cholera Morbus.
Gordon, Gulielmus, Scotus. On Vesicular Emphysema.
Gregory, Gulielmus Bartleman, ex America Septentrionali. On Cardiac Hypertrophy.
*Henderson, Thomas Arkell, Anglus. On Intus-Susception.
**Hickson, Joannes Georgius, Anglus. On Hydrophobia.
Hogue, David, Scotus. On Dental Caries.
*Inglis, Alexander Monro, Scotus. On Chloroform.
Jackson, Jacobus Rawlinson, ex India Orientali. On the Reproduction of Bone.
Johnston, Alexander, Scotus. On the Influence of Temperature.
***Johnston, Josephus, Scotus. On the Mucous Membrane of the Uterus.
**Kirk, Joannes, Scotus. On the Structure of the Kidney.
***Liddon, Eduardus, Anglus. On Vomiting of Sarcinæ and Torulæ.
Louttit, Jacobus, Scotus. On Diabetes Mellitus.
*Lucas, Franciscus Norman, ex Insula Taprobane. On the Diseases of Artizans and Labourers.
*Macfie, Gulielmus Henricus, Anglus. On Dysentery.
Mackay, Angus Joannes, e Capite Bonæ Spei. On Animal Mechanics.

Maclaren, Jacobus Duncanus, Scotus. On the Nutritive Process.
M'Allum, Donaldus Colemann, ex India Orientali. On Dropsy of the Amnion.
*M'Cullough, David Moore, Hibernus. On Dropsy.
Moore, David, Hibernus. On Anæsthesia in Labour.
Noble, Hugo, Scotus. On Euthanasia.
Paterson, Joannes Black, Anglus. On health and Disease.
*Phillippo, Jacobus Cecil, Jamaicensis. On Diseases of the Lungs and of the Heart.
Pringle, Robertus, Bengalensis. On Influence of Mind over the Body.
Reid, Douglas Arthurus, Anglus. On Puerperal Fevers.
Richardson, Gulielmus, Scotus. On Pleurisy.
*Rudd, Thomas, ex Insula Taprobane. On Urine as a Diagnostic Agent.
*Seddall, Joannes Vernon, ex Insula Melita. On Pulmonary Tuberculosis.
**Sibbald, Joannes, Scotus. On the Physiology of the Acrita.
*Sharpe, Robertus, Hibernus. On Dysentery.
*Smith, Carolus, Anglus. On Chloroform.
Stamers, Benjamin Henricus, Bermudensis. On Croup.
Syme, Georgius Henricus, e Capite Bonæ Spei. On Diseases of Joints.
*Taylor, Carolus, Anglus. On Criminal Abortion.
Walker, Jacobus, Novo-Brunsvicensis. On Erysipelas.
Wallace, Petrus, Gulielmus, ex Insula Mona. On Acute Rheumatism.
***Wilis, Matthæus, Anglus. On Endemic Bronchocele.
William, Evanus Pierce, Anglus. On Inflammatory Diseases, and Mercurial Treatment.
Young, Robertus, Scotus. On Puerperal Fever.

The promoter, Sir George Ballingall, after the usual ceremonies had been gone through, addressed the new graduates and a numerous audience, as follows :—

ADDRESS TO THE EDINBURGH GRADUATES OF 1854, BY SIR GEORGE BALLINGALL.

I HAVE to congratulate you, gentlemen, on the acquisition of the highest professional honour which we have it in our power to bestow, and on the successful result of your recent examinations. This must have been a period of anxiety—more or less—to all of you; and in this I can fully sympathise, having passed not less than five times through similar ordeals. Once before the College of Surgeons here, again before the College of Surgeons of London, again before the Army Medical Board, again before the Medical Faculty of this University, and lastly, a second time before the College of Surgeons of Edinburgh, when candidate for the Fellowship. I am free to confess, however, that all these successive examinations were not, in point of variety and extent, to be compared with those to which you have been subjected. But, while I believe this to be the case, you must also bear in mind the additional sources of information which are now opened up to you, both in the collateral and strictly professional branches of education. Since I was a student, and indeed since I became a professor in the University, several courses have been added to those requisite for obtaining a doctor's degree—Natural History, which has been, and will be, well taught in this school, is indispensable to any man who aspires to the elevated rank of a physician. Practical Anatomy, Clinical Surgery, and Medical Jurisprudence, are equally indispensable even to those who do not look beyond the grade of what is termed a general practitioner. When I look to the importance which these subjects have acquired in the general estimation of the profession, and to the extent to which they have been adopted in other schools, and other codes of education, I look back with a mixture of surprise and regret—surprise at the position which some of us have attained without a more careful study of those subjects in our earlier

years; and regret that I have only been enabled to pick up at intervals, in the course of a long professional life, some of that information which you have thus early, and more completely obtained. I look also with pride to the fact, that all the branches to which I have alluded, with the exception perhaps of the practical anatomy, originated here, and were first efficiently taught in the medical school of Edinburgh. Here it was that the teaching of medicine first assumed a systematic form, and here, I believe I may say, that all the more important additions have been made to it.

Many improvements both in teaching and in practice have, indeed, taken place since my early recollections of the profession. These it would be impossible for me to enumerate with accuracy, even if your time permitted. In medicine, one of the most remarkable which occurs to me at the moment is the diminished extent to which mercury is administered and blood withdrawn. In surgery, again, we have made such colossal strides, that I find it difficult to fix upon any leading feature; but certainly not the least important changes are the greater promptitude, and consequently greater success with which operations for hernia are undertaken; and the facility which has been shown to exist in the greater amputations—even the removal of limbs from the trunk; while the extended practice of minor amputations, and the excision of joints, have gone far to obviate more extensive mutilations. In the suggestion, and in the practice of these improvements, this school has certainly not been behind; and we look to its pupils for even more rapid progress than we have made. As your education has exceeded ours, so ought your practice more rapidly to improve. But, gentlemen, while I would urge you to the adoption of rational, well-considered, and substantial improvements, I would even more earnestly caution you against the hasty adoption of *new lights*, and the extravagant pretensions with which new practices, or new remedies, may be urged upon your attention. The number of these which have risen and decayed, which have flourished and have faded, under my own observation, have tended to supersede in my mind all the enthusiasm and credulity of youth, by more than, perhaps, a common share of the caution and scepticism of advanced life.

You have already become bound by the oath you have taken to practise your profession "caute caste *probeque*." This last significant word is, no doubt, meant to include what we express by the English word *honestly*; and I would not restrict this to the faithful administration of remedies in conformity to your principles, but would extend it to an open, straightforward, and manly expression of your opinions, and to a candid communication with your patient or his relatives, whether you are enabled to hold out hopes of relief and restoration to health, or whether circumstances compel you to make a communication of a very opposite description. You will sometimes, after having expressed your views of a case, find the friends or connections of a patient coming to you in an underhand or circuitous manner to ascertain your *real* opinion of his case, as if the profession was in the habit of acting on a system of duplicity—as if a man kept one opinion in his pocket, and gave another to his patient. These are applications by which I have never felt flattered, and to all of them I have had but one answer to give, that I had already stated my opinion of the case, and was quite ready to do so again to any one having an interest or a right to require it, but that I had no *two* opinions upon any case. Many are the lamentable consequences which I have known to occur from the want of plain speaking upon the part of professional men, and their hesitation, from a truckling subserviency to public prejudices, to call things by their right names. In this way, the truth sometimes bursts unexpectedly on a family so as to plunge them into the deepest affliction; and many are the cases in which serious losses have fallen upon families and individuals from the want of a timely communication from their professional attendants. One or two brief illustrations of my views will suffice. I had a patient in this town, an old brother officer, who had been severely wounded, and afterwards fell into a consumption. Every one about him fore-

saw that his end was near. He, on the contrary, apprehended no danger, but was talking of his dogs and his gun, and looking forward to the 12th of August, which he was never destined to see. It became an object of great importance, both to his spiritual and temporal concerns, that this young man's eyes should be opened to his perilous condition; and the duty of doing so was devolved upon a venerable and worthy clergyman, now no more. But the reverend doctor, who was much interested in the young man and his family, became overpowered by his feelings, and requested me to make the necessary communication. I went about it with all the delicacy I was master of, telling the patient, that although he might feel himself better, complaints would sometimes take an unexpected turn, that we should be prepared for the worst, and that it was right he should sign the papers necessary for the sale of his commission. He received this death-blow as might be expected from a gallant young soldier—called for pen and ink—the papers were signed before I left the room—and the price of his commission saved to his orphan sister. Compare this with the case of another young man, also a brother officer of my own; but here the circumstances were so far reversed, that the patient saw his approaching end, and was, I believe, well prepared for it, while the surgeon of his regiment was buoying him up with hopes of recovery. This young gentleman, at a morning visit, asked the surgeon, how long he thought he might live. The surgeon told him not to take such a desponding view of his case, and even hinted that things might yet take a favourable turn. The patient eyed him with a look, which I should not wish to have encountered, and said, "You know, sir, that you are talking nonsense; I merely want to know how long you think I can live." The surgeon was obliged to admit that he thought he could not live till night. His prognosis turned out to be correct; but his candour was, I think, greatly at fault. I would not have you to make abrupt or gloomy prognostications which may tend to their own fulfilment; but it is ten thousand times better that you should be mistaken, that the patient should recover and get a laugh at the doctor, than that he should be left to die unprepared.

I have already adverted to the increased extent, and consequently increased expense of medical education in the present day. As your opportunities of information increase, so also do your responsibilities—of him to whom much is given much also will be required. But I am grieved to think that the public have not always seen this in the proper point of view. While they derive all the advantages from the increased attainments of medical men, their liberality in remunerating them has not been proportionally increased; and while you are expected to keep pace with the progress of the science, little is done to facilitate you in economising your time and methodising your business; and here the profession itself is perhaps somewhat to blame. I am not going to inculcate upon you maxims of selfishness or of avarice. If I attempted this, I know that your youthful and generous natures would recoil from it. But there is, *or there has been*, a little too much affectation of disinterested labour, and of prosecuting science for its own sake, without regard to those emoluments, with which medical men in general are ill able to dispense. You have no doubt been well and wisely told that the prosecution of science will ultimately bring its own reward; but we must not let the world forget, that to prosecute science a man must live. In saying all this, I speak, personally, with feelings of the utmost gratitude to the inhabitants of this good town. But I am led into this train of thought from having so often, since I became resident in the country, witnessed the scanty returns of the country practitioner. There you may see a steady, intelligent, and well informed man toiling from day to day, in sunshine and in storm, often roused from his bed and obliged to travel miles in a boisterous night, and for all this rewarded with a pittance much more discreditable to him who offers it than to the man whose poverty, and not his will, compels him to receive it. In some instances, he does not even meet with

that extent of gratitude which was evinced by an honest woman in this neighbourhood, who told her surgeon, very candidly, that she had no means of paying him, but would give him *renown*. Nay, I have known an honest, an upright, and a benevolent man loaded with abuse, because, in a chronic and incurable case, where he could do no more, he refrained from paying his patient superfluous visits, or idle attentions, and thus making a job of the case as an unprincipled man would have done. There have been some indications lately that the profession begins to think, with me, that a spurious liberality has been carried a little too far, and I hope this will be followed up by those who have risen, or who may rise, to eminence and independence in the profession—it behoves such men to recollect that in addition to what may be due to themselves and their families, they have a duty to perform to their junior or less fortunate brethren. If medical men underrate their own services, it is not very likely that the public will correct their error.

There is another point upon which the profession has it still more within its power to promote its own comfort, and at the same time to benefit the public. I allude to those heedless, inconsiderate, and unseasonable calls which some families are in the habit of making upon their professional attendants, sometimes proceeding as if the doctor preferred working at unseasonable hours, sending for him at the moment he has sat down to his meal, or stepped into bed. In a case of accident, or of sudden illness, no man would grudge this ; but, when roused from your bed, after a hard day's work, it does not tend to enhance the comfort of a midnight visit, to find, upon seeing your patient, that, in as far as the urgency or the issue of the case is concerned, he might just as well have been seen the day before, or the day after. This, however, is an evil which, I feel confident, needs only to be pointed out to be in a great measure redressed. Let the public see that, by a little more attention to the arrangements of professional men, they would in every way be the gainers. You may be enabled to say (as I have often been when receiving a summons late in the afternoon), that you had passed your patient's door over and again in the course of the day, and that if you had got a message in the morning, you would have been enabled to see the patient many hours sooner, with great convenience to yourself, and with still greater advantage to him. You should show that you put a value on your own time ; that to enable a busy practitioner to discharge his duties successfully, he should have all the time which can possibly be afforded him for reading, for reflection, and for rest ; and that by a more seasonable call, wherever it is possible, a longer visit will be secured, and a more deliberate and attentive, may I add, a more kindly? consideration of the case. I know, from having repeatedly expressed myself in this way, that no sensible person will take it amiss, and that upon this point the public are prepared to listen to reason.

I wish, gentlemen, that I could be equally sanguine of carrying the public along with us on a point, in which its own interest is more deeply involved than ours. John Hunter used to say, that if it were not for the bad surgeons the good ones could not live ; and so it is, I believe, with every species of quackery—it ultimately succeeds in throwing back many patients into the hands of the regular practitioner, provided the unfortunate sufferers do not perish *in transitu*. But there is a propensity in the public mind to grasp at every novelty in practice, to cling to things which are mysterious, incomprehensible, or absurd, and this to such an extent as has led me sometimes to think that the public mind was insane on the subject of its own health. "There is," indeed, "no system of miscalled philosophy, especially if it meddle with the business of the physician, that is too outrageous for encouragement, or too ridiculous for admiration." I do not blame the public for believing what it cannot comprehend or explain. This, with the limited faculties with which an all-wise Providence has endowed us, is what we all must do. But the difference is this,—I believe most readily what I can best understand, while the public, in so far as physic is concerned, often swallows most greedily what is most novel and least comprehensible—its value,

apparently, bearing an inverse ratio to its departure from common sense. How this is best to be corrected, whether by reason or by ridicule, it is difficult to say. Certain I am, that it will not be done by railing and loss of temper. Our late distinguished professor of the practice of physic in this University, expressed himself as follows, to a gentleman who was reprobating some fashionable quackery of the day :—" There is no use, sir," said Dr Gregory, " in you or I railing against quackery—an Englishman values liberty above all things, and there is no liberty which he values so much as that of destroying his own health;" or, as I believe, the Doctor more emphatically expressed it, " going to the dogs in his own way."

These few hints, gentlemen, although hastily thrown together, I venture to offer to you as the reflections of one who, as an apprentice, a student, a practitioner, and a teacher, has now been more than fifty years attached to his profession. I offer them for your consideration, and most sincerely do I wish that they could be improved to your advantage.

To such of you as may be about to enter the public service, I may, under existing circumstances, be permitted to call to mind that here it is not only with the health and lives of individuals, but of legions that you may have to deal. From a letter, which I recently received from Dr Mackenzie, and of which an extract is published this day, in the " Monthly Journal of Medical Science," you will see that the Turkish army, which has fought so nobly, and gained so much by good generalship, is suffering more than enough from bad surgery, and the faulty condition of the hospitals. It has been well said that there is no man in an army upon whom more responsibility rests than on the chief of the medical staff. " Upon his judgment may depend the lives of thousands, the expenditure of millions, and finally, the safety or downfall of the state to which he belongs." I would also remind you of what your predecessors, in this, and other countries, have done to bring good out of evil. The improvement of the healing art is, I believe, the only redeeming quality which war brings along with it—the only benefit in which both parties, whether the victors or the vanquished, may equally participate.

The diminished numbers in which our graduates appear here this day is, I know, partly owing to the number of your comrades who have already gone to give their assistance to those brave men who are employed in turning the battle from our gates. I have been repeatedly consulted, since I came to town, by candidates, as to the propriety of their taking immediate service in the army or waiting for their degrees. I have told them, one and all, that were I in their position, I would not lose an hour's rank in the army in waiting for the degree, which can be obtained at a future period. I was myself one of fourteen old army surgeons who graduated here in one session, some five-and-thirty years ago; and I have always been of opinion that we were all the more leniently dealt with because of the experience which we had previously acquired in the public service. I know not whether all my colleagues will concur in the advice I have given, but I feel confident that I shall carry them all along with me in wishing you every possible happiness, and every possible success, personal and professional. You are about, my young friends, whether in civil or in military life, to enter upon a profession proverbial for its cares, anxieties, and toils, and for all which your reward, in many instances, must be the simple " luxury of doing good." But this in itself is great—yours is the Godlike office of giving health to the sick and comfort to the afflicted. " Homines ad Deos nulla re propius accedunt quam salutem hominibus dando."

WHY SHOULD THE DIPLOMA OF DOCTOR OF MEDICINE BE CONSIDERED AS MERELY HONORARY, AND AS GIVING NO RIGHT TO PRACTICE?

It seems to constitute a settled article of the faith of certain medical reformers, that they, as fellows of the colleges, are to possess the sole power of admitting

to practice, and that the universities who have hitherto possessed that privilege in conjunction with them are to be deprived of it, and their diplomas converted into so much waste parchment. These reformers have circulated the statement that this is a recognised principle of sound medical reform, universally admitted by the profession; and such is the influence of constant reiteration, that several who have not paid particular attention to the subject have thought that the narrow views of the few are truly the opinions of the many. It may be useful, in the present junction of affairs, to discuss the question placed at the head of this article, and point out what we conceive to be the injurious policy of the principle involved in it both to the public and to the profession.

We imagine that there are few who will dispute the propriety of medical men being as well educated as possible, and that it is for the interest of the public that the highest qualification rather than the lowest should be an object of their ambition. We take it for granted that the degree of Doctor of Medicine should only be given to him who has studied longer, and has passed a stricter examination than he who passes a board which licenses for general practice. The only circumstance which ought to be attended to is—as stated in an article in our June number—not to make the qualification for M.D. so much above that for general practice, as to place it beyond the reach of men of ordinary capacity and means. In short, the object of every well-wisher to medical education should be, while he tolerates the minimum knowledge, to encourage by every means in his power the desire for the higher degree. But it seems most absurd to insist, as is done by those who look to mere corporation advantages, that the man who voluntarily goes through the more extended and expensive education, and passes the highest examination, shall have no right to practise, but that he who limits himself to the minimum shall possess that right. Such, however, is the doctrine of certain parties in the royal colleges. A university education of four years, and the degree of M.D., they argue, ought to be considered of no legal value. But a provincial hospital education of three years and examination *by themselves* is what stamps the man. This is a specimen of medical legislation, which evidently has no reference to the good of the public, or the honour of the profession, but is directed solely to the acquirement of as many examination fees as possible to the lower examining boards, and to the deterioration of our universities, by preventing students from taking degrees. When the medical corporations are so evidently influenced by selfish considerations, well may legislators sneer at the distracting character of professional efforts at reform, and well may all honest men hail the determination of Lord Palmerston to have his committee or commission composed of LAYMEN, having no medical corporate interests, but possessing, we hope, a respectable modicum of common sense.

But it is argued that candidates for admission into the profession should be admitted only by the profession itself through its representatives. Who are these representatives? The reply made by our politicians of the College is, we are these representatives. Now this we beg leave to deny. The Colleges in Edinburgh, as in London and Dublin, consist principally of practitioners who reside in those cities, and who join the institution to obtain a status or local privileges. Their proportion to the mass of the profession is very small, and the assumption is altogether gratuitous. But supposing the Fellows of the College were acknowledged to be the representatives of the profession, does that qualify them to be examiners in the various branches of medical study? We say certainly not, because we hold that he only can examine well in a branch of science or art who is thoroughly acquainted with it. The only sound principle for electing such a body of examiners is that which induces the choice of men who are well known to possess the requisite knowledge, the only true foundation for professional respect. We do not, of course, deny that among the teachers of our colleges many such men are to be found, but it is not the mere circumstance of their being Fellows that renders them so. If good teachers are the men who may reasonably be expected to know best the subjects they profess,

these must be the examiners, and the only proper parties for admitting candidates to honours. We care not whether they be Fellows of Colleges or not. Let the profession be first satisfied that the ability to act exists, and then take measures if it chooses to see that that ability is properly exercised. But to follow the plan adopted by the Royal College of Surgeons of Edinburgh, and exclude teachers altogether from the examining board, is, we humbly think, tantamount to saying that such a board must necessarily, in certain departments, be very defective. Teachers, therefore, or men of eminence in their respective departments of science and practice, are the true representatives of the profession, and the only examiners in whom it can place confidence. And it follows that our Universities, which at least ought to possess the greatest minds, and most eminent teachers, are the institutions which should furnish the best examiners. If this mode of reasoning be correct, and we are satisfied that it is the only one which will bear examination, we may be induced to regard in its right aspect the mischievous tendency of that policy which desires to see University degrees and examinations rendered valueless, in order that examining fees may be secured by a body of collegiate Fellows.

REGULATIONS OF THE EDINBURGH TOWN COUNCIL RELATIVE TO THE DEGREE OF M.D. IN THE UNIVERSITY OF EDINBURGH.

THE Town-Council have lost no time in promulgating their statutes, relative to the degree of M.D. which are introduced as follows :—

"Edinburgh, 7th August 1854.

"The Magistrates and Council having considered the judgment recently pronounced by the House of Lords, in the processes of suspension and interdict and of declarator, at the instance of the Senatus Academicus, together with the Statutes of the University relative to the degree of M.D., sanctioned by the Magistrates and Council on 27th October 1846, and the Regulations as to Lecturers whose Courses of Lectures are to qualify for the degree of M.D., approved of on 26th January 1847, the operation of which Statutes and Regulations was suspended by reason of the said process of suspension and interdict; further, considering that in consequence of such suspension the Magistrates and Council, on 7th September 1847, revived and re-enacted the Statutes of 13th November 1833 (and which were rescinded by the foresaid Act of 27th October 1846), as interim Rules and Regulations, until such time as other or further Regulations should be enacted by the patrons, and that it is proper that the Statutes and Regulations first before mentioned should now receive effect and be acted on; therefore the Magistrates and Council, on report from the College Committee made in pursuance of the remit to them dated 6th June last, RECALLED the interim rules and regulations revived and re-enacted as aforesaid, and RESCINDED the same accordingly : REVIVED and RE-ENACTED the Statutes of the University relative to the degree of M.D., sanctioned by the Magistrates and Council on 27th October 1846, and the Regulations as to Lecturers whose Courses of Lectures are to qualify for the degree of M.D., approved of on 26th January 1847 ; and DIRECTED the Senatus Academicus to abide by, and conform to, the said Statutes and Regulations in every respect.

"The Magistrates and Council directed this Act to be extracted without abiding another reading in Council, and that the same, together with the Statutes and Regulations revived and re-enacted as aforesaid, be printed, and copies transmitted to the Senatus Academicus and to the Royal College of Physicians and Surgeons for their information and guidance.

"D. M'LAREN, *Lord Provost.*"

Then follow the same regulations as formerly existed, with the addition of a new section.

"SECT. III. Attendance on the lectures of Teachers of Medicine in the Hospital Schools of London, or School of the College of Surgeons in Dublin, or of Teachers of Medicine in Edinburgh, recognised as such by the Royal Colleges of Physicians and Surgeons of Edinburgh (in accordance with regulations to be adopted by these Colleges jointly, and approved of by the Patrons of the University), shall, to the extent of one-third of the whole department required by Section II., Clause 1, to be studied by Candidates, be held equivalent to attendance under Professors in this or in some other University, as already defined. And such attendance shall be available to Candidates to the extent of one of the four years of study required by Section I., provided it has embraced, in one year, at least two Six Months' Courses of Lectures, or one of these and two Three Months' Courses."

We are not of those who object to this section, as far as medical education is concerned, if the Royal Colleges of Physicians and Surgeons would perform the duties entrusted to them in a *bona fide* spirit, and would see that the examiners appointed are qualified for their duties. This is what the Town Council in common fairness are bound to see properly accomplished. We have too much reason to believe that hitherto the examination has been looked upon as a mere form, and that practically it is little better than a solemn farce. But now we trust the Colleges will be alive to the importance of the functions they have agreed to perform, and will take care in future to act up to the spirit of the regulations approved of by the patrons. This is only to be accomplished by choosing as an examining board, men, whom the whole profession are prepared to recognize as qualified, and seeing that he who professes to teach any branch of medicine, gives unequivocal evidence of his fitness for the important duties he is to perform.

VARIETIES.

INSTRUCTION OF THE DEAF AND DUMB.—Thomas Braidwood, who has been justly styled the Father of British instructors of the deaf and dumb, opened in Edinburgh, in 1760, an articulating school, on the system of Wallis. His institution was visited by Dr Johnson, and is alluded to in terms of commendation by the great lexicographer, in his tour to the Hebrides, in 1772. "The first attempt to found a public school for the gratuitous education of the deaf and dumb of Great Britain, was made at Bermondsey, in 1792, by the Revs. Messrs Townsend and Mason. It was committed to the care of Dr Watson, a nephew of Braidwood, and from it sprung the London Asylum in Kent Road, which was opened in 1807." Since then, schools for the gratuitous education of mutes, and solely supported by private benevolence, have been established at Edinburgh in 1810, afterwards in Birmingham, Manchester, and Liverpool, and subsequently in Exeter, Glasgow, and different other parts of England, Scotland, and Wales. The Governors of Donaldson's Hospital, at Edinburgh, one of the noblest institutions in the United Kingdom, have lately devoted a portion of it to the education and maintenance of the deaf and dumb; and in London an "Institution for providing employment and religious instruction to the adult deaf and dumb" has been lately established.—*Census of Ireland for* 1851.

THE UNIVERSITY OF LONDON BILL.—This most unjust and partial measure has received the sanction of the Legislature, but not without a most positive promise from the Home Minister, that the whole subject of Medical Reform shall be brought before Parliament next session. The efforts of the two Edinburgh Royal Colleges have thus only been successful in preventing the extension of the privileges of Oxford and Cambridge to the Universities of Scotland and Ireland, without checking the proceedings of the London senate. They can congratulate themselves on having rendered the monopoly complete, and on having inflicted a deep injury on the universities and their graduates.

There has of late been a great cry for Justice to Scotland. But what will the National Rights Association in Edinburgh say, on learning that their chief medical orator has been an active agent in preventing the Legislature from granting to Scotch graduates those privileges which are conceded to English ones.

What is a Junto?—Our employment of this word in the last number of the Journal, has given rise to great excitement among a certain party in the Edinburgh College of Physicians. Dictionaries have been ransacked, and an amount of philological learning displayed truly wonderful. The result seems to be that junto is a corruption from the Spanish *junta*, as grotto is a corruption from *grotta*. According to Johnson, junto is a cabal; a kind of men combined in any secret design. Again, a cabal, he says, differs from a party, as few from many. All this proves our correctness in the application of the term. But, says Webster, this word is never employed in English in a good sense, and our collegiate junto, in consequence, feels desperately offended. Of course we are deeply concerned at this, but a junto they are, and a junto they will remain to the end of the chapter.

More Nice than Wise.—The Chief Baron of England, disgusted at the nature of an action brought before him, suggested that the defendant should have pleaded the immorality and illegality of the transaction as a bar to the plaintiff's claim, and this would at once have put an end to the inquiry. But does morality, after all, gain by this suppression, or is the suppression of a symptom of disease to be accepted as a sign of health? The vice will not exist the less because justice is too nice to look at it. A bird is traduced by the common saying that it puts its head into a hole, and fancies it has blotted its enemy from existence; but nature has not produced any creature capable of such folly, and it is reserved for man to put his head into a wig, and to fancy that shutting profligacy out of view is extinguishing it, and serving morality.—*Examiner.*

The Law of Copyright in Foreign Works.—From the recent decision of the House of Lords, it must in future be taken as settled in law that an alien resident abroad possesses no right of exclusive publication recognised or protected in this country. Hitherto the United States of America have carried on a most extensive and lucrative trade by reprinting and underselling British medical publications. It will now be in the power of our own publishers to retaliate should any American work of importance be brought out. We trust that as the injury to authors and to literature will not now be so one-sided, it will be for the interest of both parties to frame an inter-national copyright.

The Cause of Amaurosis.—The following conversation is vouched for as being genuine, by one of our contributors, who has forwarded it to us:—

Q. What did you say was the matter with your husband's eyes?

A. The habberrosis the doctor calls it, please, Sir.

Q. The what?

A. Why, you see, Sir, his bowels went backwards instead of forwards, and when he went to the petty he strained so hard that he strained the nerves of his eyes, and that made the habberrosis—and he thought it was only the piles.

More English Law.—A duchess was proved to have been mad, and to have had disease of the brain of a serious nature shortly before she made a will. It has been decided, notwithstanding, that the will was valid. Ann Brough commits adultery, and is taxed with the crime by her husband. She then murders her six children, and attempts to cut her own throat. On her recovery she says that she committed the murders under "a dark cloud," and the jury decide that she was *non compos mentis.*

The New Board of Health.—This new board is to consist of a President, her Majesty's principal Secretary of State, and the President and Vice-Presi-

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dent for the time being of the Committee of Council appointed for the consideration of matters relating to Trade and Foreign Plantations. The President appointed is Sir Benjamin Hall, with a salary of L.2000 per annum. What a bitter satire is this on the medical profession of the country. A Board of Health without the presence of a single medical man!

Spirit Rapping Exposed.—The mechanism by which the sound is occasioned by some of the media has been determined by Dr Flint, of Louisville, formerly of Buffalo, in the United States, to consist in the voluntary displacement of the tendon of the perronæus longus from the sheath behind the external malleolus. The discovery has been lately confirmed by some observations and experiments by Dr Schiff, of Frankfort.

The Cholera.—The cholera is again becoming frequent in various parts of the country, especially towards the western coast. In Edinburgh there have been nine or ten cases. Our armies and fleets have been considerable sufferers, especially in the East. It is reported that in the Dobrudscha, the French troops have lost upwards of 7000 men from this disease.

Medical Attendance on Servants.—It has been ruled by the judge of the Mildenhall County Court in the case of Harris *versus* Scaber, that if a master send for a medical man to attend a servant, he is liable to pay for such attendance.

PUBLICATIONS RECEIVED.

A Treatise on Diseases of the Lungs, having especial reference to Consumption, including Diagnosis and Treatment. By Anthony William Clarke, M.D. London, 1854. 8vo. Pp. 259.

Clinical Handbook of Auscultation and Percussion; an Exposition, from first principles, of the method of investigating Diseases of the Respiratory and Circulating Organs. From the German of Weber. By John Cockle, A.M., M.D., etc. London, 1854. 8vo. Pp. 137.

Wanderings among the Wild Flowers; how to see and how to gather them; with two chapters on the Economical and Medicinal Uses of our Native Plants. By Spencer Thomson, M.D., etc. London, 1854. 12mo. Pp. 518.

A Handbook to the Peak of Derbyshire, and to the use of the Buxton Mineral Waters; or, Buxton in 1854. By William Henry Robertson, M.D., etc. London, 1854. Small 8vo. Pp. 227.

Moffat: its Walks and Wells; with incidental notices of its Botany and Geology. By William Keddie, prefaced by general remarks on Water, etc. By John Macadam, F.R.S.S.A., etc. Glasgow, 1854. 12mo. Pp. 163.

The True Scriptural Sabbath Vindicated and Enforced; and the Anti-Scriptural Character of what is called "The Christian Sabbath" Exposed; being a Review of Professor Miller's late pamphlet on the "Physiology of the Sabbath." By Robert Hamilton, M.D., F.R.S.E., etc. Edinburgh, 1854. Foolscap 8vo. Pp. 67.

A Letter to the President and Fellows of the Royal College of Physicians in relation to the Evidence, cited in their late Report, on the Treatment of Epidemic Cholera. By Joseph Ayre, M.D., etc. London, 1854. 8vo. Pp. 71.

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 17th of February 1854; prefaced by the announcement of the award of the Wollaston Palladium Medal and Proceeds of the Donation Fund for the same year. By Edward Forbes, Esq., President of the Society. London, 1854. 8vo. Pp. 65.

Statement of Cases Treated at Abington Abbey, near Northampton, during 1853; with a few Observations. By Thomas Prichard, M.D., etc. Northampton, 1854. 8vo. Pp. 22.

Charge to the Graduates of Jefferson Medical College of Philadelphia; delivered March 11, 1854. By Professor Robley Dunglison. Philadelphia, 1854. Royal 8vo. Pp. 20.

Sketch of Life and Character of Dr Abercrombie. Read before the Harveian Society of Edinburgh, at the Annual Festival, April 12, 1854. By Douglas Maclagan, M.D., F.R.S.E., etc. Reprint. Edinburgh, 1854. 8vo. Pp. 31.

Remarks on Double Capital Amputations. Elephantiasis Arabum of the Right Inferior Extremity successfully treated by Ligature of the Femoral Artery—Exsection of the entire Ulna. Reprints. By J. M. Carnochan, M.D., Professor of Surgery, New York. 8vo. Plates.

On the Pathology of Delirium Tremens, and its Treatment without Stimulants or Opiates. By Alexander Peddie, M.D., etc. Edinburgh, 1854. 8vo. Pp. 51.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Of the Weight and Specific Gravity of the Brain in the Insane.* By DAVID SKAE, M.D., F.R.C.S., Physician to the Royal Edinburgh Asylum.

THE most minute and careful examination of the brain in persons dying of insanity, have been prosecuted for many years in the largest asylums of this and other countries. The general results of all these observations amount to this, that in a very large proportion of cases there are found some degree of thickening and opacity of the arachnoid, and serous effusions into the sub-arachnoid tissue, into the arachnoid sac and ventricles of the brain, accompanied sometimes with increased, sometimes with diminished vascularity of the brain and its membranes. In the special form of disease called general paralysis, there appears to exist also a peculiar softening of the grey matter not indicated by any change appreciable to the eye, but by layers of the grey matter stripping off readily with the membranes, to which it often adheres, by the readiness with which it is broken up by a stream of water, and by an increase in the size of the nucleated cells of which the grey matter is principally composed.

With the exception of the morbid changes last mentioned, which are limited to cases of general paralysis alone, all the other morbid appearances are found very frequently in the brains of persons who have died of other diseases, and without any mental affection. And on the other hand, cases are not unfrequently to be met with in the deadhouse of the asylum, of patients who had exhibited during life —for many months or even years—all the symptoms of raving madness, and in whose brains the morbid appearances described are altogether wanting, and in which the brain could not be distinguished from that of one who had died in all the vigour of a sound mind.

From these facts it must be inferred that the morbid appearances described, although the very frequent concomitants of insanity, do not constitute the pathological conditions by which the symptoms of mental derangement are produced; and that we are, in fact, ignorant of the true pathology of this disease.

There are many considerations which may well lead us to doubt whether any other morbid changes in the structure of the brain may ever be discoverable in insanity. The analogy of the symptoms

to those produced by poisons which are known to enter the blood, the suddenness of the invasion in some cases, and the suddenness of the cure in others, even occasionally of long standing, the remarkable remissions, and at times the temporary restoration, for a few hours or moments, to perfect sanity in persons long plunged in profound dementia or labouring under a chronic and protracted mania, are facts which lead to the reflection that insanity may be a disease of the blood, or may depend upon a peculiar organism of the nervous centres, upon conditions not to be discovered in the brain after death.

Pathological investigations, however, for changes in the nervous substance itself, are not to be abandoned until we have exhausted all our means of observation; and it occurred to me, accordingly, some years ago to commence a series of experiments by collecting the weights of the brain, cerebrum and cerebellum, by measuring the grey matter, by endeavouring to determine the degree of hardness or softness of the cerebral substance, by ascertaining by what length of a column of water it could be broken up, and more lately by taking the specific gravity of the grey and white substance of different parts of the brain.

In an hospital for the insane, where the deaths are much fewer than in a large general hospital, it takes some years before a sufficient number of data can be accumulated to warrant any general inferences; and I feel that the data which I possess, although the accumulation of some years, are still rather meagre. I have been induced, however, to record them publicly, as offering a few very interesting suggestions, which may lead others to follow similar methods of observation, and thus facilitate the collection of a larger number of facts.

My measurements of the grey matter, and experiments on the hardness and softness of the cerebral substance, I do not propose to detail at present, as I have no similar observations on the brains of persons dying sane with which to compare them. I shall confine myself, therefore, to the weight and specific gravity of the brain, and to such general deductions only from those observations as the number of facts seem to warrant. I shall leave for another opportunity several comparisons and inquiries which might be instituted through these methods, inasmuch as the data seem to me too few to justify speculations which relate to more special questions.

I have compared my observations on the weights of the brain with those collected in the Royal Infirmary of Edinburgh, by the late Dr John Reid and Dr Peacock, and the experiments on its specific gravity, with those published by Dr Sankey in the *British and Foreign Medico-Chirurgical Review* for January 1853 (vol. xi., p. 240), and made in the Royal London Fever Hospital. The weights used were avoirdupois.

Weights.

The observations made of the weights of the brain comprise 172

cases, of which 83 were males and 89 females. The following tables exhibit the weights of the heaviest and lightest encephalon, cerebrum, and cerebellum (with the pons and medulla), and their average weights in the *males* at different periods of life. The figures are arranged in parallel columns with those of the corresponding results from the tables of Drs Reid and Peacock.

On comparing the columns showing the heaviest brains in the sane and insane, it will be seen that in only one instance did the encephalon exceed 60 ounces in the insane, while in several of the sane it amounted to 62 ounces and upwards, the heaviest brain in the one series being 60 oz. 8 dr., and the heaviest in the other being 62 oz. 12 dr. It will also be seen that, with two exceptions, the superiority in weight was among the brains of the *sane in each* of the quinquennial or decennial periods into which they are classified. Whether this justifies the inference that persons having large brains are less frequently the subject of mental derangement than others, I shall not venture to say; but the fact is a striking one when contrasted with the results derived from a comparison of the average weight of the entire number of cases, where it appears that the average weight is *increased* in persons dying insane. The average weight in the insane from 15 to 90 years of age being 30 oz. 4 dr., and in the sane 49 oz. 14 dr.

The same results are derived from a comparison of the weights of the brains of the females; here, in one exceptional case, the brain weighed $61\frac{1}{2}$ ounces in a female who died insane, but in a majority of cases the greatest weight was on the side of the sane. On taking the average of all the cases, the weight of the brain in the insane was 44 oz. 7 dr., and in the sane 44 oz. 5 dr.

This comparison establishes, I think, a strong presumption that the absolute weight of the brain is increased in the insane, a presumption which is greatly strengthened by the reflection, that in many cases of insanity the absolute *size* of the brain must be materially diminished by the large quantity of serous effusion found in the ventricles, arachnoid sac and sub-arachnoid tissue.

This increase in weight, however, appears to depend chiefly upon an increase in the weight of the *cerebellum*, for, on comparing the weights of the cerebra in the two series of cases, it will be found that the difference is inconsiderable, and indeed in the case of the females that it is in favour of the sane. On comparing, however, the columns showing the weight of the cerebellum with the pons and medulla in the two classes, it will be found that there is almost a uniform preponderance in the weights of those of the insane. The average weight in all the cases of the insane males exceeds that of the sane by 4 drachms, and in the females by 3 drachms.

In the two last columns of these tables the ratio between the cerebellum (with the pons and medulla) and the cerebrum is given at the different ages distinguished. Here the same fact is very clearly brought out; in almost every instance the cerebellum in the insane

COMPARATIVE WEIGHT OF BRAINS

TABLE I.—

Age.	Numbers Weighed.						Heaviest.								Encephalon.			
	Encephalon.		Cerebrum.		Cerebellum.		Encephalon.		Cerebrum.		Cerebellum.							
	Sane.	Insane	Sane.	Insane	Sane.	Insane	Sane.		Insane		Sane.		Insane		Sane.		Insane	
							oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.	oz.	dr.
16-20	8	3	7	3	7	3	56	0	56	0	47	0	48	8	7	2	7	8	40	4	40	0
20-25	16	7	10	7	11	7	61	2	57	12	54	0	52	0	7	2	7	0	47	0	48	0
25-30	24	8	15	8	15	8	56	8	55	4	49	0	48	8	7	0	7	8	38	0	44	8
30-40	41	19	28	19	28	19	62	8	56	0	54	8	49	8	8	8	7	12	40	8	44	0
40-50	44	23	32	23	32	23	62	12	58	0	49	0	50	8	7	10	8	0	34	0	41	12
50-55	22	8	20	8	21	8	59	0	56	8	51	0	50	4	8	4	7	0	42	2	41	0
55-60	10	6	8	6	8	6	52	14	60	8	46	12	53	12	6	14	6	12	39	0	41	4
60-70	18	7	10	6	12	6	60	4	52	8	51	13	45	8	7	4	7	8	40	0	44	0
70-80	5	1	5	1	5	1	54	10	57	8	48	2	51	0	6	8	6	8	43	8	57	8
80-90	3	1	3	1	3	1	52	0	48	0	45	8	41	12	6	8	6	4	40	12	48	0
	191	83	138	82	142	82	62	12	60	8	54	8	53	12	8	8	8	0	34	0	40	0
	Total,																	

TABLE II.—

13-16	1	1	1	1	1	1	41	0	39	0	35	8	33	8	5	8	5	8	41	0	39	0
16-20	13	1	9	1	9	1	49	12	39	8	43	8	35	0	6	8	4	8	40	14	39	8
20-25	13	7	10	7	10	7	50	0	48	0	43	4	41	11	6	1	6	5	44	0	37	12
25-30	13	5	11	5	11	5	50	0	48	0	43	14	42	0	6	2	8	0	39	0	38	8
30-40	33	25	25	25	25	25	54	0	61	8	45	2	54	8	6	8	7	8	38	0	39	8
40-50	23	21	18	21	18	21	53	0	51	0	47	0	44	8	7	0	6	8	36	12	37	0
50-55	5	8	4	8	4	8	48	6	52	0	42	14	46	0	5	15	6	8	43	14	37	8
55-60	2	8	2	8	2	8	44	0	55	0	38	12	49	0	5	4	6	8	43	4	41	8
60-70	14	13	14	13	14	13	48	12	50	0	41	2	44	0	6	0	7	0	36	2	34	12
	117	89	94	89	94	89	54	0	61	8	47	0	54	8	7	0	7	8	36	2	34	12
	Total.																					

being heavier in relation to the cerebrum than it is in the sane. From the average of the entire number, the cerebellum was found in the males to be as 1 to 6·8 in the insane, and as 1 to 7·06 in the sane; and in the females as 1 to 6·68 in the insane, and only 1 to 7 in the sane. The cerebellum therefore is considerably heavier in relation to the cerebrum in the insane.

This mode of estimating the results appears to me to be free from the source of fallacy arising from the small number of cases compared, when we compare the absolute weights together, as here, on examining all the cases separately we find, in almost every instance,

OF THE SANE AND INSANE.

MALES.

Lightest.				Average.						Ratio of Cerebellum to Cerebrum.	
Cerebrum.		Cerebellum.		Encephalon.		Cerebrum.		Cerebellum.			
Sane.	Insane	Sane.	Insane	Sane.	Insane	Sane.	Insane	Sane.	Insane	Sane.	Insane.
oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.		
34 0	34 12	5 8	5 4	50 13	48 11	43 9	42 9	6 6	6 1	1 to 6·8333	1 to 7·0206
41 7	41 8	5 12	6 0	52 3	52 1	45 10	45 8	6 6	6 9	1 „ 7·1569	1 „ 6·9333
38 0	37 4	5 4	5 8	49 10	50 9	43 7	43 14	6 1	6 11	1 „ 6·8557	1 „ 6·5607
35 9	37 8	5 1	6 0	51 1	50 12	45 2	44 0	6 6	6 11	1 „ 7·0784	1 „ 6·4860
37 13	36 0	5 6	5 0	49 4	49 3	43 5	42 6	6 5	6 12	1 „ 6·8614	1 „ 6·2777
36 6	35 0	5 8	6 0	51 4	48 8	44 15	42 8	6 2	6 4	1 „ 7·2143	1 „ 6·8
39 0	34 12	4 14	5 12	48 1	49 1	42 9	42 14	6 3	6 2	1 „ 6·8787	1 „ 7·0
34 8	38 10	4 14	5 14	48 8	48 3	43 8	42 2	5 13	6 9	1 „ 7·4839	1 „ 6·419
38 4	51 0	5 4	6 8	48 2	57 8	42 4	51 0	5 14	6 8	1 „ 7·0851	1 „ 7·8462
40 12	41 12	5 14	6 4	49 13	48 0	43 10	41 12	6 3	6 4	1 „ 7·0505	1 „ 6·68
34 0	34 12	4 14	5 0	49 14	50 4	43 11	43 13	6 3	6 7	1 to 7·0606	1 to 6·8058

FEMALES.

35	8	33	8	5	8	5	8	41	0	39	0	35	8	33	8	5	8	5	8	1 to 6·4545	1 to 5·9773
35	13	35	0	5	1	4	8	44	14	39	8	39	0	35	0	5	12	4	8	1 „ 6·7826	1 „ 7·7777
38	0	31	12	4	12	5	0	46	13	43	7	41	6	38	3	5	11	5	7	1 „ 7·2747	1 „ 7·0459
35	8	32	8	4	12	5	4	43	2	45	8	37	11	39	2	5	1	6	6	1 „ 7·25	1 „ 6·1372
32	12	33	8	4	4	5	4	45	3	45	15	39	1	39	13	5	9	6	2	1 „ 7·0213	1 „ 6·396
32	8	31	4	4	4	4	12	45	3	48	11	40	0	38	8	5	15	5	14	1 „ 6·7368	1 „ 6·553
38	1	32	4	4	13	5	0	46	0	44	8	40	6	38	12	5	6	5	11	1 „ 7·511	1 „ 6·813
38	2	35	12	5	2	5	0	43	10	47	14	35	10	40	10	5	3	6	0	1 „ 6·867	1 „ 6·66
31	2	30	0	5	0	4	12	43	3	44	5	37	10	38	6	5	9	5	15	1 „ 6·764	1 „ 6·77
31	2	30	0	4	4	4	8	44	5	44	7	38	8	38	0	5	8	5	11	1 to 7	1 to 6·81

Average.

the cerebellum of each brain heavier in relation to its own cerebrum in the insane than in the sane.

Further inquiries, and a larger number of data, may modify these results; but, I think, from those which I have thus collected, it may be regarded as highly probable that there is an increase in the weight of the cerebellum in persons dying insane.

On arranging the weights of the brain according to the form of disease under which the patients laboured, and taking the average, the following results were obtained:—

TABLE III.—MALES.

No.	Form of Mental Disease.	Heaviest.			Lightest.			Average.		
		Enceph.	Cerebrum.	Cerebel.	Enceph.	Cerebrum.	Cerebe .	Encephalon.	Cerebrum.	Cerebellum.
		oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.	oz. dr.
6	Mania, ...	60 8	53 12	7 0	51 4	44 4	5 12	54 $11\frac{1}{3}$	48 0	6 $11\frac{1}{3}$
14	Monomania, . . .	57 8	51 0	7 8	47 0	40 4	5 8	51 $11\frac{3}{7}$	45 $0\frac{6}{7}$	6 $10\frac{4}{7}$
20	Dementia, ...	56 0	49 8	8 0	40 0	34 12	5 0	50 $5\frac{4}{10}$	43 $11\frac{7}{10}$	6 $9\frac{7}{10}$
21	General Paralysis, . .	58 0	50 8	7 12	41 4	34 12	5 12	49 $12\frac{8}{21}$	42 $15\frac{13}{21}$	6 $12\frac{16}{21}$

IV.—FEMALES.

2	General Paralysis, . .	42 0	36 12	6 0	39 8	33 8	5 4	40 12	35 2	5 10
9	Mania, ...	49 0	42 0	7 0	39 8	35 0	4 8	44 $14\frac{2}{9}$	38 $15\frac{1}{9}$	5 $15\frac{1}{9}$
12	Monomania, . . .	51 8	46 0	6 8	38 3	32 8	5 8	44 0	38 $1\frac{1}{3}$	5 $14\frac{2}{3}$
26	Dementia, ...	55 0	49 0	7 8	37 0	31 4	5 0	46 $1\frac{14}{20}$	39 $15\frac{1}{26}$	6 $2\frac{13}{26}$

TABLE V.

Form of Mental Disease.	Average Enceph. to average Cerebellum, etc.		Average Cerebrum to average Cerebellum, etc.	
	Males.	Females.	Males.	Females.
Mania, ...	1 to ·1226	1 to ·1324	1 to ·1396	1 to ·1526
Monomania, ...	·1288	·1346	·1478	·1552
Dementia, ...	·1312	·1336	·1511	·1541
General Paralysis, ...	·1366	·1380	·1582	1601

In mania the average weight of the encephalon in males was 54 oz. $11\frac{1}{3}$ dr.; in monomania, 51 oz. $11\frac{3}{7}$ dr.; in dementia, 50 oz. $5\frac{4}{10}$ dr.; and in general paralysis, 49 oz. $12\frac{8}{21}$ dr.; the weight being greatest in mania, and least in general paralysis. In comparing the average weights of the cerebella, etc., however, in the same series, while they are found to follow the same decrease from mania, the highest, through monomania to dementia, the weights being respectively 6 oz. $11\frac{1}{3}$ dr., 6 oz. $10\frac{4}{7}$ dr., and 6 oz. $9\frac{7}{10}$ dr.; the cases of general paralysis present the highest average, the average weight being 6 oz. $12\frac{16}{21}$ dr. From the fact that in general paralysis the morbid appearances are most constant and most distinctly marked; this result might, perhaps, be anticipated, if we assume what I have endeavoured to show, that the relative weight of the cerebellum to the cerebrum is increased by insanity, and that the cerebellum is the organ principally affected. This increase, too, would appear from a review of the history of the cases to bear a constant relation to the form of the disease, and to be greatest in the more protracted and gravest cases. On comparing the average weight of the cerebrum to that of the cerebellum in the cases classified as above, it was found in males to be as 1 to ·1396—·1478—·1511 and ·1582, in mania, monomania, dementia, and general paralysis respectively. The same gradual increase in the relative weight of the cerebellum was found to obtain in the females, the cerebrum being to the cerebellum in them as 1 to ·1526—·1552—·1541, and ·1601, in same class of cases respectively.

It appears, therefore, that in cases of acute mania, a disease of comparatively short duration, there is the smallest amount of increase in the relative weight of the cerebellum, and that in general paralysis the greatest increase takes place.

In reflecting upon these results, the impaired control over the voluntary movements, which forms so marked a feature of general paralysis, and the very constant increase of the relative weight of the cerebellum in this disease, must at once be viewed as very interesting and important facts in connection with the functions ascribed to the cerebellum by Fleurens, and other physiologists.

May it not be asked—supposing it to be established by an extended series of observations, that there is uniformly an increase in the relative weight (and specific gravity, as we shall presently see,) of the cerebellum as compared with the cerebrum in all forms of insanity—whether it may not be inferred that the cerebellum is the organ through which we exercise self-control,—control over the volitions, and successions of our thoughts, as well as over the voluntary muscular movements;—a perturbed volition or a loss of self-control, being, of all other symptoms, the most essential characteristic and pathognomonic feature of insanity?

Specific Gravity.

The specific gravity of the grey and white substance of the brain

TABLE VI.—SPECIFIC GRAVITY OF THE GREY SUBSTANCE OF THE BRAIN IN THE SANE AND INSANE.

Ages.	Cerebrum.												Cerebellum in the Insane (Average of 27 Cases).	
	Males.				Females.				Both Sexes.					
	No. of Observations.		Mean Specific Gravity.		No. of Observations.		Mean Specific Gravity.		Total No. of Observations.		Mean Specific Gravity.			
	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Males.	Females.
15-20	4	2	1·037	1·045	11		1·0353		15	2	1·0357	1·045		
20-30	3	6	1·037	1·0401	6	2	1·037	1·035	9	8	1·037	1·0388	1·036	1·037
30-40	9	8	1·0355	1·0393	4	11	1·0336	1·039	13	19	1·0349	1·0392	1·041	1·041
40-50	9	4	1·0355	1·0367	7	8	1·0358	1·0372	16	12	1·0356	1·037	1·043	1·040
50-60	2	5	1·033	1·039	1	7	1·029	1·0374	3	12	1·0316	1·038	1·0407	1·038
60-70	6	4	1·0365	1·0352	4	5	1·0325	1·039	10	9	1·0349	1·0373	1·040	1·044
70-80					2		1·032		2		1·032			
Total,	33	29	1·0357	1·0392	35	33	1·0336	1·0375	68	62	1·0345	1·0391	1·0401	1·040
													Average.	

Lowest, { Sane, 1·028. Insane, 1·030. } Highest, { Sane, 1·046. Insane, 1·049. } Mean, { Sane, 1·0345. Insane, 1·0391.

TABLE VII.—SPECIFIC GRAVITY OF THE WHITE SUBSTANCE OF THE BRAIN IN THE SANE AND INSANE.

Ages.	Cerebrum.												Cerebellum in the Insane (Average of 27 Cases).	
	Males.				Females.				Both Sexes.					
	No. of Observations.		Mean Specific Gravity.		No. of Observations.		Mean Specific Gravity.		Total No. of Observations.		Mean Specific Gravity.			
	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Sane.	Insane.	Males.	Females.
15-20	5	2	1·0398	1·0455	11		1·0405	1·0	16	2	1·0401	1·0455		
20-30	3	6	1.0416	1·0458	6	2	1·0410	1·040	9	8	1·0412	1·0443	1·043	1·043
30-40	9	8	1·0419	1·044	4	11	1·040	1·0428	13	19	1·0413	1·0433	1·044	1·0444
40-50	8	4	1·0418	1·0402	7	8	1·0421	1·0422	15	12	1·0419	1·0416	1·048	1·043
50-60	3	5	1·0410	1·0424	1	7	1·042	1·0451	4	12	1·0412	1·044	1·0437	1·0425
60-70	6	4	1·0413	1·0415	4	5	1·041	1.0412	10	9	1·0411	1·0413	1·044	1·043
70-80					2		1·0410		2		1·0410			
Total,	34	29	1·0413	1·0432	35	33	1·0411	1·0422	69	62	1·0411	1·0424	1·0445	1·0432
													Average.	

Lowest, { Sane, 1·032. Insane, 1·034.

Highest, { Sane, 1·048. Insane, 1·053.

Mean, { Sane, 1·0411. Insane, 1·0424.

was taken in 62 cases, of which 29 were males and 33 females. In the foregoing tables the results are arranged in parallel columns with those of Dr Sankey, and classified into decennial periods of age. The one table exhibits the specific gravities of the grey, and the other of the white matter.

A glance at the tables will at once show that the specific gravity in the cases of insanity was almost uniformly higher, and this observation applies to both the grey and white matter.

GREY MATTER.

In Dr Sankey's cases the lowest specific gravity of the grey matter was 1028, in the Asylum ones the lowest was 1030; the highest in Dr Sankey's cases was 1046, in mine 1049; while the average specific gravity in all the cases of both sexes was, in the former, 1034, and in the latter, 1039, showing an increase in the specific gravity in the cases of insanity.

White Substance.

The lowest specific gravity of the medullary substance in the healthy brains was 1032, in the diseased 1034; the highest in the healthy was 1048, in the diseased 1053; the mean of all the cases was 10,411 in the sane, and 10,424 in the insane, showing an increase in the specific gravity of the white matter of the brain in cases of insanity.

These results are corroborative of those obtained by Dr Sankey, in his observations at the London Fever Hospital, where in all the cases complicated with cerebral symptoms of a grave character preceding death, such as convulsions, strabismus, paralysis, and utter unconsciousness, the specific gravity was high, averaging both in the grey and white matter 1041. He makes two exceptions in regard to the white matter, in both of which the specific gravity was below the mean—these were both cases of children of eight and ten years of age, and both of tuberculous meningitis. Deducting these cases, the average specific gravity of the white matter in his series of cases complicated with cerebral symptoms was 1043.

On examining my cases in detail, I find that in most of those cases where the specific gravity of the grey matter was considerably below the mean, the patients had died of phthisis, and in other instances of exhaustion occurring at an advanced age. Where exceptions to this general inference were met with, it was found that either the symptoms immediately preceding death were of a grave character, or that the morbid appearances found in the membranes indicated chronic inflammatory action.

I have also taken the specific gravity of the grey and white matter of the *cerebellum* in twenty-seven cases. I have placed on the table the averages at different ages. The number of observations is too few to warrant more than a presumption that the specific gravity of the cerebellum is higher than that of the cerebrum. The difference

is much more apparent when, instead of comparing averages, the specific gravity of the cerebellum is compared with that of the cerebrum to which it belonged.

I have not been able, as yet, to obtain a sufficient number of data to enable me to compare the specific gravity of the cerebellum of the sane with those I have made. Dr Sankey has kindly furnished me with a number of observations made by him, but as he has not separated the grey from the white matter, but has taken his specific gravity from a portion of brain containing both, I cannot compare my observations with his directly. I may state, however, that they tend to the same general conclusion, that the specific gravity of the cerebellum is higher than that of the cerebrum, and that it is so in healthy brains as well as in diseased ones, although probably in a smaller ratio.

Through the kindness of Dr Haldane, I have been enabled to make a few observations on the specific gravity of the grey and white matter of the cerebellum in persons dying in the Royal Infirmary. The number of cases suitable for a comparison of this kind, of cases dying without any cerebral symptoms, has as yet been rather too small to merit a special record. I may, however, state, that in a series of five cases of males dying without cerebral symptoms, the average specific gravity of the grey substances of the cerebellum was 1042, that of the cerebrum being 1041. In the cases of the insane examined by me the average was 1040 for the cerebellum, and 1035 only for the cerebrum, showing a much greater increase in the ratio in the insane than in the sane, although the absolute specific gravity was less in these particular cases.

The *white* substance of the cerebellum in the five cases referred to, had an average specific gravity of 1.0433, while that of the cerebrum was 1·0430. In the cases examined in the asylum the average specific gravity of the white substance of the cerebellum was 1·044, while that of the white substance of the cerebrum in the sane cases was 1·039 only—thus showing a small increase in the absolute specific gravity of the white substance of the cerebellum on the insane in the cases compared, and a very considerable increase in the relative specific gravity to that of the cerebrum in the case of the insane.

From these data, although limited, I infer that the specific gravity of the cerebellum is increased in insanity, and attains a greater increase in relation to that of the cerebrum than it does in persons dying sane.

In comparing the specific gravity in the different forms of mental disease, and taking the average of all the cases of each kind, I find the lowest specific gravity of the grey matter to occur in cases of dementia, where, however, it is still 0·003 above the average in the sane. The next highest specific gravity occurs in cases of melancholia and monomania, the next in general paralysis, the next in mania, and the highest in epilepsy.

Of the *white* substance, the lowest average of specific gravity occurred in cases of mania (1040), the next in dementia (1041), the next highest in general paralysis, the next in monomania (1044), and the highest also in epilepsy, being 1·0458.

The results of the preceding observations are curious and interesting. They are inferences certainly from a comparatively limited number of data, but they are, I think, sufficient to prove that more extended observations of a similar kind may probably lead to some satisfactory and important deductions regarding the pathology of insanity, and the functions of the brain.

In conclusion, I think it right to state that the specific gravity of the central substance was taken exactly in the same manner as that followed by Dr Sankey. A number of jars were prepared with solutions of common salt, the density of which was determined by the urinometer. A series was prepared, ranging from 1·030 up to 1·050. Small portions of cerebral matter were dropped into these solutions, until a jar was found in which the portion so dropped floated midway, at the point to which it sank. This fluid gave the specific gravity, and to insure accuracy, and avoid fallacies arising from the spontaneous evaporation of the fluid, their specific gravity was in every experiment tested afresh by the urinometer at the time of the observation. The temperature of the room was 60° F. It is right to repeat the precaution pointed out by Dr Sankey, namely, to take the first effect of the experiment, and that only; as by allowing the portion of brain to remain a few minutes only in the solution, its specific gravity rapidly alters by endosmose, and it will soon sink even in the strongest solutions. From not attending to this precaution, it is, I presume, to be explained why in the pathological report lately published of a metropolitan asylum, the brain is stated in many cases to have had a specific gravity of 1090 and upwards!

ARTICLE II.—*Cases of Artificial Pupil, with Remarks.* By BENJAMIN BELL, F.R.C.S.E.

THE medical man is liable to many disappointments in the course of his professional experience, encountering as he does on all hands, diseases and injuries which baffle his best directed skill and care. Sometimes he has the painful conviction that more accurate knowledge and larger experience might have procured a different result; sometimes he can console himself with the belief that there were specialties which no human sagacity or tact could overcome; but in either case, he feels how very weak and helpless we often are when our aid is most wanted. Still, in the midst of many discouraging circumstances, he is continually meeting with events of a brighter and more cheering character, which give him fresh energy and zeal in the performance of his professional duties, and make him more

than ever thankful that he follows a calling at once so useful and so honourable. Next in order to the pleasure of being a humble instrument in saving the life of a patient believed to be dying, we would place that which the surgeon experiences when he has been enabled to bring back the faculty of sight after a prolonged period of blindness. And this satisfaction is enhanced to both the patient and the surgeon, when the hope of recovery has been long abandoned. In cataract this seldom happens, because the vision of such cases is never completely gone, and they know that an operation may be expected to bring relief. But in cases that require the formation of an artificial pupil, the previous history is usually one of severe injury, of acute inflammatory action, or of troublesome ulceration; while the remaining power of vision seldom exceeds the mere recognition of a strong light, and therefore the unfortunate individual becomes gradually reconciled to his fate, and hopeless of a cure. Under these circumstances a successful operation is peculiarly gratifying to all concerned. Having recently met with several cases answering to this description, I venture to give a short detail of them in the present communication.

They were all cases which admitted of relief by incision of the iris; but as the procedure requires a good deal of consideration and care, in order to secure a favourable result, it may be useful to offer a few preliminary observations. Before doing so, however, I shall briefly enumerate and describe the various morbid conditions of the cornea, iris, and capsule, which demand the formation of an artificial pupil.

These may be classified under the following heads:—

1. An opacity of the cornea more or less extensive, even while the iris and capsule continue healthy, may render the pupil unserviceable, preventing the direct transmission of light. Sometimes this evil may be remedied in part, by causing dilatation of the pupil with belladonna or atropine; but in the more aggravated cases, it is necessary to extend the pupil towards the ciliary margin of the iris, by means of operative procedure.

2. A wound, or a penetrating ulcer of the cornea, may cause prolapsus of the iris, with contraction or even closure of the pupil. The latter, although altered in form and diminished in size, may sometimes continue large enough for useful vision, and under these circumstances, no judicious surgeon would think of operating; but when the dimensions and shape of the pupil have been much interfered with, it may be proper to attempt enlarging it by operation. In aggravated instances of this class, where the opacity of the cornea is very considerable, and where a narrow rim towards the circumference is the only transparent part, we may find it necessary to form an artificial pupil by detaching a portion of the iris at its ciliary margin, and fixing it in an opening of the opaque or opalescent cornea. In this class of cases the lens is sometimes absent; occasionally it is opaque; and under favourable circumstances it

may preserve its transparency. When this last is believed to be the case, every care should be taken to avoid wounding it during the performance of an operation; but if the lens and its capsule are opaque, the mere formation of a new pupil, however successfully executed, will not be sufficient, without some ulterior measures, for getting rid of the cataract.

3. Inflammation of the iris occasioned by injuries or by certain diseased conditions of the blood, may lead to adhesion and closure of the pupil. In some instances the pupil is filled entirely with lymph, and the patient's vision does not exceed a mere sensibility to light. In others there is a small central portion of the capsule, on which, if there be any deposited at all, the layer of lymph is extremely thin and filmy—a condition of the eye admitting of more vision than we might anticipate from the barely diaphonous appearance of the pupil. In the majority of cases caused by idiopathic inflammation, there is no reason to believe that the crystalline lens itself is either absent or opaque. Of course, opacity of the capsule at the part opposite the pupil, may seem to render the condition of the lens comparatively unimportant; but this is far from being the case when we come to consider and decide upon the practicability of an operation.

The preceding enumeration embraces, I believe, the various morbid conditions of the eye which admit of being improved by the formation of an artificial pupil; and we may now confine our attention more exclusively to those under the third head, to which belong the individual cases narrated in the sequel of this paper.

The condition of the lens, as already hinted, is a point of great importance in all cases of closed pupil depending upon adhesion of the iris to the capsule. If there is reason to believe that the lens has either escaped previously through a wound of the cornea, or has undergone solution, as often happens, after injuries, we are freed from a source of considerable embarrassment, and our line of procedure is comparatively simple. All that remains to be done is to make, if possible, a permanent opening in the iris for the ingress of light to the retina. It is unnecessary to say, that even this must not be attempted unless we have ascertained previously that the individual is able to distinguish light from darkness.

But it may be asked, how are we to discover that the lens is absent? Partly by the history of the case, partly by the form and size of the anterior chamber. If there has been a wound of the cornea, and the anterior chamber seems to be larger than usual, with more or less concavity of the iris, we are safe in deciding either that the lens has been lost, or that it has undergone a certain amount of solution. If, however, these signs are wanting—if the anterior chamber is of its ordinary size—if the quantity of lymph in the pupil is not great, and the iris retains some appearance of health, we have good grounds for believing that the lens remains *in situ*, and is probably little altered from its natural condition.

Sometimes we observe more or less bulging forward of the iris,

while the pupillary border is tucked backwards by reason of its union with the capsule. I am in the habit of drawing several inferences from this appearance of the parts; 1. That the lens is present; 2. That the posterior chamber is distended with aqueous humour; 3. That the portion of the capsule and lens corresponding to it is probably translucent.

A small anterior chamber with a general convexity or even flatness of the iris accompanying the closure of the pupil intimates too surely in most instances that the textures are matted together by inflammation, and that no operation will be of any service. In cases answering to this description, the iris is usually altered in colour and devoid of its wonted lustre; the sclerotic also has lost its pearly appearance, and varicose vessels are seen entering its texture, while the ball of the eye itself seldom possesses its natural firmness. Seeing such cases for the first time, we are tempted to hope that benefit may follow an operation; but in general it will be found that the result is unsatisfactory.

Having thus reviewed the circumstances which may help us to form an opinion as to the condition of the textures concerned in an operation for artificial pupil, we are ready for considering the various plans of procedure.

When the lens is gone and the iris tolerably healthy—that is to say fibrous looking and resplendent—a simple cut may suffice for the formation of an adequate pupil. It may be done either through the cornea or through the sclerotica. The former situation seems to be on the whole preferable, because the opening can be made without injury to any other texture; whereas the posterior incision unavoidably endangers the hyaloid membrane and ciliary processes. But, on the other hand, in operating through the sclerotica, there is no escape of aqueous humour or flaccidity of the iris to interfere with the permanency of the artificial pupil.

So much for cases where we are not embarrassed by the presence of the lens; but how ought we to proceed when the lens is present, and possibly free from opacity? Two operations have been recommended; and as both of them have answered the purpose of maintaining the integrity of the lens, they deserve a fair trial in favourable cases. The first is a modification of the operation by excision, and was introduced by the late Mr Tyrrell. It consists in penetrating the cornea near its margin with a broad needle, making a small wound in the iris near its point of adhesion to the capsule, introducing the blunt hook (Tyrrell's) into the opening thus made in the iris, and then carefully drawing a portion of the latter membrane through the wound in the cornea. The second operation may be regarded as the fruit of increased delicacy and excellence in the construction of ophthalmic instruments. A pair of extremely fine forceps (made by Luer after the pattern of Charriere) have been used by Mr White Cooper, in seizing a few fibres of the iris at the external border of the pupil, and drawing them very gently out of

the wound previously made in the cornea, with the result of forming a good sized and efficient pupil. But results so favourable are not to be looked for every day; and I believe that the most important point for our consideration is the treatment applicable to the generality of cases where the lens is present, without paying much regard to its condition as transparent or opaque. How then are we to deal under ordinary circumstances, with cases of closed pupil, when the crystalline lens still retains its integrity?

In the first instance the anterior division of the iris through the cornea may be tried, the operator remembering the close proximity of the lens, and the desirableness of preserving its transparency. Should an opening in the iris be the result, with the lens and its capsule free from opacity, he may congratulate both himself and his patient on the successful issue. But this is seldom to be looked for so speedily; and ulterior proceedings are generally found necessary. We must endeavour to get rid of the lens by repeating, from time to time, the operative procedure to which Mr Tyrrell gave the name of "drilling," and which is described by himself in the following terms: "The patient being placed as if to undergo the anterior operation for solution, I have passed a very fine straight needle through the cornea at the outer part; and then directing the point to the anterior capsule of the lens, close to the inner margin of the pupil (taking care not to injure the iris), and causing the instrument to penetrate the capsule, and enter the substance of the lens to the extent of about one-sixteenth of an inch, I have rotated the handle of the needle between the forefinger and thumb, so as to make the point act as a drill; and have thus secured an opening more free than could be effected by a simple puncture; then I have withdrawn the needle."

Our object in repeating this operation from time to time, is twofold; 1. To promote solution of the lens, the progress of which is indicated by a gradual enlargement of the anterior chamber; and, 2. To loosen the morbid connection betwixt the iris and capsule. When these objects have been more or less attained, we may then either divide the iris by posterior incision, or endeavour to remove the opaque capsule from the axis of vision, by means of a curved needle passed through the sclerotica. It may happen, while we are making the posterior incision, that a partial detachment of the opaque body takes place simultaneously, so as to promote rather than interfere with the end contemplated. This occurred in the case of William Hood, detailed in the sequel of this paper. Should simple incision of the iris fail in forming a pupil, recourse may be had to the operation recommended by Maunoir, and modified by others, which consists essentially in opening the cornea close to its border, introducing a fine pair of scissors, and making a V incision of the iris.

CASE I.—James Gibb, a healthy, intelligent man, twenty-eight years of

age, was severely injured about six months before coming under my care, by an explosion in the blasting of a rock. The right eye-ball had been completely destroyed, and was now atrophied and sunk in the socket. His countenance was much disfigured with black marks from the powder. The conjunctiva of the left eye was dotted with many dark spots, and there were a few similar deposits imbedded in the cornea itself. The anterior chamber was larger than natural, indicating more or less absorption of the crystalline lens. The iris retained its lustre, but adhered all round its pupillary margin to the capsule, which presented an opaque, yellow, and leathery appearance. Although vision was virtually gone, he could distinguish light from darkness.

The eye being free from all remains of inflammation, the iris brilliant, and the anterior chamber large, I preferred the operation for artificial pupil by incision through the cornea. Passing a sharp pointed knife cutting on both edges, and of medium size, obliquely through the cornea at its outer margin, I penetrated the iris at its outer and lower part, near its connection with the capsule. The lips of the wound immediately separated, forming a very tolerable pupil of a triangular shape, as represented in the drawing. Within a week he could distinguish large objects, and now, after the lapse of several months, he reads ordinary print with facility, aided by a convex lens, and is able to support his family by means of his own exertions.

Case II.—William Hood, an inmate of the Asylum for the Blind, was eighteen years of age when he came under my notice, and had been blind, or nearly so, for more than ten years. In the right eye the pupil was obliterated by complete adhesion of its margin to a large central opacity of the cornea; in the left, the cornea was transparent, but the iris adhered to the capsule, which was opaque and covered with lymph. Both eyes retained their natural shape and firmness. With the right he could barely distinguish light from darkness; with the left, the sensibility to light was greater, but no objects were discernible. The normal condition of the cornea pointed to the left eye as the more favourable of the two for operative interference; although my anticipation of a successful issue was not great, from the long period of time in which the retina had been out of use. I began with performing Mr Tyrrell's operation of "drilling" already described, and repeated it after an interval of some weeks. Subsequently to this, as it seemed probable from the increased dimensions of the anterior chamber, that little of the crystalline lens remained, I attempted to divide the iris by means of a narrow knife introduced through the sclerotica. Partly by incision, partly by separation of the iris from the capsule at its inner aspect, a pupil of good size was formed; and the young man was soon able to follow out-door occupations, and in a few months, to read ordinary print with ease and comfort. The progressive recovery of function by the retina in this case, has been very remarkable.

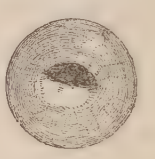
Case III.—Robert Stevenson, a fine lad of eighteen, suffered from a severe inflammatory affection of both eyes, more than three years ago, which resulted in almost total blindness. There was complete *synechia posterior*, with opacity of the capsule in both eyes. The irides were somewhat convex, and the anterior chambers small. Besides these morbid changes, a certain amount of cloudiness was present in both corneæ, but in the left, fortunately a little below the level of the pupil; while in the right it was placed directly in front. He possessed a very little sight in the left eye, which he valued highly; but in the other eye it was a mere sensibility to light.

The left seemed the more favourable of the two for operative interference,

from the cloudy portion of the cornea being more out of the way of the axis of vision.

Before attempting to divide the iris, I introduced a straight needle on two different occasions through the capsule, at its point of union with the iris, and performed the operation of *drilling* as in the preceding case. The immediate effect of this was to aggravate the capsular opacity, and discourage the poor lad not a little by lessening the sight that he had; but the anterior chamber gradually became more capacious, and at length there seemed to be no reason against attempting the formation of an artificial pupil by incision of the iris.

The previous history of the case, which was one of protracted inflammation, seemed to point out the necessity for an opening of considerable size, as the lips of the incision were not likely to gape with much freedom, and I accordingly had recourse to the posterior operation through the sclerotica. The iris happened to be in a better state than was anticipated, and its fibres started as under, forming an oval-shaped pupil of good dimensions. There was neither bleeding nor inflammation; and the young man now sees remarkably well, and aided by a lens can read even small print with perfect facility.



Case IV.—Robert Nichol, æt. 19, about eleven years ago had his right eye destroyed by a blow from a stone. It is now atrophied and quite sunk in the orbit. The left eye became severely inflamed shortly after the accident, and he has been blind, or nearly so, ever since. The pupil is very small, and occupied by an opaque capsule to which it adheres all around. A few weeks ago, I passed a sharp-pointed knife through the cornea, and transfixed the iris at no great distance from its connection with the capsule. A pupil of good size was formed at the time, but next morning, after the anterior chamber was again filled with aqueous humour, the new pupil had disappeared, the edges of the wound being in contact. For a few days the scanty vision which he had enjoyed was sensibly diminished; but it has gradually returned, and now he can see almost as well as formerly. That he should ever have been able to discern objects at all is rather remarkable; but, on careful and close inspection, there seems to be a very minute chink at the outer and lower margin of the pupil.

July 18, 1854.—The eye-ball being free from vascularity, I this forenoon introduced a narrow iris knife through the sclerotica at its outer aspect, brought the point through the iris, mid-way betwixt its ciliary border and the pupil, and then carrying it inward and in front of the iris, endeavoured to make an adequate incision of the latter. On withdrawing the knife, a considerable quantity of flocculent substance, evidently a portion of opaque lens, issued from the line of incision, hiding its extent and occupying a portion of the anterior chamber. I expect this to be dissolved in the course of a few days, when, not improbably, an artificial pupil will be found at the point from which the exudation escaped.

20*th*.—No inflammation or uneasiness in the eye.

August 1*st*.—The portion of lens in the anterior chamber has disappeared. An opening in the iris is now visible, and although it is occupied by an opaque body, he has more light and can discern any shining object towards the left hand.

15*th*.—The portion of opaque lens or capsule visible through the opening in the iris is now distinctly movable, disclosing in certain positions of the globe a small black opening at the lower lip of the incision, and giving him, for a moment or two, an unobstructed perception of light.

Sept. 2*d*.—The opening in the iris is now entirely free and quite black, presenting the appearance of a spindle-shaped pupil of moderate size. His sight

improves from day to day, and is as good as could be looked for, considering that the retina has been almost completely disused for so many years. I see no reason to doubt, that in the course of a few months, it will have recovered itself sufficiently, as in Case II., to admit of his reading ordinary print with comfort.

ARTICLE III.—*Considerations in regard to the Treatment of Displacement of the Uterus.* By J. MATTHEWS DUNCAN, M.D., F.R.C.P.E., Lecturer on Midwifery, etc., etc.

IN a late meeting at the Obstetric Society of Edinburgh, I adduced the main part of the following observations in regard to the general aspects of this difficult therapeutic question. Although originally forming part of the discussion of a communication by Professor Simpson, I am induced to publish them now by the pressing interest and importance of the question at this time.

The point of greatest weight to insist upon first of all, is the difficulty of arriving logically at conclusions on this subject having even moderately good claims for acceptance. "The impossibility in these inquiries of commanding all the conditions of any experiment, or continued observation, so as to leave out one after another of those conditions in each repetition of the observation, and have an *instantia crucis* as to any one of them on the result."

In applying the numerical, along with other methods of inquiry, to such therapeutic questions, "The probability is, that the efficacy of the measures under observation will be overrated, because the desired result is the *positive* one of the recovery of patients (not *negative*, as in questions of etiology, having for their practical end the prevention of disease); we know that in almost every case, various causes besides that under trial have contributed to that result; in acute cases, especially, the salutary provisions of nature for the decline of diseases, or, as we may very often more correctly express it, the essentially temporary nature of the diseased action itself; in chronic cases, more remarkably, the unobserved agency of other internal circumstances besides the remedy in question. Of the degree in which these causes have contributed to the fortunate event of any individual case, all candid and intelligent medical men will allow that it is very difficult to judge; and without judging of them, we can have no certain inference as to the power of any remedy."[1]

In the subject before us all these difficulties and more exist, and we have one of the most complicated and ravelled questions in pathology to solve. The treatment of uterine displacements by intra-uterine pessaries has been now for a considerable time before the profession, and hitherto the difficulties in the way of accurately estimating the value of the treatment have proved so for-

[1] Alison.

midable, as to deter obstetricians from entering fully into the question. A great deal has been written about the frequency and the diagnosis of these affections, and about the mechanical modes of treatment by intra-uterine pessaries, how they are to be introduced, and how used; but the value and results of the treatment have only lately been discussed. I do not here allude to the short statements which have frequently appeared in the medical press, and which have generally been summary condemnations of the practice, or equally groundless laudations of it. Such opinions have a place in the question as mere opinions, weighing on one side or the other; but they do not assist the real progress of the inquiry.

The difficulties, indeed, of this as of many other therapeutic questions, are so great as to render any absolute or logically adjusted resolution of it all but impossible. I may refer, in illustration, to the great question of large blood-lettings in serious acute inflammations, as one similarly complicated, and which, if even now decided, does not certainly owe this to the writings of eminent or philosophical physicians (however much influence such may justly have in guiding the professional mind), or to the debates of learned societies, or to the diligence of statisticians. This question has been decided by the clinical experience of the profession. The revolution from large and repeated blood-lettings, as formerly, to the more guarded use of the lancet, as at present, is mainly due to a gradually acquired consciousness on the part of the leading men of the profession as to the advantages of the newer method. To say the least, no amount of writing, debate, or of statistics, could have effected this without the gradually acquired consciousness of the profession in accordance with the newer views. And assuredly this question of the treatment of displacements of the uterus, must also rest on this professional consciousness for its final arbitrement. The difficulties surrounding it yield to those of no other subject.

We are indebted to the fatal results of the treatment in M. Broca's and in M. Cruveilhier's cases, for the stirring up of this question in the heart of the Academy of Medicine of France, and assuredly it could not have fallen into better or abler hands. The attentive perusal of this animated and very able discussion, while it will amply repay the student, will sufficiently attest the truth of the remarks I have just made. Nothing can be clearer than that the question is not brought many degrees nearer a logical issue by this unequalled passage of arms; and it must be gratifying to reflect, that there is another bar by which the question will be tardily but solidly decided, namely, that of the general consciousness of the profession.

The chief difficulties to be kept in mind in the question before us, may be enumerated as follows:—

1. The well-known facility of all patients, and especially of females, inducing them to admit their feeling relief from treatment without good grounds. Every physician must have observed this in a thousand different circumstances; and there can be no doubt

it is much more liable to happen when the physician enjoys great notoriety or *eclât*. In the case before us, this amiable failing in females is aided by the novel character of the treatment, and the imposing aspect of the instruments.

2. The impossibility of deciding the gravity of the affection. The symptoms calling for relief in the cases considered are, painful feelings of various character and site. For the description of these feelings, and the estimation of their intensity, the physician is completely at the mercy of his patient, and is very liable to form misconceptions on the subject. Nervous and hysterical females will probably exaggerate in their statements, and not a few will underestimate their sufferings. Nor can it be forgotten that malingering, of a modified kind, is not very rare among women in good society.

3. The impossibility of deciding what symptoms, in any case, are due to the simple displacement, and of separating these from what are caused by the engorgement or inflammation of the womb, by irritability of the womb, or of the neighbouring organs, or by other more obscure neuralgic conditions.

Under this head may be mentioned the confusion apt to arise in discussing this subject, from physicians looking at and describing cases from different "points of view." The case of a Lancashire lady, which was lately the occasion of some sharp professional correspondence, may be taken to illustrate this matter. The lady is apparently one of a class who have confidence in different doctors and practices "time about," and whose statements can be of little value. Her case was diagnosed in London by some most eminent obstetricians, as one of fibrous tumour in the back wall of the uterus. In Edinburgh, it was diagnosed by a very eminent accoucheur, as retroversion. If the case had come into my hands, I should possibly have thought it was neither a case of fibrous tumour nor of retroversion, but of enlargement of the uterus. All these statements may have been correct and consonant, although at first sight different. The lady *may* have had an engorged and retroverted uterus, with a fibrous tumour in the posterior wall, and different physicians *may* have seen reason to ascribe the symptoms complained of to one or other of these different unnatural conditions. The great error in this case was the assertion of an exclusive diagnosis by one practitioner as against another. The retroversion could be diagnosed without any difficulty. The existence of a small fibrous tumour (and in such a case the supposed or real tumour must be very small) in the posterior wall of the uterus, if it do not cause deformity of that wall, cannot with certainty be contradicted, after the most careful examination during life, and great doubt will be cast over the case if the uterus is engorged and hypertrophied.

4. The impossibility of saying what part of the good or bad results observed after the treatment is to be attributed to the mechanical retention in the normal position, and what to other conditions of accident, regimen, or simultaneous and diversified treatment. The

question even of death as the result of this treatment, has been in some cases successfully perplexed by arguments too ingeniously based on this dilemma. In hearing the mechanical treatment argued for, I have been amused to observe the greediness to seize upon and assert the occurrence of fatal cases from the most trivial uterine operations, such as the application of caustic, or even from inappreciable undiscoverable causes, while at the same time the greatest pains are taken to discountenance the very idea of fatal results from the permanent intra-uterine pessaries. These instruments are admitted on all hands to cause serious constitutional disturbance occasionally, and sometimes pelvic abscess. The latter affection is well known to be sometimes unavoidably fatal. The partisans, then, of intra-uterine instruments, if they admit (as they do) that they sometimes cause serious pelvic inflammation and abscess, must also admit that they may be directly influential in causing death.

In the public discussions of the value of intra-uterine instruments, it has been painful to observe the levity with which the fatal results of their use have been descanted upon. These deadly consequences have generally been spoken of as mere negative results of the treatment, as cases of failure. Some of the partisans of this method have even attempted to gloss these dire results, by pointing out that death is sometimes the result of treatment far less imposing in severity than this, and that deaths in general are wonderfully easily caused in some individuals. A French academician has distinguished himself by reaching the climax of this absurdity. While commenting on the mortality among M. Valleix's cases, he exclaimed, in the midst of the Academy of Medicine (and he was not answered), that 5 or 6 deaths in the treatment of 160 cases, of a disease which was always innocent of such results, was not of much importance,[1] and that the instruments are not dangerous! I have frequently heard it boasted of the British school of medicine, that opinions and practice such as that spoken of and defended by M. Velpeau, would not for one moment be tolerated in the professional circles of our country. And I trust we shall never have much reason to defend our profession from the shame of similar views, or our fellow-citizens from being the victims of such practice.

It must not be forgotten, that a certain allowance must be made to the partisans, properly so called, of these intra-uterine instru-

[1] M. Depaul a cité plusieurs cas malheureux de M. Valleix lui-mème ; sans doute il y a eu plusieurs cas malheureux qui peuvent être attribués à une fausse manœuvre ; mais, en définitive, 5 ou 6 cas malheureux sur 160 observations, c'est fâcheux sans doute, mais ce n'est pas encore de nature a faire rejeter le moyen ; et il resterait à démontrer d'ailleurs que les accidents dont il s'agit doivent réellement être imputés au redresseur.—SPEECH OF M. VELPEAU.—*Gazette Médicale*, 8 Juillet 1854, p. 415.—It is only just to M. Velpeau to add, that his own practice indicates very different opinions from those expressed in his speech. He himself desisted from using intra-uterine pessaries, long before they were known in Edinburgh, because *accidents* (not death) occurred in some of his cases.

ments, in consideration of their very partisanship, of the novelty of the subject, and of the flattery of many patients and practitioners. Such influences have a wonderful power over most minds, in shutting out the true view of the cause of evil results, and of their importance. But a very little reflection will place the matter in a proper light for every one.

The *gravity and fatality of the disease treated* ought undoubtedly to determine the importance which we should attach to death as a result of treatment. The *severity of the treatment itself* ought to have no weight, unless such treatment have been used to test the powers of endurance of the subject of it.

Uterine displacements are described by those who attribute to them the greatest importance, as affections causing painful feelings of various kinds, and sometimes difficulty and pain in walking, defecation, urination, and menstruation. Fatal consequences are not attributed to them, unless in a very remote degree of indirectness. That such affections, then, should not be subjected to a plan of treatment which is unavoidably fatal in instances which are not extremely rare, is a proposition which requires only to be stated to command assent. And if this be admitted, it follows *a fortiori*, that a treatment which not only occasionally causes death, but frequently induces other diseases, equally bad with the original complaint, and, in any case, rarely if ever cures the displacement, cannot receive the sanction of the profession. If the treatment of uterine diseases by intra-uterine pessaries comes within the scope of these propositions, then their condemnation is inevitable.

Fatal results, and numerous injurious consequences, have resulted, here and elsewhere, from intra-uterine instruments. This undoubted fact is sufficient to show the necessity for the most careful use of these instruments, and their restriction to cases of a serious description. But these fatal results and injurious consequences may not have been, or may not be, inevitable. They may not have been inevitable, but the results of unskilfulness, foolhardiness, or ignorance, or neglect of the conditions for the safe use of the instruments. They may not be inevitable, for experience and anxious care may teach us the conditions necessary for safety in the treatment.

A great body of the profession has already a decided opinion in regard to these instruments. Some reject them entirely as dangerous to life; some as frequently, if not invariably, inducing diseases at least equally distressing with that which they are proposed to remedy; others as being, to say the least, quite inefficacious.

I have already said, that if the question of death as a necessary occasional result of the treatment under discussion were decided in the affirmative, then the therapeutic applications of permanent intra-uterine pessaries would be easily limited. But it is necessary to notice some ingenious methods of concealing the force of this argument adduced by the defenders of these instruments. It has frequently been argued that they were not dangerous, because in many

cases they had been worn for months, or even years, without causing death, or any intolerable symptoms. But it is clear that this argument is good only for those cases. Such cases show that intra-uterine instruments may be long worn without fatal or dangerous results in certain cases; but they do not affect the argument of danger equally well demonstrated in the cases of death. There is no method of discovering beforehand which are the cases in which the instruments are to be safe, and which are not. Every case takes part in the risk. To use an illustration, the frequent successful amputations of the thigh, or excisions of the mamma, do not blind the eyes of surgeons to the dangers and fatality of these operations. If it were possible to predict in what cases the amputation or excision would not be fatal, then these operations might be applied to the treatment of the simply painful and not dangerous affections of the knee or breast. The same might be done, were it possible by any care or attention to ward off the fatal result. But surgeons are convinced that amputation of the thigh, and excision of the mamma, are operations which have a certain amount of necessary fatality. They therefore refuse to apply these operations to the treatment of affections of the knee or breast which are not in themselves dangerous to life, or productive of serious and otherwise incurable suffering. It is evident, then, that remarkable endurance of the intra-uterine instruments in certain cases (just as successful amputations and excisions) does not of itself justify the use of these instruments in any case.

Another argument which has been often repeated in defence of intra-uterine instruments, requires only to be mentioned. It has been shown that a large number of these intra-uterine instruments have been purchased from a fortunate surgical cutler. This is exactly the point at which these instruments are of no therapeutic importance. It is not the display of them on the shopman's counter, nor the manipulation of them by purchasers, that is important. The amazement (or amusement) of the patient may be important, but what certainly is so, is their introduction and retention in the female passages, with their ultimate results.

It may here be useful to direct attention to the manner in which surgeons view the occurrence of death after one operation, in their discussions of the new remedy for stricture of the urethra. That disease is one of great danger, and having a very considerable fatality, yet the occasional occurrence of death from the operation has been urged as sufficient reason for its total abandonment, and all the energy of the supporters of the new method has been called out to defend it. But in the novel plans of treatment of uterine displacements, and of other simple and innocent uterine affections, the occasional occurrence of death seems with many to cause no question of their propriety, or even of the circumstances necessary to prevent in future like painful results. Similar remarks might be made as to the importance attached to injurious results,

short of death, on the one hand in the treatment of a very serious disease, namely, urethral stricture, and on the other, in the treatment of many uterine affections which are not dangerous.

Before concluding, I merely remark, that M. Dubois has attracted professional attention strongly to the opinion which he has expressed, that although these intra-uterine instruments do not cure displacement, they may, under certain circumstances, come to be of some use, either by modifying neuralgic conditions of the uterine cervix, as the catheter sometimes does in the urethra, or by inducing an acute inflammatory state of parts previously only engorged, with the hope that the subsidence of the acute inflammatory condition may cause the disappearance also of the engorgement, which is so often the real cause of symptoms attributed to displacement.

In another place (see *Edinburgh Medical and Surgical Journal* for July 1854), I have expressed my own views as to the nature and treatment of uterine displacements. It yet remains for obstetricians to decide, whether or not intra-uterine instruments can be used with proper regard to the patient's safety, and if so, what are the conditions under which they are to be recommended.

ARTICLE IV.—*On the Weight and Dimensions of the Heart in Health and Disease.* By THOMAS B. PEACOCK, M.D., Assistant Physician to St Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest.

(*Continued from* p. 214.)

Note on the Weight of the Heart in Phthisis.

THE inference drawn in the former portion of the paper, as to the relative weight of the heart in phthisis and in other diseases, differs so considerably from that arrived at by Dr Clendinning, that I have since carefully compared the two series of observations with the view of ascertaining the cause of the discrepancy. After re-calculating the data collected by Dr Clendinning, separating from them all the weights which exceeded the limit of health, and classifying the remainder according to the acute or chronic character of the disease occasioning death, the results are as follows :—

			oz.	dr.
Males.	Weight of the heart in cases of phthisis		9	9
,,	,,	,, of acute disease only	9	1
,,	,,	,, of chronic disease only	8	0
Females.	Weight of the heart in cases of phthisis		7	13
,,	,,	,, of acute diseases only	8	8
,,	,,	,, of chronic diseases only	7	8

It will thus be seen that it is only in reference to males that the observations collected by Dr Clendinning, bear out the inference which he has drawn from them, that in phthisis the heart is heavier

than in other diseases; while n females, the results are similar to those deduced from the larger series of data contained in the paper, and show that the weight of the heart in phthisis, is ordinarily less than in acute diseases, but greater than in other chronic diseases, unconnected with obstruction in the lungs. The explanation given in the former part of the paper is doubtless the correct one, that in phthisis there is a tendency to enlargement of the heart, which, though counteracted to a greater or less extent by the emaciation, prevents the organ declining so much in weight as would have been the case were no source of obstruction present. This inference, though different both from the conclusions of MM. Louis and Bizot and Dr Clendinning, is quite compatible with the correctness of their observations, and the different results arrived at by them and by myself, is doubtless due to their having compared the condition of the heart in phthisis with its state in *all* other diseases, instead of with its weight and size in *chronic* diseases *only*. There is certainly nothing in phthisis which prevents the heart becoming considerably hypertrophied, as is shown by two cases included in Dr Clendinning's paper in which it weighed 13 oz. 4 dr. and 13 oz. 8 dr. in males; and by others in my own in which it weighed 15 oz. 8 dr. in a male, and 15 oz. in a female.

TABLE V.

Weights of the heart in males and females under 20 years of age.

Age.	Males. No.	Mean Weight. oz.	dr.	Heaviest. oz.	dr.	Lightest. oz.	dr.	Age.	Females. No.	Mean Weight. oz.	dr.	Heavieet. oz.	dr.	Lightest. oz.	dr.
6 months	1	0	13½					7 days	1	0	13				
10 months	1	1	10					2½ months	1	1	3				
2 and 2½ yrs.	2			2	12	2	10	3 months	1	1	0				
3 years	1	2	15					1 year	1	2	0				
4 and 4½ yrs.	2			4	4	2	9	1¾ years	1	1	8				
3 years	1	3	0					3 years	1	1	10				
8 „	1	6	4					5 „	1	2	14				
10 „	1	4	4					6 „	1	2	12				
11 „	5	5	10⅘	8	8	4	0	7 „	2			3	0	2	3
14 „	2			9	0	6	8	8 „	2			5	1	3	0
15 „	2			8	0	6	0	11 „	1	3	14				
16 „	3	6	5⅓	7	0	5	8	13 „	1	6	2				
17 „	1	8	8					17 „	2			7	0	4	8
19 „	1	11	0					18 „	3	9	5	10	8	7	15
								19 „	2			8	0	7	12
	24								21						

Of the observations included in this table, all but 11 were cases

of acute disease, and the results would not have been materially different had the cases of chronic disease been excluded from the calculation. The difference in the weights of the heart at different ages, in young persons, is chiefly due to the relative vigour of the children and the greater or less rapidity with which their growth proceeds; and in this respect the rate of growth of the heart corresponds with that of the brain, and indeed of the body generally.

Part II.—*Weight of Diseased Hearts.*

Table VI.

Weight of the heart in cases in which it exceeded the limit of health, but in which there was no obvious source of obstruction; and in cases of old adhesions of pericardium, and disease of the aorta (including atheromatous deposit, dilatation, and aneurism), unconnected with valvular defect.

Weights.	Hypertrophy without Obstruction.		Adhesions of the Pericardium.		Aortic Disease.			
	Males.	Age.	Males.	Age.	Males.	Age.	Females.	Age.
8 oz. 8 dr.							1	29*
10 „ 0 „					1	46*		
11 „ to 12 oz.			1	50	2	38* and 60		
		Mean.						
12 „ to 13 „	5	37·2			2	57 and 65	1	30
13 „ to 14 „	5	50					1	60
14 „ to 15 „	4	46·7			1	29		
15 „ to 16 „	1	34	1	40	1	45		
16 „ to 20 „	2	45 and 34	2	44 and 32	4	38 to 78 Mean 56·4.	2	60 and 66
20 „ to 24 „					5	30* to 71 Mean 52·2.		
40 „ 12 dr.	1	65						

In 11 out of the 18 cases of hypertrophy included in the table it is stated in the reports that there was no valvular disease, and in the others any slight thickening, opacity or atheroma which existed was inadequate to explain the increase of weight. In all the cases, also, there was an absence of material chronic disease of the lungs and aorta.

In two females the hearts weighed 11 oz. and 12 oz. in persons of 17 and 36 years of age: in the former, though there was slight thickening of the mitral valves, it was inadequate to explain the unusual weight of the heart for a person of the age, and in the second there was no valvular disease.

From the table all the cases in which there was recent pericarditis

are excluded, and one, in which the pericardium was universally adherent by old attachments, but in which there was chronic bronchitis, is also omitted. In the case of the male, 32 years of age, in whom the heart weighed 18 oz., there was not only adhesion but extensive ossification of the pericardium. The cases marked with an asterisk were aneurisms. In two of these in which the heart weighed 8 oz. 8 dr. and 24 oz., the patients were phthisical, and this was also the case in the female in whom the heart weighed 12 oz. 8 dr., and the aorta was dilated.

TABLE VII.

Weight of the heart in cases of aortic valvular disease, whether obstructive, regurgitant, or both.

Weights.	Males.			Females.		
	Obstruct.	Obst. and Regurgit.	Ages.	Obstruct.	Obst. and Regurgit.	Ages.
			Years.			Years.
8 oz. and 12 dr.					1	66
10 „ and 12 „				1		75
11 oz. to 12 oz.	1	1	40 and 57			
12 „ to 13 „				1		70
14 „ to 15 „	2	2	44.7			
15 „ to 16 „	3	3	45.5		1	46
16 „ to 20 „	1	3	47.7	1		67
20 „ to 25 „	1	8	44.5		1	44
28 „		1	18			
34 „		1	55			

In several of these cases there was also dilatation or atheromatous disease of the aorta, and, generally, some thickening of the mitral valve.

TABLE VIII.

Weight of the heart in cases of mitral valvular disease.

Weights.	Males.	Age.	Females.	Age.
		Years.		Years.
8 oz. and 8 dr.			1	27
10 „ and 8 „			1	23
11 „			1	17
12 „ and 12 oz. 8 dr.			2	62 and 40
14 „ and 14 „ 8 „	2	29 and 52		
15 „ and 12 dr.			1	39
17 „ and 8 „	1	36		
18 „			1	21

Of the cases included in this table the male in whom the heart weighed 14 oz., and the two females in whom it weighed 8 oz. 8 dr. and 11 oz. died respectively of attacks of cholera, peritonitis and fever.

In one case not entered in the table, there was free regurgitation through the left auriculo-ventricular aperture from dilatation, without disease of the valves; and the organ weighed 10 oz. 8 dr.; the subject being a girl 8 years of age. In several cases the tricuspid valves were also slightly thickened.

Table IX.

Weight of the heart in cases of combined aortic and mitral valvular disease.

Weights.		Males.	Age.	Fem.	Age.
oz.	dr.				
7	8			1	11
12	0			1	12
13	8			1	18
14	8	1	24		
17	0			1	40
18	4	1	65		
21	8	1	51		
22	0			2	39 and 63
23	0			1	53

In the three cases in which the organ attained the greatest weight, the predominant valvular disease was mitral. In that in which the heart weighed 21 oz. 8 dr., the pericardium was also universally adherent, and there was considerable thickening of the tricuspid valves, and disease of the aorta; and in some of the other cases the tricuspid valves were also slightly thickened.

Inferences.

1. The weight of the heart may very greatly exceed the limit of health without the existence of any material valvular, aortic, or pulmonic disease, adhesions of the pericardium or other obvious source of obstruction.

Thus it will be seen in Table VI. that in 18 males, the weight of the heart exceeded 12 oz., and in 5 cases it amounted to 15 oz., 16 oz., 17 oz. 8 dr., 20 oz. and 40 oz. 12 dr., the last being the heaviest heart weighed. In all these cases there existed no source of obstruction, either in the heart itself or in the aorta or lungs, which appeared sufficient to explain the great increase of weight, and in 11 of them it is expressly stated in the reports, that there was no valvular disease. In the case in which the heart weighed 40 oz.

and 12 dr., though there was slight atheromatous change in the aortic and mitral valves and in the coats of the aorta, the lungs were healthy, and nothing sufficient to explain the hypertrophy was detected.

The process by which the heart attains this great increase of weight in cases where there is no marked obstruction to the circulation, is most probably a slow one, but the data given show that there is no relation between the age of the individual and the amount of hypertrophy. The heart is found to weigh very considerably above the average in persons in comparatively early life, and the more extreme degrees of hypertrophy are not more common in advanced age, than in persons at earlier periods of life.

Only two cases which could be regarded as instances of simple hypertrophy in females are included in the tables, and, in neither of these did the organ exceed 12 oz. in weight;—facts which show the infrequency of this form of disease in females, and the very slight increase of weight which the female heart attains, when there is no serious source of obstruction.

2. The tables contain only four cases in which there were old adhesions of the pericardium, without valvular, aortic, or pulmonic disease, or recent pericarditis;—and this number is too small to warrant any decided inferences. It will, however, be seen that in one instance the weight of the heart did not exceed the limit of health, being only 11 oz. 12 dr., while, in the other three cases, it was very considerably greater, or 16 oz., 17 oz. 4 dr., and 18 oz. These facts do not confirm the opinion that adhesions of the pericardium, so far from leading to enlargement of the heart, rather tend to produce atrophy; but I have examined other hearts in which the pericardium was entirely adherent by old cellular attachments, without the organ being larger than natural. The person in whom the heart weighed 18 oz. was a male, 32 years of age, and, in this instance not only was the pericardium much thickened and adherent, but there was also a considerable formation of bone.

3. The heart most generally acquires very great increase of weight in cases of disease of the aorta, whether consisting in atheromatous deposit and ossification in the coats, dilatation, or sacculated aneurism; but in none of the cases of this description did the heart attain so great a weight as in one of those in which there existed no obvious source of obstruction to explain the enlargement.

The last column in Table VI. shows that in one female the weight of the heart did not attain the average, being only 8 oz. 8 dr. In one male also it only slightly exceeded the average, being 10 oz.; and in two other males it did not exceed the limit of health, weighing in each 11 oz. 8 dr. In the remaining cases (seventeen in number—thirteen males and four females), it very considerably passed that point, weighing from 12 oz. to 24 oz. in males, and from 12 oz. 8 dr. to 18 oz. in females; the greatest weights being, in males, 16 oz. 4 dr., 18 oz. 4 dr., 19 oz., 21 oz., 23 oz., 23 oz. 8 dr., and 24 oz.; and in females, 17 oz. 8 dr., and 18 oz. It must, how-

ever, be observed, that in the male in which the heart weighed only 10 oz., in a second in which it weighed 24 oz., and in the female in whom it weighed 8 oz. 8 dr. in addition to the aneurisms, the lungs were tuberculous; and in the female in whom the heart weighed 12 oz. 8 dr., the aorta was dilated and the lungs tuberculous, so that, in these instances, the hypertrophy of the heart resulting from the aortic obstruction, may have been to some extent counteracted by the emaciation from phthisis.

As in the cases of hypertrophy only, so also in this form of disease, there is no just relation between the ages of the individuals and the increase in the weight of the heart.

4. The heart usually becomes very greatly enlarged in cases of aortic valvular disease, whether obstructive or regurgitant, or both; and the increase of weight is apparently greater in these cases than in those of disease of the aorta; but in no instance did the organ acquire from this cause so great a weight as in one of the cases of hypertrophy unconnected with obvious obstruction.

These inferences are deduced from Table VII., from which it appears that in males the weight of the heart exceeded the average in all the cases; but, in two instances, in one of which there was obstruction only, in the other regurgitation, it did not pass the extreme limit of health, weighing only 11 oz. 8 dr., and 12 oz. In seven other cases of obstruction its weight ranged from 14 to 21 oz., the greatest weights being 16 oz., 16 oz., 19 oz., and 21 oz.; and in eighteen cases of regurgitation, from 14 oz. to 34 oz.; the greatest weights being 20 oz., 21 oz., 22 oz. 4 dr., in three cases 23 oz., 23 oz. 8 dr., in two cases 24 oz., 28 oz., and 34 oz.

In females, the increase of weight in cases of aortic valvular disease also obtains though to a less degree. If a case in which one of the segments was destroyed by acute endocarditis, and in which death rapidly ensued, and the organ weighed only 8 oz. 12 dr., be excluded, the lightest heart in the table weighed 10 oz. 12 dr., or within the limit of health; yet, in this instance, there was most extensive thickening and ossification of the valves, probably originating in malformation, and causing extreme obstruction; and as the subject of the case was 75 years of age, the progress of the disease must have been very slow. In two other cases of obstructive disease the heart weighed 13 oz. and 18 oz. 8 dr., and in two cases of regurgitation 16 oz. and 23 oz.

It will thus be seen that, though the weight of the heart in cases of aortic valvular disease was greater than in the cases of disease of the aorta, the heaviest heart in the former weighed nearly 7 oz. less than in the remarkable case of hypertrophy without valvular or aortic disease before mentioned.

The table also shows that the weight of the heart in cases of aortic valvular disease is by no means proportionate to the age of the subjects; the organ having attained a weight of 28 oz. in a boy of 18, and 23 oz. in a female of 44.

It is not possible to ascertain the respective effects of obstructive and regurgitant disease in increasing the weight of the heart, for the two generally coexist, and the latter is very frequently only the final stage of the former; and the influence which is due to each cause cannot therefore be assigned. In the case, however, in which the heart weighed 17¾ oz. in a male 33 years of age, the disease was the result of rupture of the angle of attachment of two of the valves, from violent muscular exertion, 27 months before death, and though there must have been some obstruction to the flow of blood from the ventricle into the aorta, the great increase of weight must have been chiefly due to the incompetency of the valves. A still more remarkable instance of enlargement from regurgitation, is afforded by a case of rupture of the valves, related by Dr Quain, in which the heart acquired the weight of 22½ oz. in a period of two years which elapsed between the occurrence of the accident and the death of the individual. In both these cases the heart was most probably healthy till the accidents occurred.

In all cases of regurgitant disease, on whatever cause dependant, the increase of weight probably takes place rapidly; thus, in the boy of 18, in whom the organ weighed 28 oz., the duration of illness had only been two years and ten months; on the contrary, in cases of obstructive disease, the enlargement of the heart is probably a slower process. A striking example of the length of time during which obstruction may exist without the heart acquiring great increase of weight, is afforded by the case of the elderly female, before referred to, in whom the organ weighed 10 oz. 12 dr., but in this instance, it seems probable, that a decrease of weight may have occurred with the advance of life. The case, however, with many others which might be quoted, shows the absence of any just relation between the degree of obstruction and the amount of hypertrophy.

5. In cases of mitral valvular disease the heart ordinarily exceeds the limit of health, but does not attain so great an increase of weight as in cases of aortic or aortic valvular disease.

Of the cases included in Table VIII., three, that of the male in whom the heart weighed 14 oz., and two females, in whom it weighed 8 oz. 8 dr., and 11 oz., death did not result from the direct effect of the disease, but from other causes. If, therefore, these cases be excepted, it will be seen that in one female the heart weighed only 10 oz. 10 dr., or within the limit of health, while in all the other cases it greatly exceeded that point, weighing, in males, from 14 oz. 8 dr. to 17 oz. 8 dr., and in females from 12 oz. to 18 oz. These weights are, however, much less than those in the cases of aortic and aortic valvular disease, and still less than that of the heart in which the hypertrophy was not dependent on obvious obstruction. It will also be observed, that in these cases there is not the great difference in weight between the male and female heart which is observed in other forms of disease, but the cases are so few, that the inferences cannot be fully depended upon.

The weight of the heart in cases of mitral valvular disease bears no certain proportion to the age of the individual, as is shown by the subject of the case in which the heart weighed 18 oz. being a female only 21 years of age.

6. In diseases of the mitral and aortic valves combined, the weight of the heart is generally intermediate between that in cases of aortic and mitral disease alone; the organ being lighter than in aortic valvular disease, and heavier than in mitral valvular disease.

It will be seen from Table IX., that if the case in which the heart weighed 7 oz. 8 dr., in a girl 11 years of age, be omitted, in all the observations, including two females of 12 and 18 years of age, the weights of the heart exceeded the extreme limit of health; and that in three males it weighed from 14 oz. 8 dr. to 21 oz. 8 dr., and in three females from 17 oz. to 23 oz.

7. The tables contain only one observation of the weight of the heart in extensive disease of the mitral and tricuspid valves. In this instance the disease was probably congenital. The organ weighed 9 oz., and the subject was 37 years of age, but she was peculiarly ill-formed and stunted, and did not appear more than 16 or 18 years old.

8. In the tables the weights of four hearts are given in which there were congenital malformations. Of these cases that which presented the least important deviation from the natural structure, was one in which the foramen ovale was unclosed, in a girl $8\frac{1}{2}$ years of age, and the heart weighed 6 oz. 8 dr. A second case was that of a girl aged 5 years, who died of hemorrhage from the throat or stomach, during an attack of scarlet fever, and the only malformation was the existence of a septum dividing the infundibular portion of the right ventricle from the sinus, and the heart weighed 3 oz. 12 dr. In a third case, that of a boy 15 years of age, in addition to a similar source of obstruction in the right ventricle, the pulmonary artery was small and its valves malformed, and there was an aperture in the septum ventriculorum, by which the aorta freely communicated with the right, as well as with the left ventricle; in this instance the heart weighed 10 oz. In three other cases (two of which are not included in the tables), the pulmonary orifice was the seat of obstruction. In one of these, a young man, 20 years of age, it was very greatly contracted from the adhesion and thickening of the valves, and the foramen ovale was largely open, and the heart weighed 12 oz. In another, a female, 19 years of age, the pulmonary orifice was greatly contracted from malformation of the valves, the tricuspid valves were also diseased; the aorta arose from both ventricles, and the ductus arteriorus was still pervious; and the heart weighed $17\frac{1}{2}$ oz. The subject of the third case was an infant nearly 12 months old, in which the pulmonary artery was entirely obliterated, the aorta arose from both ventricles, and the blood was transmitted to the lungs through the ductus arteriosus. The heart in this case weighed 3 oz. 8 dr. The last instance of mal-

formation is one in which, in a female infant aged 5 months, there were two small apertures in the septum ventriculorum, with thickening of the tricuspid valves, causing contraction of the orifice, and the heart weighed 2 oz. 4 dr. A comparison of the weights of the heart in these cases with that of the healthy organ in infancy and early life, as given in Table V., will show how greatly it exceeded the ordinary weight in healthy children at similar ages; indeed, in the female 19 years of age, it very considerably exceeded the extreme limit of health in adults.

The weights of the diseased hearts in the above tables are, so far as I am aware, the most extensive series yet published; indeed with the exception of M. Bouillaud, I do not know any writer who has specially collected the weights of diseased organs. M. Bouillaud's observations are, however, very few in number, amounting to only 11 cases, in which the heart was hypertrophied, and 7 which he regarded as cases of atrophy. Of the latter, in five males, the weights ranged from 5 oz. 6 dr. in a person 18 years of age, to 7 oz. in one of 20, and the causes of death were marasmus, typhoid, and the effects of having taken nitric acid. The two females were aged 45 and 30, and died respectively of schirrus of the pylorus, and of disease of the liver, and the hearts weighed 4 oz. 5 dr., and 5 oz. 13 dr. Of the observations in the paper, the lightest male heart weighed 5 oz. in a person 53 years of age, who died of cirrhosis hepatis, combined with disease of the kidneys. In a second case the heart weighed 6 oz. in a person 39 years of age, who died of cancer of the pylorus, and in a third, the heart had the same weight in a man 29 years old, the cause of whose death is not recorded. The lightest female hearts weighed 5 oz. 8 dr. in two persons 25 and 35 years of age, and 5 oz. 12 dr. in a third, 21 years old, and all these died of phthisis.

The weight of the heart in the cases of hypertrophy collected by M. Bouillaud, are as follows:—

Hypertrophy apparently without Valvular disease.

1. Male, 60 years of age, . . 12 oz. 15 dr. avoirdupois.
2. „ 50 „ . . . 13 „ 11 „ „
3. Female, 75 „ . . . 14 „ 10 „ „
4. Male, 67 „ . . . 18 „ 3 „ „
5. „ 69 „ . . . 17 „ 10 „ „

Aortic Valvular obstruction and probably Regurgitation.

6. Female, 54 years of age, . . 24 oz. 4 dr. avoirdupois.

Mitral Valvular Regurgitation.

7. Male, 47 years of age, . . . 22 oz. 3 dr. avoirdupois.

Mitral Valvular obstruction and Regurgitation.

8. Female, 56 years of age, . . 12 oz. 15 dr. avoirdupois.
9. Male, 53 „ . . . 16 „ 3 „ „

Combined Mitral and Aortic Valvular disease.

10. Female, 17 to 18 years, . . 11 oz. 14 dr. avoirdupois.
11. Male, 40 years of age, . . 18 „ 10 „ „

In the tables in the paper the weight of only one heart is given in which there was regurgitation through the left auriculo-ventricular aperture, without disease of the valves, and in this case the patient was a girl only 8 years of age;—so that no comparison can be instituted between the weight of the heart in M. Bouillaud's 7th observation and my own; but, with this exception, it will be seen that the weights of the heart, in the different forms of disease in the comparatively small number of observations published by M. Bouillaud, correspond with those deduced from my own much more numerous data; though in none of his observations did the heart attain so great a weight as in several of my own.

In M. Bouillaud's treatise the weights are given in grammes, and also in the pound, ounce, and gros. The pound being the poids de marc of 7560·6 grains troy, and the ounce and gros respectively the medicinal weight of 472·5 and 59·1 grains. The weights given above, are avoirdupois, calculated from the gramme, as equal to 15·434 troy grains.

In Dr Clendinning's tables the lightest male heart weighed 6 oz., in three cases, in persons who died of ascites, mania, and phthisis, at the ages of 24, 41, and 43; and the lightest female hearts weighed 4 oz. 8 dr., 5 oz., 5 oz. 8 dr., and 5 oz. 4 dr., in persons aged respectively 32, 31, 20, and 26, who died of epilepsy with ulceration of the intestines, chronic bronchitis, typhus, and phthisis.

The heaviest hearts in males weighed 24 oz., 24 oz. 8 dr., 26 oz.[1] and 40 oz. 8 dr., in persons 48, 34, 33, and 33 years of age; but, except in reference to the last case, in which there was aneurism of the aorta near the heart, the nature of the disease causing death is not reported. It is curious that the heart in the last case should be precisely the same weight as the heaviest which I have weighed. In females the greatest weights given by Dr Clendinning are $15\frac{1}{2}$ oz. and $17\frac{1}{2}$ oz. in persons 23 and 50 years of age; but though the foramen ovale is stated to have been open in the former case, the precise kind of disease is not mentioned in either.

(*To be continued.*)

ARTICLE V.—*Cases of Congenital Malformation of different portions of the Digestive Apparatus.* By KELBURNE KING, M.D., Edin., Hull.

CASE I.—*Impaired nutrition and death from congenital malformation of the inferior maxilla.*

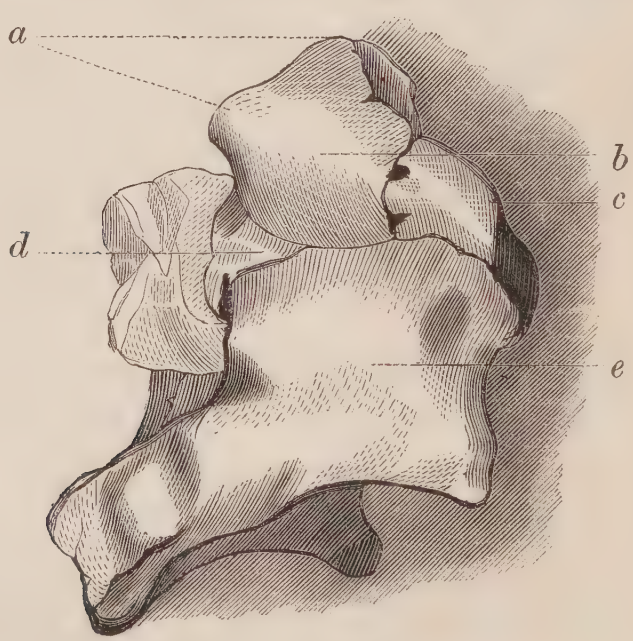
In the month of January last, I was consulted regarding a female child then three weeks old, who from birth had exhibited defective power of suckling and swallowing. It seemed eager enough, but was unable to perform efficiently the act of suction, and obtained nourishment only by means of a

[1] This case is evidently twice inserted in the Tables.

spoon used so as to push down the tongue, and introduce food into the posterior part of the mouth. On looking into the mouth (which was constantly open, owing, I supposed, to the child's crying) I perceived that the whole of the right side of the tongue was occupied by a tumour fully as large as a bean, which from its appearance I judged to be of a vascular nature, and, supposing that it might be the cause of the difficulty, I injected into its substance a few drops of perchloride of iron.

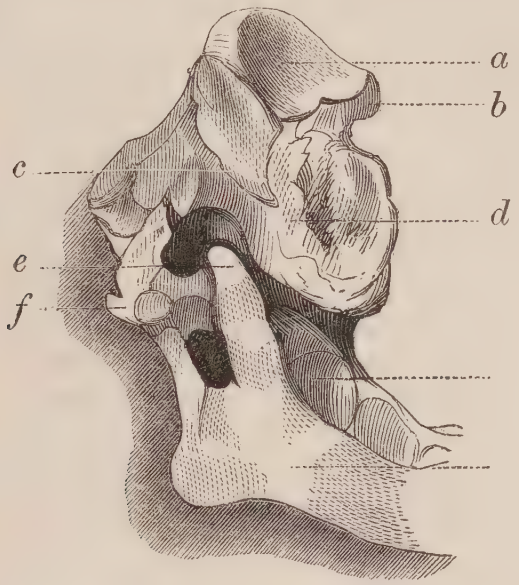
About a fortnight after I again saw the child; the tumour was greatly diminished, the parents thought its power of swallowing something improved; but it was unable to suck or rather to swallow what it did suck, and was still supported only by food administered in the manner described above. It was much emaciated, cried continually while I saw it, and was evidently suffering from deficient nutrition. I recommended the use of cod-liver oil, but heard from time to time that no improvement took place. It became gradually more and more wasted and debilitated, and finally died on the 21st May 1854.

On the 22d, I made a post-mortem examination, and desiring to remove the tongue and appendages, commenced by making an incision from one articulation of the inferior maxilla to the other passing round the front of the throat. In making this incision my attention was attracted by the very rudimentary state of the masseter muscle, the fibres of which were hardly discernible. I then observed that the mouth was in the half-opened state assumed during the act of crying, and on endeavouring to close it, found that it was firmly fixed in that position. On closer examination, it appeared that complete anchylosis existed between the inferior maxilla and the temporal bones—that the position of the masseter muscle—which was absent or nearly so—was occupied by a strong osseous plate descending from the inferior margin of the zygomatic process and malar bone. This abnormal osseous deposit extended as far forward as the anterior projection of the malar bone, incorporated itself in its passage downwards with the tuberosity of the superior maxilla, and was finally lost in a complete amalgamation with the angle, base, and ramus of the inferior maxilla. It consequently extended much further forward than the usual outline of the bone, so as to overlap the cavity of the second bicuspid tooth, and presented the appearance of an excessive development of the ascending ramus. Looking at its outer aspect,



The external aspect of part removed; *a*, orbital plate of malar bone; *b*, malar bone; *c*, zygomatic process; *d*, superior maxilla; *e*, enlarged ascending ramus of inferior maxilla.

the posterior edge of the ramus had its usual form, direction, and position; but the anterior part was prolonged so much further forward than usual as to give the ramus generally the appearance of twice its ordinary breadth, whilst superiorly in place of the condyles and coronoid process it terminated in a thick osseous plate, which extended over the whole length of the inferior edge of the zygomatic process and malar bone, and was likewise connected with the posterior part of the superior maxilla. These bones were all firmly connected by osseous union; a superficial serrated line marked the suture of the zygomatic processes of the malar and temporal bones, while a straight line marked the junction of these and of the superior maxilla with the abnormal plate. This last sprung without any line of separation from the ramus, showing its course of development to have been from the latter towards the former. Judging from the external surface, one would have supposed that the appearance described had been the original formation, and that no condyles or coronoid processes had ever existed. But on looking at the internal aspect this proved not to be the



Internal aspect of part removed; *a*, orbital plate of malar bone; *b*, malar bone; *c* and *d*, abnormal deposit; *e*, coronoid process; *f*, condyle of inferior maxilla; *g*, fovea for second bicuspid tooth.

case, for there these processes were seen of pretty much their natural size and shape, but surrounded and enveloped in the abnormal deposit.

The same description applies to both right and left sides of the bone. I was permitted to remove as much of the parts concerned as I could without interfering with outward appearances, and perhaps the accompanying sketches of the external and internal aspects of the part I contrived to remove, will explain better than words the nature of this remarkable deviation from the usual formation.

The history of this case shows that this malformation was congenital. It seemed more like an ossification of the masseter muscle than anything else. As to the peculiar circumstances which gave rise to it I have no means of forming even a surmise. I do not believe that it was the original formation of the bone. The appearance of well-formed condyles and coronoid processes would lead rather to the supposition that it was superinduced at a somewhat advanced period of pregnancy. The effect of it was to change the mouth from a moveable to a fixed cavity, leaving a certain fixed

space in front as an opening. The jaws were placed in a half-opened position, and the difficulties the child had to contend with, and which proved fatal to it, arose from the want of power to approximate the maxillæ, and perform the first steps in the great process of digestion. But it was not the act of suction which was most impeded by this condition of the parts. For if a hard substance was introduced between the teeth, fixing the jaws at a certain distance from each other, the lips could still be brought together so as to permit that process to go on. Neither was the final achievement of deglutition defective, for when the tongue was pressed down by a spoon, and a fluid poured into the back part of the mouth, the child could swallow, and in this way its life was prolonged for four months. The deficiency lay in that part of the process intermediate between the reception and the final swallowing of the food—that by which it is passed over the arch of the tongue. In consequence of the distance between the floor and the roof of the mouth, the tongue could not be brought up to the anterior palatine arch. The food, consequently, could not be forced from the anterior part of the mouth where it was received to the posterior where it might be swallowed—mastication, of course could never have been performed—but independently of that, the incapacity of executing the apparently trivial movement referred to, was sufficient to cause wasting and death, showing how necessary is each link of the chain in the process of digestion to the permanent prolongation of life. I may mention that the *post-mortem* examination proved the efficacy of the injection of the perchloride of iron. The vascular tumour had completely disappeared, a small fibrinous mass about the size of a split pea being all that was found in its place.

CASE II.—*Death from congenital malformation of the large intestines.*

On the 29th May 1852, Mrs —— was delivered, after a labour which lasted six or seven hours, of a full-grown well developed female child, still born. Decomposition had commenced at the umbilicus, showing that death had taken place before the commencement of labour. As the infant lay on the napkin a little meconium oozed away from the anus—a circumstance which was observed by the mother, and added greatly to the grief which was the natural result of her disappointment.

It afterwards came out that she had lost two other female children shortly after birth in consequence of what she supposed to have been imperforate anus. The sight of meconium led her to think that this child had escaped from the malformation which had proved fatal to the others, and, as she was very anxious to have a girl, increased the grief she felt for its loss. Her family consisted then of five children—the eldest a girl—and four boys ; so that after her first she had had four boys, all healthy, and two girls who had died besides the one I have alluded to—still-born. She had a strong desire to learn the cause of the fatal event in this instance, and on the following day I examined the body.

On laying open the abdomen, an enormous viscus, which at first I supposed to be a greatly enlarged stomach, stretched across, and almost filled the whole cavity. Closer inspection, however, showed the stomach of its ordinary form and size, occupying its usual position. The duodenum also presented no peculiar appearance, but terminated in the large pouch I have mentioned, which occupied the whole extent of the abdomen from the liver to the pelvis. Al-

though it represented the jejunum and ileum, it was only about eight inches in length. It contained a considerable quantity of meconium, while the rest of the intestines were almost empty. It terminated at the usual spot in the large intestines, or rather in what ought to have been the large intestines. For the cœcum, colon, and rectum, though found in their ordinary situation, and presenting on a reduced scale their usual appearances, were no bigger than a goose quill, as far as they were covered with peritonæum. The lower part of the rectum and the anus were of the usual size, readily admitting the point of the finger.

Such malformations, or at least similar ones, are not uncommon, but when I exhibited to the father of this child a sketch I made of the disposition of the viscera, he drew out from an old pocket-book a precisely similar sketch, given him by the surgeon who had examined the bodies of the other two children—with this exception, that in them there had been also imperforate anus. It does seem singular, that after her first confinement, this lady should have had four boys perfectly healthy, and three girls presenting this peculiar deformity.

The former cases were beyond the reach of art; the one to which I now refer might have been saved by more timely interference.

Case III.—*Congenital Malformation of the Rectum; operation. Death from paralysis of the intestinal walls.*

On the 24th March 1853, I was consulted regarding a male child which, though born on the 18th, had not had any evacuation of the contents of the bowels. Its mother had been attended by one of the midwives of the Hull Lying-in Charity, who, on the circumstance being reported two days after birth, examined the anus, pronounced it "all right," and administered a teaspoonful of castor-oil. This was repeated six times without effect, when the midwife declared that nothing more could be done. Thus discouraged, the poor woman applied to a druggist, who administered various medicaments, but in vain. Soap suppositories were then tried, but it was found that they could not be completely introduced. Six days having been thus wasted, the child was brought to me in a truly pitiable condition. For two days it had refused all nourishment, and from a fat plump child had become extremely thin and shrivelled. Its abdomen was swollen, hard and tympanitic, presenting a remarkable contrast to the rest of its shrunken body. It kept up a continual low weak whine, which was most painful to listen to.

On examination the anus was found fully developed, and endowed with a sphincter, but rather less than an inch higher up, the bowel terminated in a *cul de sac*. I thought I felt an impulse communicated to my finger when the child cried, and supposing the septum to be thin, recommended its immediate division. This was not agreed to, and I saw no more of the child till the 26th, when it was again brought to me. I then carefully punctured the septum and dilated the opening. The escape of a quantity of flatus showed that the intestinal canal had been penetrated, but to my disappointment no fœculent matter was discharged. An elastic tube was then passed up a considerable distance, the sides of which, when withdrawn, were tinged with meconium, but though the septum was evidently completely divided, no discharge could be procured even by injecting a quantity of warm water. I recommended that a little castor oil should be given, and in the evening was gratified to find that there had been a free evacuation of the bowels. This was repeated twice on the following day (the 27th)—the child partook of some food, but an abscess which had formed on the neck some days previously, became excessively large and troublesome. On the 28th the child cried much, refused all nourishment,

and died on the 29th, the twelfth day after its birth, and fourth after the operation.

On the day following I made a *post-mortem* examination of the body, and found the whole tract of the intestinal canal, to the termination of the sigmoid flexure perfectly healthy and normal. From that point to the septum, the remains of which were still perceptible, the rectum was enormously distended, forming a pouch at least six times more capacious than the stomach. The opening through the septum was quite pervious, and it was evident that the difficulty of procuring an evacuation had been caused by paralysis of the muscular coats produced by long-continued distension.

I was permitted to remove the contents of the pelvis for more minute investigation, directed more especially to ascertain the nature and connections of the structure which formed the septum. This I found to consist of a strong fibrous membrane, into the upper and under surfaces of which were inserted the longitudinal muscular fibres of the two segments of the rectum, but which was otherwise only loosely connected with the bowel, the two *culs de sac* of which could be readily dissected away from it. It was attached to the prostate in front and to the sacrum behind, and was, moreover, intimately connected with the pelvic fascia, of which, indeed, it proved to be a prolongation. This fascia being dissected out, was found dividing as usual at the upper edge of the prostate, one layer passing downwards, investing that gland and the neck of the bladder, the other passing upwards and forming the vesical fascia. From the point of this division there passed directly backwards another layer, which, in place of being reflected on the sides of the rectum and enclosing it, cut right through it (so to speak), dividing it into two segments, and then went to be inserted into the anterior aspect of the sacrum.

It is not uncommon to find a septum occurring at this point, and so forming an obstruction, identical in a practical point of view, with imperforate anus, by which name it is usually, though erroneously, described. But I have not noticed any accounts of dissections showing its nature and origin, and have thought it worth while to put this case on record. I had, in this instance, an opportunity shortly afterwards of demonstrating to the Hull Medical Society, the connection of the septum with the pelvic fascia from the parts which I removed, and which I still preserve.

The length of time this little patient survived is rather remarkable. It was born on the 18th March, and obtained no evacuation till the evening of the 26th. Yet other cases have occurred in which a cure has been accomplished after even a longer delay. In Langenbeck's New Surgical Bibliotheca, a case is detailed in which the imperforate state of the anus was not discovered till the twelfth day, but which was cured by operation. I believe that this little patient would probably have recovered, had not its strength been exhausted by the formation of a large abscess in the neck.

The immense distension of the rectum explained the difficulty which occurred in obtaining a discharge even after the division of the septum. The muscular fibres had been paralysed by long overstretching, but the evacuations which took place spontaneously on the 26th and 27th proved that that difficulty might have been overcome in time.

These cases are all examples of congenital malformations of different parts of the digestive apparatus terminating fatally. The

first I have been induced to record from its singularity. The second, less remarkable as a single case, derives an interest from the fact of its being thrice repeated in the female children of the same mother, whilst her boys were exempt. The third I have thought worthy of notice from the length of time the infant survived without obtaining any evacuation, and from the peculiar mode of formation of the septum as revealed by the dissection.

ARTICLE VI.—*Case of False-joint in the Fore-arm, resulting from a Gun-shot Wound.* By Dr BEVERIDGE, Surgeon of H.M. Ship Samson.

THE following case, communicated to us by Dr Beveridge, Surgeon of H.M. Ship, Samson, was forwarded through Sir George Ballingall, who remarks that it is confirmatory of the aphorisms laid down by Mr Guthrie relative to gun-shot wounds of the forearm :—

On the 17th July this vessel anchored off Bardan on the coast of Circassia, at the distance of a thousand yards from the beach, on which a heavy surf broke. Shortly afterwards an object in the water approaching the ship attracted attention. From there being a heavy swell the object alternately appeared and disappeared ; but ultimately proved to be a Circassian, swimming off with *one* hand. He reached the ladder, and, unassisted, came on board, attired simply in a pair of trousers, and smiling, asked permission to see the ship, which was immediately granted. He was a man of about 45 years of age, and on examination it appeared had been wounded by the "Muscoffs." An extensive cicatrix at the upper third of the left forearm was visible, where a passive false joint existed, the body of the ulna had been shot away, but both the upper and lower extremities entering into the formation of the elbow and wrist joints remained entire, admitting in the former of active as well as passive motion, in the wrist joint only passive motion. The radius had been fractured transversely at the upper third, the lower fragment overlapping the other, so as to shorten the forearm a third. The distal phalanx of the thumb, and also the cartilaginous extremity of the first phalanx, (which could be passively moved on the metarcarpal bone), were absent. The four fingers were rigidly in a state of extension, and could not be even passively flexed. He was very willing to show what he could do, and of his own accord lifted a kettle by the handle on the elbow joint, which he could both flex and extend—the insertion of the biceps tendon being preserved, as also the olecranon. He also was able to approximate the fore and ring fingers to the middle so as to lift a handkerchief. Had any one presented himself with so extensive an injury, as indicated by the cicatrix, etc., before a naval or military surgeon, there is little doubt in my mind but that immediate amputation above the elbow joint would have been performed. The temperate habits of the Circassians, and their general good health, prove favourable to recovery from wounds. Many badly united fractures, from want of proper apposition of the broken surfaces, were seen.

Part Second.

REVIEWS.

1. *Sammandrag af officiella Rapporter om Cholerafarsoten i Sverge, aar* 1850, *efter uppdrag af Kongl. Maj:ts Sundhets-Collegium utarbetadt af dess ledamot,* Dr Fr. Th. Berg. Stockholm, 1851.

Summary of Official Reports on the Epidemic Cholera in Sweden, in 1850, *drawn up at the request of the Royal College of Health, by its member,* Dr Fr. Th. Berg. Stockholm, 1851. 8vo, pp. 370.

2. *Reports on Epidemic Cholera, drawn up at the desire of the Cholera Committee of the Royal College of Physicians.* By William Baly, M.D., and William W. Gull, M.D., Members of the Committee. London, 1854. 8vo, pp. 565.

3. *Notes on the Pathology and Treatment of Cholera.* By John Rose Cormack, M.D. London, 1854. 12mo, pp. 67.

4. *Letter to the President and Fellows of the Royal College of Physicians, in relation to the evidence cited in their late Report on the Treatment of Epidemic Cholera.* By Joseph Ayre, M.D. London, 1854. 8vo, pp. 71.

5. *Result of an Inquiry into the invariable existence of a Premonitory Diarrhœa in Cholera.* By David MacLoughlin, M.D. London, 1854. 8vo, pp. 61.

6. *Observations on the Nature and Treatment of the Asiatic Cholera.* By James Tucker, M.D. London, 1854. 8vo, pp. 49.

7. *Sulphur as a Remedy in Cholera and Diarrhœa.* By John Grove, M.R.C.S.L. London, 1854. 8vo, pp. 35.

8. *On the use of Vegetable and Mineral Acids in the Treatment of Cholera.* By J. H. Tucker, Surgeon, Hon. Sec. of the Epidemiological Society. London, 1854. 8vo pp. 32.

9. *Cholera: An Analysis of its Epidemic, Endemic, and Contagious Character.* By Henry Stephens, M.R.C.S.L. Lond. 1854. 8vo. pp. 31.

10. *The Common Sense of Cholera.* By a Practical Practitioner. London, 1854. 12mo, pp. 68.

11. *El Semanario Médico Español.* Madrid, Num. 14 y 16, 24 de Agosto y 7 de Setiembre 1854.

The Spanish Medical Weekly Journal. Madrid, No. 14 and 16, 24th August and 7th Septr. 1854.

It is at a time so near our going to press, that circumstances have permitted us first to direct our attention to the mass of publications

on cholera which are now lying before us, that we can entertain no reasonable hope of being able to render them that justice which the importance of the subject demands. We regret this the more, because several of these publications come before the profession with that stamp of authority which entitles them to our most respectful consideration; and there is not one of them on which we should not have wished to have bestowed a more deliberate examination, and to have delivered our judgment with candour and sincerity. As these latter valuable qualities, however, may remain to us under any circumstances, and as the subject of cholera is now one of immediate and pressing interest, we are unwilling to defer our notice to a future occasion. We may offer it with the less hesitation, because we are at least no novices in the study of cholera, with which we became first acquainted on its earliest introduction to this country; and regarding the treatment of which, in its severest type, we can claim to speak from ample and repeated opportunities of experience.

The Reports on Epidemic Cholera, which appear under the sanction of the Royal College of Physicians of London, are founded upon the answers, more than four hundred in number, received to a series of queries relating to the epidemic of 1848-9, issued by Drs Baly and Gull, in the name of a committee of the College appointed for the purpose of collecting information regarding the history of its progress in England. To form a summary of these answers, and to abstract, and, in part, tabulate the result, has been the important and laborious task of the reporters; and it is one which has many claims upon our gratitude. Other and more public resources, such as the reports of the Board of Health, and of the Registrar-General, and the returns at the General Register-Office, have also been brought into requisition, to complete the survey of their momentous topic, as it related to our home experience; while, in addition, the investigations of continental writers have not been neglected, and have been assimilated, or contrasted, with the results of other independent inquiries instituted among ourselves.

Yet, while we cheerfully own the obligation conferred upon us by a labour so severe and so humanely directed, we cannot conceal our deep regret that it should have proved so altogether barren and profitless. We cannot discern that it has succeeded in adding a single new fact of importance to our knowledge of the history and phenomena of the disease; or in eliciting any better founded doctrines as to its origin, its specific nature, and its means, if not its modes, of dissemination; or in establishing any more rational and successful method of cure; or even in assigning its just and precise value to any of the multifarious plans which, to the disgrace of our therapeutics, thrust themselves before the public with a diversity as monstrous as the pretensions of many of them are overweening and preposterous. As for ourselves, who have not yet deserted the old paths of experience, who believe there are a few rational principles in therapeutics, and who would willingly apply these to our new

emergencies as closely as the similarity of circumstances will admit, we would gladly have learned more definitely what was our actual position with reference to those less scrupulous innovators, who hug some favourite theory, the study of which, as Cabanis tells us, they find more easy than that of nature, and which seems to smoothe away before them all difficulties in practice. Assuredly we speak thus in no spirit of reproach, for we cannot refuse to admit, that trustworthy statistics of this description are of very difficult attainment. Making the liberal concession, that all are alike faithful in their statements, we have yet the certainty that the circumstances recorded are everywhere widely diversified, that the cases are arranged under different categories, that different periods of disease, and different types of severity are assumed as the basis for equivalent results, and that not only the apparent conditions are not similar, but many of them are utterly unknown. Still, we should have been glad to have seen the effort to procure them more rigidly adopted, had it been only to show to the 400 respondents to the queries, what information was specially desired, and wherein lay chiefly the proneness to fallacy, and the risk of error. We want the records of that experience which is the mother of truth, and not of that which, often unwittingly, seeks only to give a specious air to a preconception, and ignores everything which opposes it: and, to attain these, we must proceed universally upon more definite, and more nearly identical, grounds, and with greater watchfulness to exclude the sources of deception. Such statistics are requisite to establish the true value of a mode of treatment; but such statistics are as yet of exceeding rarity with reference to cholera.

The chief objects of the inquiry, in as far as the results have been communicated by Dr Baly, related to the investigation of the origin and modes of propagation of the disease: the departments of pathology and treatment have been more immediately under the supervision of Dr Gull. Six hypotheses, different, or professing to be different, are proposed as embracing the leading opinions which have been prevalent with reference to the first division of the subject: 1st, That the disease spreads by an atmospheric influence or "epidemic constitution," the particular localities affected being determined by certain localizing conditions; 2d, That its cause is to be regarded as a morbific matter, undergoing increase only within the human body, from which it is propagated by means of emanations, or, in other words, by contagion; 3d, That the contagion prevails in the specific form of a poison which is swallowed, and acts directly on the mucous membrane of the intestines; 4th, That the cause is a morbific matter or poison, reproduced only in the air, and diffused by its medium; 5th, That it is reproduced in the air, but nevertheless is distributed by means of human intercourse; and 6th, That it may have a twofold origin, being propagated in and by the air, as well as in and by the human body. We shall not cavil at the use here of the term "epidemic constitution," although we do

not think it is employed in the sense assigned to it by Sydenham, who first gave it value and currency, or in that of other esteemed writers who have followed him; and although we know nothing which is more prejudicial to the progress of medical science than the irregular and inaccurate use of its appropriate language. The epidemic constitution is never properly understood to originate a disease: it only favours its diffusion and modifies its character, so that even other diseases, concurrent with the main epidemic, are constrained to assume a similar type. It is the "aëris massæ occulta crasis, sive textura, diversarum diversis temporibus ægritudinum ansa atque pararia;" but, to create the disease, "verum præter istam aëris constitutionem, ceu caussam communiorem, accedat oportet et alia procatarctica, nimirum μιασματος sive seminii, a pestifero aliquo corpore, vel immediate ac propiore consortio, vel mediate ac per fomitem aliunde transmissi, susceptio;" while even this is held insufficient unless, farther, the individual to be affected "corpus habeat humoribus ad afflatum excipiendum paratis refertum."[1] If the reporter does not understand and apply the phrase in a different sense from this, we do not see the necessity of introducing the fourth hypothesis, which implies the existence of a poison reproduced only in the air. Indeed, as to the other hypotheses, as the second involves contagion directly, and the third diverges only by assigning to it a particular form, while the fifth virtually admits it by allowing the agency of human intercourse, and the sixth equally affirms its influence, we cannot discern why, in as far as all practical advantages are concerned, the question should not have been reduced to a simple consideration of the existence or non-existence of contagion, and so all but confined to the uncomplicated discussion of two sole propositions. Assuredly, had only this been in the first place attempted, and had the local and personal influences and conditions been viewed only in their proper light of predisposing causes, the inquiry would have made fewer intricate reversions upon grounds which it had already traversed, and the inferences arrived at would have been more direct and substantial in themselves, and more distinctly enunciated, because less qualified by reservations, if not sometimes by contradictions, depending more upon that which was accessary than upon that which was essential.

The conclusions, however, ultimately adopted through an examination of the facts, were of the following tenor:—The supposition, we are told, that "the cause of the disease is a general state of the atmosphere—a general 'atmospheric influence,' or 'epidemic constitution,'" was found untenable. This being admitted, we need not pause to inquire how "characters of *site*" and other "*localizing* conditions" should have been received as evidences of this alleged *general* influence; for we should have thought that that which was localized must, as such, have been taken out of the category of that

[1] Sydenhami Opera Universa Medica, sect. ii., c. 2.

which was general. It will be perceived, however, that we have here an example of the confusion which arises from failing to regard uniformly the local and personal influences as predisposing, and not as immediate or specific, causes of the disease. These predisposing causes, general or individual, were doubtless in wide and efficient operation; but they acted collaterally, and the existence of the one could be no proof of that of the other. Upon the whole, the objections enforced in the Report against this supposition must be held as conclusive. It is then inferred that the cause of the disease is a material substance, and that it is only partially distributed; and it is farther hinted that this matter is in the form of solid or liquid particles, capable of attaching themselves to the surfaces of other bodies. We suspect that this must be regarded as a wholly gratuitous supposition. A large body of evidence, we are next assured, renders it "certain" that human intercourse has, at least, a share in the propagation of the disease, and that, under some circumstances, it is the most important, if not the sole means of effecting its diffusion. But it is added, that it is also "certain" that the extension of the disease over large towns, if not over large areas, may take place independently of communication between the sick and healthy. This latter statement is attempted to be sustained by a series of negative proofs, which can have little weight when contrasted with more positive evidence. It is an error in science, when we have established, in a parity of cases, the truth of a fact or a principle by visible proofs, to seek for a different explanation of a similar fact or principle where the proofs chance to be less patent. If we see the husbandman sow the seeds of the ash, and the ash tree sprouts on the plain, we are not entitled to say that another seed has produced the same tree because it crowns some inaccessible cliff, or garlands a ruined tower, although no careful watch has noted the actual passage of the identical seed to its secluded place of germination. It is enough that, knowing the positive relations of cause and effect in the one case, we have no right to question them in the other, and still less to substitute an assumption in their place; and we conclude unhesitatingly that the seed has been conveyed thither by some agency, possibly by a bird, less probably by the wind, though the precise manner has eluded our detection. But the context appears to show that it is only the *direct* communication between the sick and healthy which the report supposes to be excluded; and this is merely to remove the question to the distinction between an immediate and mediate contagion, for a contagion remains still the source of causation.

Now, we have no reason to believe that any of the contagions of this class can operate, unless through the medium of fomites, at considerable distances from the body from which they have first emanated, or through which they have been transmitted in the course of a specific disease. A simple illustration to this effect occurs in a report by Dr Wilson of this city, drawn up at the request of the

authorities of the town of Kelso,[1] in which he was then resident, and which, we really believe, gives pretty nearly as accurate an expression of the law of dissemination as it is capable of admitting. If we know anything, it argues, of the nature of noxious emanations, we know that they are volatile poisons, diffusing themselves from their sources, of whatever description, in all directions, and gradually dissipating themselves wholly where there is free movement of the atmosphere. So apparent is this, that it seems impossible to consider them, at least in this individual respect, as governed by any other conditions than those which attend the diffusion of all other influences emanating from a central point; or, in more technical language, that their intensity diminishes in the proportion that the squares of the distances increase. Thus, if we assign to their virulence, within a foot of the exhaling body, a force estimated as 10,000, at the distance of ten feet it will already have diminished to 100, and at the distance of a hundred feet it will have dwindled to as one only; or it will be ten thousand times stronger at one foot than at a hundred. On the other hand, under the existence of circumstances limiting their diffusion, they necessarily accumulate, and acquire greater degrees of intensity at greater distances; though not without something like the same ratio being constantly preserved. We infer, therefore, that the contagious virus does not admit of being transported through the air to remote situations, without its undergoing that dilution which must render it innocuous; and we revert to its proved transmission by more direct communication in one class of cases, to account also for its transmission in another class, where the channels have been not the less too intricate, or too obscure, to reveal themselves to what can be rarely otherwise than a casual and inadequate observation. As to the occasionally rapid first extension of an epidemic, this occurs only rarely, where the virus chances, on its first introduction, to encounter numerous individuals predisposed to its influence, and grouped closely together, so that it may affect them at once, or in quick succession, and thus not only generate a more abundant and more concentrated reproduction of its poison, but establish simultaneously more multiplied points of contact with the surrounding population. As the predisposed undergo the attack, and their number becomes exhausted, the epidemic diminishes. Where none are predisposed, and we believe that a strong and marked predisposition is demanded, with, or without, the operation of a powerful occasional, or exciting cause, in a large degree distinctly appreciable, the virus is inert; just as any seed may fall where it cannot vegetate. On the other hand, where the predispositions may be considered strong, yet the disease has not presented itself, we are entitled to believe that the *tertium quid*, the contagious virus, has never approached with sufficient nearness. Nothing in nature depends upon a single condition. As

[1] General Report on the Sanitary Condition of the Town of Kelso. By Charles Wilson, M.D., p. 24.

circumstances give to one factor a predominating force over the others, its agency will become more conspicuous, and a narrow observation may be attracted to it exclusively; but the product is still the same, and the casual inversion of the chain of causation has not altered its essence, or removed one of its links.

It will be observed that the Report leans everywhere to the idea, that cholera is a disease propagated by contagion; yet not without a vacillation in its modes of expression, such as seems to imply an unwillingness to give to the opinion the full weight of its authority. We have once more a remarkable instance of this, in its fourth inferential proposition, bearing that the propagation of the disease by human intercourse does not prove its contagious nature. But surely words like these are not what Hobbes terms wise men's counters, by which they can reckon. If a disease can be diffused by human intercourse, it is a contagious disease in every just sense of the term. And again, in another part, under the head of preventive measures, we are told that it is probably a matter of little moment whether cholera patients are placed together in special wards, in an hospital, or dispersed among the ordinary patients; and yet, in the very next paragraph, we are enjoined to have the clothes of these patients removed from them in a special receiving room, and either destroyed, or subjected to a disinfecting process. We cannot pretend to comprehend the nicety of this distinction. The proofs of contagion gathered in this report are actually stronger, we verily believe, than could be readily adduced of any other disorder. What amount of evidence would satisfy those who insist on the value of negative facts, we cannot pretend to determine. Possibly it might be somewhat less than the extirpation of our race: certainly, it must be somewhat more than is demanded for small-pox, the contagious nature of which is nowhere doubted. Yet an individual may be exposed to the variolous contagion, in every possible manner, and never take the disorder; another may imbibe the poison in its extreme malignity, and perish, as we have ourselves witnessed, in little more than a single day; while a third may receive it in so mild a form, that it is scarcely attended with perceptible fever; and all this may occur in the same epidemic, in the same village, nay, even in a single family. And, moving as it does from place to place, with footsteps resembling those of the cholera, neither is it commonly easy to pursue the traces of the infection, though all with justice admit its identity and its existence.

For that division of the Report which relates to the pathology and treatment of cholera, we are indebted to the careful and arduous labours of Dr Gull. The summary of the morbid appearances, whether after death in collapse, or in reaction, is exceedingly interesting and valuable. As to the section on treatment, Dr Gull himself claims for it little that is positive, though we are afraid he takes a more sanguine view than rises legitimately from such premises, when he states that, having learned what is useless, we gain a better

ground for future success. For our own parts, we feel sufficiently that the profession is still merely in a transition state with regard to this formidable disorder; and we by no means despair that its startling phenomena, considered with a fitting spirit of exactness, will, by and by, group themselves into a definite form, and fall into their proper place in the records of science, to be assuredly met at last by more certain and definite methods of cure. In the meantime, the initiatory stages of the disorder are, in the vast majority of instances, immensely under our control; and we have, besides, the wide field of prophylaxis, with the brilliant aims for the moral and physical improvement of our race which it necessarily involves, and of the efficacy of which we possess ample and conclusive illustration. These aims are as entirely within the province of the enlightened physician as any other department of his career of usefulness; and, in whatever subordinate position he may be held by our Boards of Health, upon him all must finally lean, and to him only they can look for a wise and safe guidance. We could have wished to have entered a little farther into the subject of the treatment of cholera; but we shall content ourselves with adding, from our own experience, that we think we have seen good service, in cases of collapse, from the exhibition of the *liquor ammoniæ*, in doses of fifteen minims, repeated every twenty minutes, in a wineglassful of cold water. The remedy has always appeared grateful to the patient, and we would willingly see it receive a more extended trial. The lavish use of opium and of alcoholic stimulants we were early taught to abandon; but we should regret to see ourselves deprived utterly of their valuable assistance. In the troublesome vomiting, which occasionally associates itself with the consecutive fever, we have seen excellent effects from enemata, with laudanum and tincture of assafœtida, after other remedies had been vainly exhibited. We shall only farther add, that Dr Gull considers a primary absorption of poison into the blood, in this disorder, a highly probable hypothesis. It is possible that we might derive some additional light on this subject, were the origin, and modes of propagation, of our ordinary autumnal cholera to be somewhat more closely studied.

The summary of Dr Berg is also a work of great interest and value. It takes for its groundwork an account of the precautionary measures which were adopted to ward off the ingress of the disorder; the state of the weather, and of the general sanitary condition of Sweden, prior to its accession; the reports of the progress of the disorder in the districts affected; and, lastly, the special sanitary conditions of the districts which remained free from its attacks. Unable to proceed to an analysis of his work, we can only notice one or two of its points of interest. The reports from the different localities are considered successively, in the order of the dates of the accession of the epidemic in each, so that its progress is followed with an exactness as great as can possibly be admitted by such an in-

quiry. The poorest portion of the labouring class was, as usual, that which suffered most extensively, and the drunkard was everywhere a ready victim. Ague had been widely prevalent in 1847 and 1848, but it had then rapidly declined in frequency, and during the year before the irruption of cholera, it occurred only as an ordinary sporadic spring sickness. Typhus, often previously observed as an extensively diffused epidemic, presented itself only rarely during the year of the cholera, and in the three years which preceded it. This absence of typhus, as an intercurrent disease, is worthy of remark. We had ourselves an opportunity of observing, that, during the recent epidemic of cholera, it became associated with an extensive epidemic of measles, in a district with which we had an intimate acquaintance. The character of the latter disease was usually of a very mild description, not a single instance of a typhoid form having come under our observation: and, what was still more remarkable, there was little irritation of the mucous membrane of the bowels, so ordinarily a concomitant of measles; nor had we any cases, in our own practice, lapsing into cholera, through diarrhœa originating from this cause, although we are aware that such a sequence occasionally presented itself. In Sweden, the precursory diseases, represented as having been observed in this country, were not encountered, the general health having been at a higher than usual standard during several years previously. The premonitory, or, what should perhaps be rather termed the initiatory, diarrhœa was, however, recognized extensively, as a precious opportunity of efficient action for the physician, and of safety for the sick.

With regard to the question of contagion, it appears that, out of eighty communes, in four only could no prior intercourse with an infected quarter be detected previous to the irruption of the disorder; and these four were places situated on the coast, and having extensive commercial relations both seawards and landwards. Numerous infected vessels reached other ports; quarantine officers were affected; boatmen engaged in the inland traffic next suffered; often in a single family, or in its close vicinity, attacks followed in immediate succession; and many nurses and funeral attendants were seized and died. What wonder then, if from these facts, given with many illustrative details which we cannot transfer to our pages, Dr Berg concludes that no other inference can be drawn, than that cholera is not only capable of being propagated by contagion (personlig beröring, detta ord, analogt med contagium), but that in Sweden it has been actually so propagated? The remarks upon predisposition which follow are given with a clearness and a philosophical precision which is quite delightful, amid the farrago of undefined phrases and crude opinions which deform the common literature of cholera. The reporter does not appear to attach much importance to the efficiency of quarantine regulations or cordons. For our own country, we believe them to be wholly inapplicable. As to the period of incubation of cholera, he gives reason for surmising that it may vary from

two to twelve, or even fourteen days. It is of course the maximum which is easiest determined; and this can be best accomplished in ships at sea, the crews of which have been known to have been in contact with the disorder, in an infected port.

The Notes on Cholera by Dr Cormack are characterized by great acuteness and ingenuity, though we sometimes feel tempted to accuse him of showing too strong a tendency to idealize. What, for instance, are the splenic symptoms of a febricula? We think Pinel went far enough, when[1] he ranged cholera morbus in an annexed genus, beside his meningo-gastric fevers: but we are at some loss to imagine how able physicians, as Dr Billing and Dr Cormack, have felt themselves entitled to designate it an ague. It would be unfortunate for medical science, were it not that, at least in recent times, no disease is attempted to be treated, merely according to some abstract notion of its entity as it exists in a nosological system; but regard is always had to the physiological and pathological basis upon which the symptoms have grouped themselves. Still, nosological arrangements are of vast convenience in medicine; yet it seems to us we may as well divest ourselves of them at once, if they rule us so laxly that we can reduce under one category a disease, the cold stage of which, not to note its other special differences, is the initiative of a cycle of morbid actions, departing from it, and reverting towards it, with another disease, the cold stage of which is the termination of a series of morbid actions, which becomes either the harbinger of death, because the product of an inspissated, unaerated, and devitalized blood, or is the point of departure of a new order of phenomena showing no revertent tendency. We could scarcely attach any importance here to the author's quotation from Comparetti, even if we considered it applicable: but the "febbre perniciosa *collerica*-sincopale," is evidently an ataxic *bilious* intermittent; one of the multiform varieties which have been laid down, chiefly by continental writers. Had the Italian author himself designed to denote any relation to cholera, he would have written, not *collerica* but *colerica*. Dr Cormack's remarks on the use of opium, if not novel, are exceedingly judicious.

The pamphlet of Mr Tucker is devoted to the advocation of vegetable acids, especially as contained in cider and in vinegar, for the prevention, and of mineral acids for the cure, of cholera; and he appeals to the experience of a few localities in the cider districts, whose exemption from the epidemic he attributes to the liberal use of the indigenous beverage. This is, in many respects, a lively and an interesting pamphlet; though we own we could have expected, from an honorary secretary of an epidemiological society, a somewhat more cautious induction than he has adventured, upon the exceedingly limited number of facts which he has been able to adduce; for he, of all men, should hold steadily in view, that here it is only

[1] Nosographie Philosophique, T. III. p. 579: Ed. 5ième.

the mass of facts which can confer stability. We have had enough of the kind of reasoning of Fluellen, who could warrant that Macedon and Monmouth were alike, because there was a river in Macedon, and there was moreover a river at Monmouth. Besides, Mr Tucker is unhappy in some of his references. He cites the exceedingly respectable, yet here very insufficient, authority of the late Dr William Fergusson, Inspector-General of Military Hospitals, who has stated that he never heard of dysentery in Sweden, and who attributes the immunity of the people there to the free use of the smaller acid fruits. Now Berg[1] speaks of dysentery as a well known scourge in Sweden, which has hitherto been more fatal than the cholera itself; and Dr Huss,[2] in his interesting essay on the endemic diseases of that country, gives some particulars of its occurrence, in a truly murderous form, in the districts adjoining Lake Siljan. Out of a population of 138,000, there perished here, according to official returns, in the months of August and September 1839, no fewer than 2043 persons; and in other more recent years it has also prevailed extensively. Medicine is oppressed with this description of unsustained allegation and rash inference, and we think one of the chief duties of an epidemiological society ought to be to protect us from their inroads. Without forsaking this territory of Sweden, we could engage to prove by this description of reasoning, and were we entitled to rely upon merely isolated facts selected from documents now lying before us, that the extremes of filth, in home, in person, and in habits—bodies begrimed with dirt, ablutions unknown, inner garments of thick leather worn unchangingly till they fall in fragments, and so fetid that their wearers may be nosed at a distance, apartments confined, damp and unventilated, with floors either the bare earth, or hidden under a dense layer of impurities[3]—are actually safeguards against cholera, for such is the condition, not only of the lower classes, but also, in a large degree, of those in better circumstances in Christianstad; and yet Christianstad has remained twice exempt from the pestilence, while towns farther inland have been afflicted, as Wexiö and Jönköping, the latter having, in 1834, lost a seventh of its population.[4] But science demands broad inductions, and the consideration, not of one condition but of all conditions, and not in one place but in many places, before it can be safe in its inferences, or entitled to rank them as its assured property.

We must hurry over the other publications, whose titles we have annexed, with a brief notice. Dr Ayre reiterates his recommendation of his plan of treatment with calomel in small and frequently-repeated doses; and complains that the reporters of the Royal College have done him injustice in their comments regarding it. We confess that we ourselves have never been able to bestow our confidence upon this

[1] Om Cholerafarsoten i Sverge, p. 320.
[2] Om Sverges endemiska Sjukdomar, p. 23.
[3] Huss, Op. cit. p. 69.
[4] Berg, Op. cit. pp. 334, 343.

method. Dr MacLoughlin insists upon the uniform appearance of the premonitory diarrhœa, and in this, as a rule, we are glad to coincide with him; although we have seen collapse, terminating fatally, set in within an hour-and-a-half after the first loose evacuation. Dr Tucker, who is more odd than felicitous in his epithets, and styles cholera "that lash of the triple scourge of the human race," and a "portable pestilence," advocates the saline treatment, and is lucky enough to find that, under proper management, the disease is less fatal than typhus fever; a success which few have been fortunate enough to share with him. He challenges the merits of the mercurial treatment, and tells us that the sooner there is an end to it the better it will be (p. 40) for humanity; a statement which we leave him and Dr Ayre to reconcile as they best may. Mr Grove, who recommends sulphur and bicarbonate of soda, according to a formula (p. 19) which gives about four grains of each as a dose, but with the important addition of about half a drachm of compound spirits of lavender, advises, like the recent advocates of the castor oil treatment, that the diarrhœa should not be arrested too hurriedly, lest we should confine the morbific matter: and yet we have a very vivid recollection, that where we arrested the diarrhœa, our patients lived, and lived soundly, and where it persisted, they died. Mr Stephens recommends mercury, but he advises high doses, which Dr Ayre condemns. He finds an analogy for this treatment, in the process of "kyanizing" wood with corrosive sublimate. Now we love an analogy, not when it is accepted as a proof, but when it points in the direction of a proof; but we can scarcely hope for this advantage from that which is here presented to us. Where the object of the practitioner is to bring his patient under the influence of mercury in this disorder, we think he may best succeed by means of the fumigations which have been resorted to in various quarters, and especially in France. There is talent, if there had been but moderation, in "Common Sense, by a Practical Practitioner;" but as his common sense has little in common with our sense, we shall not pause to discuss it. If he is a young writer, we may meet him again with greater satisfaction.

The Spanish Medical Journal, which is also lying before us, supplies us with some interesting particulars of the outbreak at Barcelona. It appears that this city is the only Spanish port on the Mediterranean which has hitherto been affected, during the present epidemic. The panic which it caused has led to a hurried desertion of their homes by many of the inhabitants; and the disease seems thus to have extended itself at first to various places in the vicinity, and afterwards to remoter distances, and even into the high country. In the first fourteen days the deaths were 976; including two physicians, Dr Gali and Dr D. J. Esplugas. At a more recent period, the mortality has been as high as 300 daily. Sulphate of strychnia is reported to have been used occasionally with some advantage. The arrangements made by the authorities appear to have

been really excellent. Besides the general hospital, and another exclusively devoted to cholera cases, six houses of relief have been established in various quarters, each containing from six to ten apartments, and having medical aid in attendance, night and day, to assist those who might fall sick suddenly in the streets. For the domiciliary and gratuitous attendance upon the poor, the city has been divided into twenty-eight districts, each with an appointed medical officer; and nineteen shops have been designated in different localities as constantly ready to prepare the requisite medicines. Lastly, an ample daily remuneration ("una suficiente asignacion diaria") has been assigned to each of the physicians; with a pension to his widow or family should he fall a victim to the epidemic.

Truly, they manage these things better in Spain than in certain places in which we have a nearer interest.

On the Structure and Use of the Spleen. By HENRY GRAY, F.R.S., Demonstrator of Anatomy and Surgical Curator of the Pathological Museum of St George's Hospital. London, 1854. 8vo, pp. 380.

So important do we consider all the facts which relate to the spleen, in the present state of physiology and pathology, that we propose to give a full analysis of this work. The subject for the Astley Cooper Prize was happily chosen, and our scientific readers will naturally feel impatient to learn what has been the result.

The historical introduction extends over fifty-three pages, in which the author goes over pretty much the same ground as Dr Sanders did before him, without, however, in our opinion, giving such good accounts of the works of Malpighi and Hewson (undoubtedly the two great authors on the subject) as his predecessor has done. All the more recent observations which have tended so much to advance our knowledge of splenic physiology and pathology, including the researches of Kölliker, Handfield Jones, Landis, Gunsburg, Ecker, Gerlach, Schaffner, Hughes Bennett, and Virchow, are discussed in three pages, and we must say in a rather unsatisfactory and imperfect manner.

Mr Gray has made investigations to determine the mode of development of the spleen on more than one hundred dissections of the embryo chick, and adds, that as far as he has been able to ascertain, the same process is followed out in man and other animals. Now, considering that fœtal calves and sheep are easily to be procured at a slaughter house, we cannot but regret that some investigation to determine the fact in mammals had not been made, rather than that so broad an inference should be drawn from what occurs in the chick. Here, however, according to Mr Gray, the spleen makes its first appearance at the 114th hour after incubation, in a fold of the "intestinal laminæ," as a small whitish mass of blastema,

distinct both from the stomach and pancreas, this fold serving to retain it and the pancreas in connection with the intestine. This separation of the spleen from the pancreas is more distinct at an early period of its evolution than later, as the continued growth of both organs causes them to approximate more closely, though not more intimately, to one another. On the seventh day, however, its position is the same as in the adult bird. On the eighth day it is the size of a millet seed, forming a round reddish projecting mass from the surface of the membranous folds in which it was developed. From this period the spleen gradually enlarges, its colour becomes of a more vivid red, and its form somewhat circular. As soon as its vessels are formed, the membrane in which it was enveloped becomes almost absorbed, and it increases in size, retaining its form, position, and attachments.

The entire substance of the spleen at an early period is almost entirely composed of nuclei and granular matter. As the evolution of the organ proceeds, part of these elements become developed in trabeculæ, part into blood vessels and blood, whilst the greater portion remain to form the pulp tissue. The blood discs appear on the eighth day after incubation. Some of them may be seen in an incipient stage of formation, and consist of a dark irregular granular nucleus, around which a delicate wall may be observed. "Although," says Mr Gray, "this observation has been confirmed in many cases, I do not presume the spleen to be *the sole organ* in which the *development* of the blood globules takes place during intra-uterine life; nor have I observed that the development of the blood discs continues to take place in it after its connection with the general vascular system is affected." But as we shall subsequently see there are other grounds, besides these observations, which pretty clearly show that blood corpuscles are formed there notwithstanding. The Malpighian bodies, according to Mr Gray, are not developed until the period of incubation is near its completion—that is, between the twentieth and twenty-first days.

In the human subject, Mr Gray thinks it probable that the spleen may be developed as early as the third or fourth week of pregnancy. He has seen it well formed in a fœtus two months old, when its size is about equal to that of a large millet-seed. By the third month it is twice as large, after which it rapidly augments in volume. The organ, however, does not attain its maximum of development during fœtal life, but is largest in comparison with the body from the period of birth to the termination of adult life. This was determined by weighing 160 spleens at different ages. As regards the digestive process, Mr Gray determined, from several experiments on rabbits, that the size of the organ is increased during and after the completion of the digestive process. Further, as a general rule, its size is increased considerably in highly fed and diminished in starved animals.

The structure of the spleen is now so well known, through the

careful description of Kölliker and Sanders, that it is unnecessary for us to do more than allude to those points in which our author differs from other histologists. Speaking of the fusiform cells of the spleen, he tells us that they are *not* developed like other fibre cells. According to him the cell at one point becomes apparently absorbed, so that the circumference of the wall forms the fibre itself—the cell breaking at one point, whilst the nucleus retains its original position. The cell wall thus divided, according to him, gradually uncurls to form the spindle-shaped corpuscle (p. 98). We see no reason whatever to adopt this idea of Mr Gray, which is contrary to all analogy, opposed to the known laws of cell development, and, we will venture to say, is contradicted by all careful observation.

Mr Gray seems to have made some careful experiments to determine whether the spleen be or be not contractile. On applying the wires of a galvanic apparatus to the organ, shortly after the death of the animal, very slight changes were observed in it, whereas the muscles of the œsophagus, the large and small intestines, and bladder of the same animal exhibited, under a like stimulus, the most marked contractions. The conclusion derived by the author from these experiments is, that although exceedingly slow and faint contraction and wrinkling of the spleen's surface do occur, these phenomena are so trifling as to be incapable of explaining the variations which take place in the size of the organ.

With regard to the vascular arrangement in the spleen—a matter of the greatest importance—it results from the investigations of Mr Gray that he has made out capillaries in the organ which form large meshes and terminate in the veins. Some of them, however, he says, cannot be traced directly into veins, but are lost in interspaces among the pulp of the organ, into which injections forced through them escape, and from which the veins appear to commence. These interspaces are considered to be analagous to the intercellular passages seen in the lower tribe of plants, or to similar passages in the lower invertebrata, by means of which their venous circulation is mainly carried on. This anatomical arrangement is supposed by Mr Gray to afford a ready explanation of the constant occurrence of the diffusion of blood, and of its disintegration in the pulp tissue of the organ. In addition to the two modes of origin of the veins now pointed out, they are also described as commencing in cœcal pouches of various forms.

With a view of determining how far the quantity of blood in the spleen is influenced by different circumstances, various experiments were made on horses and asses, the details of which are not given very minutely. We presume, however (although it is not stated), that the same precautions and methods of proceeding were adopted in all of them. The results obtained from these experiments show that the quantity of blood that can be obtained from the spleen varies considerably under different circumstances. That during digestion, the greatest amount exists in it sixteen hours after taking food, when

the digestive process is completed; and the smallest amount forty-eight hours after eating. It is also small in quantity in ill-fed animals, whilst in those that are starved very little could be obtained from it. The blood in the spleen is also augmented after an animal has drank fluids to any extent, or when, by means of transfusion, the entire amount of blood in the economy is directly increased, these facts induce the author to adopt, as in a great measure correct, the reserve function of the spleen brought forward by Dodgson.

The histology of splenic venous blood, commences with the following sentence:—"Many very important differences have been observed in the *microscopic examination* of the venous blood of the spleen, as compared with other venous and arterial blood" (p. 145). The italics are the author's, and after puzzling ourselves in vain to determine why the microscopic more than any other method of investigation, should present important differences, we at length gathered from the context, that it was not the examination, but the venous blood itself which exhibited the differences. These are said to be, 1*st*, A considerable variation in the size, form, and colour of the blood discs. 2*d*, The existence of blood discs contained in cells. 3*d*, The constant occurrence of granular or crystaline colouring matter, free, or contained in cells. 4*th*, A considerable number of colourless corpuscles. These latter, composed of free nuclei and nucleated corpuscles, are described by Mr Gray, as only differing from similar corpuscles in lymph by the greater number of the latter relatively. Their appearance and structure, he says, cannot but lead to the inference that they are derived from the spleen, during the transit of the blood through it. Notwithstanding these admissions and confirmations of what has been seen by every careful observer before him, he denies that the spleen and other lymphatic glands can be secretors of the blood. "The observations above mentioned, as well as the results of the chemical analysis of the blood, will," says Mr Gray, "I think, negative this conclusion" (p. 151). Now, with great deference to the author, so far from being opposed, we will venture to state they only tend to confirm the theory referred to.

Mr Gray evidently attaches great importance to the 111 separate analyses of splenic blood, made by Dr H. M. Noad. We should like to have known the method of analysis followed, and the precautions employed to avoid fallacy in these experiments, more especially as the name of Noad is new to us as an analytic chemist, but nothing whatever is said on these important points. Presuming, however, that the same method and precautions were employed in all of them, it would seem from the results obtained, that splenic venous blood contains less solid matter than arterial or other venous blood; far less red blood globules; a considerable increase in the amount of albumen and fibrine; more fat; a variable amount of iron; whilst the serum presents in all cases a dark reddish tinge. In order to determine whether blood corpuscles

are formed or destroyed in the spleen, we are told that that method of separating the corpuscles, was had recourse to which afforded the most accurate results. We regret that the author has not informed us what this most accurate method is, which was employed in nineteen experiments, with the result of proving that little more than one half the amount of corpuscles exist in the emerging blood of the spleen, as compared with the blood which enters the organ, or with other venous blood. It is not very clearly stated by Mr Gray whether this diminution refers to the red or colourless corpuscles, or to both. As to the fibrine, its amount bears some relation to the amount of blood corpuscles in the blood, the greatest quantity existing where there is the greatest diminution of the blood corpuscles, the smallest amount where there is no diminution. Mr Gray has also observed that the blood of ill-fed or starved horses always affords a very considerable amount, as compared with the blood of well-fed animals. In this, the author confirms the observations of Nasse and Zimmerman. The albumen of splenic blood is also increased in amount in proportion to the diminution of corpuscles, while the coloration of the serum depends on the free hœmatin dissolved in it.

Mr Gray describes the elements of the spleen pulp to be colourless and coloured. The one consisting of molecules, nuclei, and cells in various stages of cell development, and the other of blood corpuscles free or enclosed in cell walls in different stages of disintegration. The former are most abundant when the animal is well nourished, and the latter also exist in much smaller amount in starved or ill-fed animals. From a careful series of analyses, the author concludes there is no uric acid or hypoxanthin in splenic pulp, a result wholly opposed to the elaborate researches of Scherer. Which is the better or more accurate chemist, further researches must determine. The principal element of the spleen pulp is an albuminous product, or as the author expresses it, a nutrient compound. There is also a considerable amount of hœmatin and iron, resulting from the disintegration of the blood corpuscles. It has an acid reaction from the pressure of lactic and phosphoric acid.

By the splenic corpuscles (p. 221), it would appear that Mr Gray means the Malpighian bodies or closed capsules of the spleen. We need scarcely say, that these are not the only splenic corpuscles, and that this term so applied is very faulty and vague. The Malpighian corpuscles, as he more properly calls them afterwards, are well described, and especially the structure of their capsules, the existence of which in man has by some observers been doubted. It consists of a prolongation from the sheaths of the small arteries, which send branches over them. On their external surface they are surrounded by a close plexus of capillary vessels of extreme minuteness. The veins, which are of considerable size, even at their origin, commence on the surface of each vesicle, throughout the whole of the circumference, and exist in such numbers as to form a dense

venous mesh, in which each of these bodies is enclosed. This demonstration of the direct communication of the Malpighian bodies, with the veins as illustrated by fig. 42, is in our opinion the most important fact which Mr Gray has recorded in his work. His account of the contents of the Malpighian corpuscles presents nothing new. They, like the pulp of the spleen, consist of an albuminous compound, and bear the same relation to the state of nutrition of the animal.

The superficial lymphatics of the spleen, Mr Gray tells us, are easily injected, as they are not provided with valves, and appear to commence in an exceedingly delicate net-work of vessels. He has not been able to demonstrate with the same certainty the origin of the deep lymphatics, but conjectures that they have no direct connection either with the Malpighian corpuscles, or the substance of the pulp. In man they are far less numerous than in the ruminantia and the horse. The lymph they contain is similar to that found in other lymphatics. The reddish colour in the deeper vessels of the organ is owing to the admixture of blood corpuscles, occasioned either by anastomosis between the lymph and blood vessels, or by their occasional rupture causing communication. Mr Gray, however, admits that the presence of blood corpuscles in splenic lymph is a *constant* occurrence, hence it cannot be supposed to be dependent on occasional causes. Moreover, having previously shown that large veins and plexuses seem to originate from the Malpighian corpuscles, and now stating an opinion that the lymph and blood vessels anastomose, it appears to us that, according to his own investigations, there must be a very ready communication between these corpuscles and the blood. Mr Gray seems to think that the lymph of the spleen cannot serve to form blood corpuscles, because it is like other lymph—because the blood corpuscles it contains are fully formed—and because their disintegration is constantly going on in the organ. These arguments are valueless, for no one supposes that the spleen, as a blood-forming gland, should yield a different lymph from the others. Then disintegration of blood may be seen to go on in other glands as well as in the spleen, especially in the ovaries and mesenteric glands. Lastly, though perfect blood corpuscles may get into the lymph of the spleen, in the manner the author points out, no one believes that the products of the organ are converted into blood within itself. On the other hand, what becomes of the innumerable nuclei and cells formed in the Malpighian corpuscles and pulp which the author has taken great pains to show find their way into the blood? Indeed, how in opposition to his own facts, he maintains that the spleen has no "relation with the lymphatic system, or with the formation of blood corpuscles," it is to us surprising.

Under the idea, however, that the spleen has no relation to the lymphatic system, he enters at great length into its comparative anatomy, and it is curious to observe that whilst almost every fact

he brings forward, affords abundant proof of this relation, he can no where find it out. Thus, he says, that in the reptilia and fishes the organ is comparatively of small size, that it diminishes in bulk in the lower vertebrata, and is absent in the invertebrata; but instead of seeing that this is explained by the increase of the lymph system in the lower animals, and its more ready communication with the blood vessels, he can only infer from these important facts that the organ regulates the quantity and quality of the blood. As to a table at page 273, in which we are told the proportion the spleen bears to the entire body in various animals, it proves nothing. From previous tables we see that the size of the organ varies greatly in an adult man, we may, therefore, reasonably ask how many lions, ostriches and snakes have been examined before we conclude the spleen is to the whole body in the first as 1 to 264, in the second as 1 to 3673, and in the third as 1 to 11,150. We must accord to Mr Gray, however, great praise for the pains he has taken in the dissection of numerous animals, and for his accurate description of the form and size of the spleen, throughout the animal series.

From the various facts in the first 339 pages of his volume, of which we have given a tolerably full outline, the author proceeds to form his conclusions as to the functions or physiology of the spleen. But we are greatly surprised that having had recourse to anatomy, healthy and comparative, to the mode of development, to histology and to chemistry, morbid anatomy should have escaped his notice altogether. Without depreciating in any way the importance of the methods of investigation undertaken by Mr Gray, we will venture to say that none of them are of so much importance as a careful observation of the morbid alterations of the spleen, and of the symptoms which they produce. It must, we think, be clear that any treatise professing to investigate the structure and use of the spleen which ignores the remarkable alterations which that organ undergoes in many diseases, and the influence which such alterations have been proved to exercise over the blood, must necessarily be very imperfect, and such undoubtedly we consider this essay of Mr Gray.

Conclusions derived from limited and partial facts we may anticipate would be vague and unsatisfactory. According to some physiologists, the spleen is a blood-forming, and according to others it is a blood-destroying gland. The grand conclusion arrived at by Mr Gray is, that it may be both. "The function of the spleen," he says, "is to regulate the quantity and the quality of the blood." In itself, this conclusion means very little, for it must be clear that whether the organ preserves or destroys blood, it must act as a regulator. Every gland is a regulator of its proper secretion. Mr Gray, however, evidently labours under the idea that this notion of his is somewhat novel, and proceeds to point out how the facts previously described agree with his theory. Of course they do. Nobody has the slightest doubt in the present day, that not

only the spleen but the mesenteric and other blood glands powerfully influence both the quantity and the quality of the blood. Allowing this, it is unnecessary to follow the author farther, for all his facts are in harmony with the views of those, who, commencing with Hewson, look upon the spleen as a blood-forming gland. The author concludes by saying that it has appeared to him unwise to enter into any detailed controversy with those whose opinions are at variance with his own. Who these persons may be we cannot conceive, and consequently think that in not disputing with his imaginary opponents, Mr Gray has exercised a sound discretion.

We confess that the perusal of this volume has caused us some disappointment. The structure and functions of the spleen have of late years excited great attention, and our own pages have contributed in no small degree to the elucidation of both. Mr Gray has added little to the histological facts lately so well and succinctly described by Dr Sanders of this city. The mode of development in the chick, the chemical analyses, and the comparative anatomy descriptions, may be important hereafter, but at present require confirmation. Dr Sanders has pointed out that the Malpighian corpuscles were occasionally traversed by large vessels, and we have already said that we consider as an important fact (should it be confirmed), Mr Gray's statement that large veins originate from those bodies, which would be a modern demonstration of the view taken 200 years ago by Malpighi. We trust that the preparation from which fig. 42 was copied, has been deposited with the others, in Guy's Hospital, and is open to the examination of the histologist. Of the many important questions which are now being agitated by those who are investigating the spleen in its healthy and morbid relations, and of which our present number offers an example in the controversy now going on between the Physiological Professors of Edinburgh and Wurzburg, we learn little from Mr Gray's essay. We do not deny that from the evident pains and labour displayed, that it was worthy of the Astley Cooper prize, we only regret that the author did not take a wider grasp of his subject, and so been led to enlarge our knowledge of the functions of this important organ. But although, in this respect, the work falls short of our expectations, we cordially recommend it to our readers, as an important contribution to the anatomy and physiology of the spleen.

The Census of Ireland for the year 1851. Part III. *Report on the Status of Disease.* Folio. Dublin, 1854. Pp. 150.

This report contains an elaborate account of all those persons in Ireland who laboured under diseases, either at their own homes or in public institutions, upon the night of the 30th of March 1851. Great differences of opinion prevail as to the utility of such laborious

researches. Statistics are considered by some to be a powerful means of advancing the science and art of medicine. For our own part we believe that neither one nor the other are yet in a position which will admit of the application of this method of inquiry. Be this as it may, all who look into this report, must feel persuaded that it is scarcely possible to conduct statistical investigations more carefully, arrange the results more methodically, and comment upon them more instructively than we find to have been here accomplished. Nay, more, nothing but a profound conviction of working in a right direction, could have supported Mr Wilde of Dublin during such tedious computations, and enabled him to bring them to a successful termination: We have no doubt that all statisticians will regard this report as one of the greatest value, and that at some future time it will be looked back upon as a work not only of great literary merit, but as one which is well calculated to confer permanent reputation on its author.

Anatomical and Physiological Observations. By JOHN STRUTHERS, M.D., Lecturer on Anatomy, etc. Part I. Edinburgh, 8vo. 1854. Pp. 239.

THE contents of this volume are for the most part familiar to our readers, as they originally appeared in our pages. We do not, however, the less recommend them to the professional public, in this their collective form, believing that they will well reward the reader, besides adding to the reputation of the author. No. VII., on the mode of action of local blood-letting in affections of the internal viscera, is a paper of great importance, eminently entitled to the best consideration of the medical practitioner.

Part Third.

PERISCOPE.

PHYSIOLOGY.

THE PHYSIOLOGY OF MENSTRUATION AND CONCEPTION.—BY PROFESSOR BISCHOFF.

Professor Bischoff, previous to relating the following thirteen cases, states that the examination, which is always difficult, should if possible be made on the fresh parts, as immersion in spirits, or even water, is apt to occasion mistakes.

1. The body of a healthy, strong woman, of about 20 years of age, examined on the 5th of December 1839. The signs of existing menstruation were manifest; there was general turgescence, and blood effused in the cavity of the uterus and in the vagina. The ovaries were fully developed, and contained numerous developed Graafian follicles, which could be easily turned out of the

soft and vascular stroma of the ovary. Both ovaries exhibited several corpora lutea in various stages of degeneration: the right one presented one freshly ruptured and filled with blood. The search for spermatozoa or an ovum was unsuccessful. The inner surface of the uterus was developed into a velvety appearance, and seemed on a transverse section to be reticular, on a longitudinal section to be shaggy; the uterine glands were somewhat enlarged. There was no distinct division between the mucous membrane of the uterus and the other structures.

2. On the 26th of October, 1840, the body of a strong young woman who had drowned herself was examined. The organs of generation were well developed, and the right ovary contained an unusually large Graafian follicle. This was still closed, and contained an ovum. The cells of its disc were round, not spindle-shaped; and therefore the ovum and the follicle were not yet ripe. The zone was very pale, and without sharp outlines; the yolk did not completely fill it. Besides this follicle, the right ovarium contained one, and the left two corpora lutea in different stages of degeneration. The search for spermatozoa was vain. Although in this case there were no signs of present menstruation, it was plainly impending. The inner membrane of the uterus appeared to possess small villi, which were plainly nothing more than the already partially developed uterine glands.

3. On the 7th of June 1843, a strong, healthy woman, who had never been pregnant, drowned herself in the Nekar. The circumstances attending the case were such as to render it possible that connection had recently taken place. The ovaries were of considerable size, soft, and each contained a great number of very large Graafian follicles. Their surface showed numerous cicatrized spots, and the left ovary a very marked yellow body. In the right ovary there was a perfectly cicatrized bluish-red spot, in which a fresh blood-coagulum showed that a Graafian follicle had recently burst; its walls were thickened, and as corpus luteum was in course of development. The os uteri was deeply injected, and, as well as the cervix, was closed up with a delicate opalescent mucus, proceeding from the numerous highly-developed Nabothi. The inner surface of the uterus was vividly injected, but no blood was effused; it was, however, covered with a whitish mucus, which under the microscope was found to consist almost entirely of cell-nuclei. On a perpendicular section, the tissue of the mucous membrane was found to be well defined from the proper fibrous tissue of the uterus. There were no glands, and especially no developed cylindrical uterine glands, nor a villous condition of the inner surface. A transverse section showed an injected network of blood capillaries encircling round openings, probably of small, undeveloped grandular follicles The right Fallopian tube and the uterus were searched in vain for an ovulum. It was ascertained positively that menstruation had just taken place in this person.

4 and 5. In the spring of 1844, Professor Bischoff received from Dr Varrentrapp, of Frankfort, the organs of generation of two young women who had died in the hospital of an acute angina whilst menstruating. The parts were not highly developed or injected. In both, the right ovarium exhibited a freshly-burst Graafian follicle, containing a blood-coagulum; the walls showed the commencing development of a corpus luteum. The inner surface of the uterus showed in neither case a very marked development. In neither could perfectly-formed uterine glands, or the beginning of a decidua, be discovered. In both cases an ovulum and spermatozoa were sought for in vain.

6. The sixth was remarkable by the considerably advanced formation of a decidua, the developed uterine glands, and the perfectly-formed corpus luteum. Although no ovum was found, it was concluded that conception had taken place.

7. On the 9th of August 1851, a young woman of 19, glowing with health, drowned herself. She had been married three weeks before against her will. The organs of generation could only be examined on the 13th, when, through

the warmth of the weather, decomposition had commenced. The parts were, however, much developed. The right ovary exhibited a follicle recently burst; the point of rupture was large, and surrounded with the remains of the broken follicle and of the tunica albuginea; the follicle retained a very slender connection with the structure of the ovary, so that it could be easily taken out with the forceps; but this was perhaps owing to decomposition. In its cavity was found a considerable blood-coagulum. The inner surface of the uterus was highly developed, vascular, velvety, and, when floated out in water, showed a commencing decidua. No ovulum was found either in the Fallopian tube or in the uterus. The hymen was destroyed. It is certain that she had not borne a child.

8. On the 26th of June 1852, the body of a young woman of 19, who had hanged herself two days before, was examined. The organs of generation were fully developed. At the uterine end of the right ovarium there was seen a very large corpus luteum projecting far from the surface, as it often does in the cow, but as Professor Bischoff had never before seen it in the human female. This corpus luteum was not quite round, but, like the ovary, flattened; its long diameter was 29·5 milimetres, its width 22·5, and its thickness 9. The ovary weighed 17 grammes. It was very vascular, and contained a black-coloured cavity. Its thick walls consisted of a yellow body, of irregular folds; it was easily turned out of the substance of the ovary. Both ovaries further showed numerous scars, and several blackish, ancient, spurious corpora lutea, of which one on each side was very large. The uterine mucous membrane was highly developed, and formed a stratum, the boundaries of which were very distinct from the fibrous tissue of the uterus. This development extended to the os uteri internum, and terminated abruptly at the insertions of the Fallopian tubes. The glandulæ utriculares were beautifully developed, thickly planted, and bound together by a soft intermediate substance. The inner surface of the uterus was not very vividly injected; it was quite smooth, and covered with a very distinct and beautiful cylindrical epithelium. The cylinders were bigger than those of the Fallopian tube. The hymen was destroyed, but she had never borne a child. In this case, also, no ovum could be found; but just at the opening of the right tube into the uterus was found a peculiar spot in the tissue of the mucous membrane, which, under the microscope, exhibited a nest of cells, granules, and spindle-shaped cells. In this a proper ovum could not be identified; but if the fate of the human ovum resemble that of the porpoises this point seemed especially fitted to serve for the attachment of the ovum during its earliest development in the uterus. This young woman had menstruated as usual during four days, eighteen days before her death. She had intercourse with a poor lad, who had promised her marriage. Professor Bischoff concludes, from the persistent development of the corpus luteum, and of the uterus, eighteen days after menstruation, that conception had taken place.

9. On the 15th of July 1852, a young woman of 19, otherwise perfectly healthy, died of trismus, following a slight injury of the nose. She came into the hospital on the 10th; menstruation began there, and lasted till the 13th. The parts were examined on the 16th, but the warm weather had already induced considerable decomposition. The parts were perfectly formed, but bore no trace of unusual turgescence. The right ovarium showed a dark blue follicle, projecting over the surface of the uterus. No opening on the surface of the ovary could, however, be discovered. The follicle was filled with a blood-coagulum. It may have been that an opening existed, but the point of rupture in mammalia is for the most part so minute, and closes so quickly, that it is often difficult to find. Both ovaries showed, besides, several scars, and smaller ancient corpora lutea; and the left ovary one of larger size, with a scar overhanging the surface of the ovary. No unusual development of the inner surface of the uterus was observed. The mucous membrane on a perpendicular section was 1 millimetre thick, and showed no glands. The inner layer was

discoloured, and without marked epithelium. This person had not borne a child. Whether she was a virgin is not known. No ovum could be discovered.

10. On the 9th of August 1852, a young woman of 20 drowned herself, after a quarrel with her lover. The body was examined early on the 11th, whilst quite fresh. The parts were strongly developed, and the uterus, from its unusual size, indicated pregnancy. In the right ovary there was observed a recently-burst Graafian follicle, not projecting over the ovary, forming a scarred dark-red spot on the surface. One out of five irregular fragments of a freshly-formed corpus luteum could be easily turned out; a dark-red coagulum filled the cavity. The inner surface of the uterus was soft, velvety, smooth, vividly red, and marked with single dark-red spots. The mucous membrane was thick, and allowed the very beautifully developed glandulæ utriculares to be distinctly seen. Between them ran numerous fine blood-vessels, which formed a capillary network on the surface around the mouths of the glands. The os uteri was large, scarred, and indicated a previous delivery. No ovum or spermatozoa could be found. No certain information could be obtained as to the antecedents of this person. The great development of the corpus luteum favoured the conclusion that conception had recently taken place.

11. On the 4th of December 1852, the organs of generation of a strong young woman, who had drowned herself on the evening of the 30th of November, were examined. Tbe parts were well developed, but showed that the person had not borne a child. Both ovaries showed on their surface numerous scars, and especially in the left one were four or five distinct remains of false corpora lutea, as well as numerous Graafian vesicles shining through. In the right ovary there appeared a still closed, but very large Graafian vesicle. No opening or scar could be found, and the spot fluctuated. Taken out, this follicle weighed 3·48 grammes. When opened there flowed out, not the usual limpid contents of an unopened follicle, but a thick, chocolate-brown mass, which formed no coagulum, and consisted, under the microscope, of altered blood-corpuscles. The walls of the follicle were not thickened, and no commencing structure of a yellow body was observed. The cavity of the uterus contained no blood; it was not reddened at the fundus, but the cavity of the cervix was so. The mucous membrane was little developed, and was not distinguished by defined boundaries from the fibrous tissue. The glands were scarcely observable. No delivery had taken place. No ovum or spermatozoa could be found. It was ascertained by Dr Lorenz, of Offenbach, that this person had menstruated ten days before her death, and that coitus had afterwards taken place.

12. On the 7th of December 1852, the body of a woman of 34, who had died suddenly after an injury of the cervical vertebræ and spinal marrow, received during an epileptic fit, was examined. It was only some days after that, a fresh corpus luteum being observed in the right ovary, the organs of generation were investigated. The parts were fully developed. Both ovaries presented a surface much scarred, and the left one the remains of five or six corpora lutea. The right one showed a fresh, conspicuous scar, and beneath it a corpus luteum, consisting of a ragged yellow surrounding substance, which enclosed a greyish, seemingly discoloured coagulum. The inner surface of the uterus was not bloody, and the mucous membrane was slightly developed. Uterine glands were not observed, either with the naked or assisted eye. No delivery had taken place. The search after an ovum must necessarily have been useless. It was ascertained that she had menstruated seven days before death; recent connection was improbable.

13. On the 21st of May 1853, the body of a woman, aged 27, who had hanged herself on the evening of the 19th, was examined. It was unusually fat. The organs of generation were strongly congested, and the uterus large, as in the case of a multipara in the non-pregnant condition. In the right ovary was a large ruptured follicle, which, in consequence of commencing decomposition,

was pultaceous. There was no blood coagulum, and the walls were not thickened. The follicle retained but slender adhesion to the stroma. The inner surface of the uterus was much macerated ; it was covered with a bloody mucus, but was clearly villous, owing to the formation of a decidua, and the enlargement of the uterine glands. The os uteri was large and widely open ; the vagina was large and covered with reddish mucus. Ovum and spermatozoa were sought for in vain. Dr Pfeffer, of Butzback, learned that this person had been married, and had borne a child fourteen months before, which she had suckled until within three weeks, when it was weaned for want of milk. During lactation she had been regular, and for the last time on the 21st of April, exactly four weeks before her death. She had connection for the last time on the 15th of May, four days before her death.

Professor Bischoff remarks that these cases confirm the doctrine, that in woman at every menstruation a follicle ripens, swells, and bursts, that the ovum escapes, and that a corpus luteum is formed. The eleventh case seems to prove that the full consequences of menstruation are not in every instance necessarily carried out, but that a follicle may swell, and the ovum ripen without the bursting of the follicle, or the escape of the ovum. Such a condition will cause sterility, notwithstanding menstruation. Perhaps, also, the pain of the menstrual period depends upon this incomplete development.

A woman may not menstruate, and yet she may conceive ; for the essential condition, the ripening and escape of an ovum, may proceed, and only the usual outward symptoms of this event, the secretion of blood, fail. Professor Bischoff remarks, that he could never succeed in finding the ovum, but observes that he does not believe that it is so soon destroyed. He says there are but two cases known to him where it was so found, and that only one of these presents the desired certainty. This is the following observation of Professor Hyrtl, of Vienna :—Thérèse Michal, aged 17, died on the 10th of October 1844, of peritonitis, in the clinique of Prof. Oppolzer. She had only menstruated twice in her life, and the last time was two days before her admission into the hospital on the 8th October. The breasts were tolerably developed ; the pubes slightly covered with hair. The hymen was uninjured ; the uterus of moderate size, compact, its cavity filled with a considerable quantity of thick blood ; the mucous membrane was so loosened as to resemble a half-liquid plastic exudation. The lining membrane of both Fallopian tubes was suffused, loose, and covered with mucus. Both ovaries were of fair size, but the left one showed a ruptured follicle of the size of a large hazel-nut, filled with semi-fluid blood. Prof. Hyrtl submitted the parts to careful inspection under the microscope on the same day. Neither in the vagina, uterus, or tubes, could he find a trace of spermatozoa. But by a careful examination of the left tube, in that part which traverses the substance of the uterus, he found an ovulum, with all its characteristic properties, somewhat dull and turbid indeed, but fully recognisable. Hyrtl believes he even detected the germinal vesicle. This is a remarkable anomaly, since the germinal vesicle of every ovum, when it has left the ovary, has disappeared. Prof. Hyrtl had the ovulum immediately drawn by his artist, and exhibited it on the same day in his lecture on physiology. This case is free from doubt, as there is no room to suspect coitus. Prof. Bischoff regrets that he cannot admit as much with reference to the cases of Dr Letheby, published in the " Philosophical Transactions" for 1852, but his reasons of dissent do not appear to be very valid.

Prof. Bischoff takes occasion to discuss the question as to the differences between the corpus luteum of simple menstruation, and that of conception. In the cow and sow the corpora lutea of the non-pregnant are altogether similar to those of the pregnant, and he has made the same observation in the bitch, the cat, and the porpoise. They disappear more quickly when no conception follows. In the human female, at the earliest period of the bursting of the follicle, the filling of it with blood, and the beginning of the development of the membrane of the follicle, to the formation of the corpus luteum, there is

no difference at all. But in the further development of the corpus luteum, the impregnation of the ovum and gestation bring about a material difference. The corpus luteum in simple menstruation never attains the full stage of formation up to the complete filling up of the follicle and the metamorphosis of the blood-coagulum ; but this substance only developes itself into a peripheral layer, and soon falls into retrogressive metamorphosis. It shrivels up, and only leaves at the next and the succeeding menstruation a more and more indistinct spot, changing from yellow to brown and black, and soon there remains nothing but a scar ; which lasts for a time, and gives evidence of the past process which has at an earlier period taken place on the surface of the ovary. The corpus lutea of conception, on the other hand, goes on developing itself throughout the first months of pregnancy, and attains a fulness of size, colour, and texture, which the menstrual corpora lutea never reach. It lasts throughout the whole period of pregnancy, and disappears after delivery. It may, however, be questioned whether it has a great diagnostic value. In the first period, so long as there are no other signs of pregnancy, these differences either do not exist, or are very difficult to determine. After delivery, when the signs of pregnancy in the uterus have disappeared, it again becomes difficult to distinguish them from the corpora lutea of fourteen days or three weeks' standing, resulting from menstruation. And where pregnancy is plain, from the state of the uterus and other parts, the corpus luteum is of no value. Prof. Bischoff, therefore, concludes that the corpus luteum cannot be used in the determination *of doubtful cases.*

Professor Bischoff holds it to be proved that conception and pregnancy depend absolutely upon menstruation as the period of the ripening and expulsion of an ovum. He remarks, however, that it can scarcely be doubted that the times occupied by the transit of the ovum from the ovary, by the arrest of the ovum, and the preparatory changes in uterine mucous membrane, suffer great differences in individuals, and hence entail a longer or shorter duration of gestation.—*Henle's Zeitschrift, and Brit. and For. Med. Chir. Review.*

ON THE ACCOMMODATION OF THE EYE. BY PROFESSOR L. FICK.

Fick maintains the view now almost generally adopted, that the mechanism of the accommodation is to be found in the alteration of the form and position of the lens. He considers the *uvea* to be the apparatus which effects, besides the regulation of the quantity of light (through the motion of the pupil), the accommodation for near and remote objects, by means of a change in the position and form of the lens, placing varying quantities of blood now before, then behind the lens.

The facts on which this view is based are the following :—1. The very limited connection between the vessels of the choroidea and the iris. 2. The facility with which, by injection from the simple venæ vorticosæ and ciliary arteries of the choroidea, a considerable part of the less minute arteries and veins, forming the larger meshes of the choroidea, and a corresponding portion of the processus ciliares, is filled,—while the capillary layer of the choroidea (membr. Ruyschiana) is only filled by increased force, the vessels of the iris remaining empty. 3. The vascular arrangement in the choroidea and processus ciliares exhibits no analogy with the vascular apparatus of other organs destined for their nutrition, exhibiting scarcely any capillary system except that of the membr. Ruyschiana, but an almost immediate transition from the arteries into the veins. 4. The perfectly normal state of the vascular apparatus in the uvea of Albino's demonstrates that the production of pigmentum can certainly not be the only function of this apparatus. 5. The existence of so strong a junction between the capsula lentis and the choroidea where the prolonged folds of the choroidea project into the posterior chamber of the eye. 6. The *contractility of* of the *processus ciliares*, which L. Fick assumes as well from experiments performed with A. Fick (having produced contraction of the congested ciliary process by means of electricity), as also from the circumstance of the processus ciliares being, after death, always found empty, except after death by strangu-

lation,—as also from pathological facts. 7. The peripheric part of the lens s softer than the central.

Fick sees in the *erectile* and *contractile* vessels of the processus ciliares an apparatus by which, the form of the bulbus remaining unchangeable, in the state of impletion of the vessels, pressure is exercised on the anterior capsula of the lens, while through their depletion into the venæ vorticosæ a pressure is exercised on the posterior capsula, effecting the lens slightly to advance towards the anterior chamber, and to become at the same time rather more convex—a process in which the difference in the consistency of the fluids of both chambers is likewise of importance.—*Muller's Arch., and Brit. and For. Med. Rev.*

THE DOCTRINE OF THE SENSE OF TOUCH. BY DR G. MEISSNER.

Meissner endeavours to prove in the present (as already in a former[1]) essay, principally by theoretical remarks, that the sense of touch is a special sense, that the specific perception of this sense consists "in a psychical state of excitation, the essential and distinctive property of which is that it possesses in itself a compelling reason for the soul to form the idea of an external object in contrast to anything belonging to our own body." The sensation of touch appears to Meissner not identical with that of pressure. The so-called sensory papillæ are, according to this view, the specific organs for the sense of touch, in the same manner as the retina is the organ for the sense of sight.—*Henle's and Pfeüffer's Zeit's. fur rat. Med., and Brit. and For. Med. Rev.*

ELSÄSSER ON THE ALKALINITY OF HUMAN MILK.

Elsässer examined the milk in 385 cases in the Stuttgard Lying-in-Hospital, and his experiments, like those of Donné and Simon, have shown that freshly drawn human milk, in the healthy condition of the breast, is alkaline in reaction. Among these 385 cases he found the milk neutral in only 45 cases, and only in one instance did he observe an acid reaction. Among animals, on the other hand, viz., among cows, mares, and sheep, he found that the milk in its normal and healthy condition varied very much in the reaction it gave. In carnivorous animals, as cats and dogs, he found that the urine was only acid when they were fed on a *mixed* diet.—*Würtemberg. Corresp. Blatt. No.* 28. 1853.

PATHOLOGY AND PRACTICE OF PHYSIC.

ON HYDROPHOBIA. BY DR ZIMMERMAN.

During 1851 and 1852, an epidemic of hydrophopia raged in Hamburgh having come there with the Austrian troops from Jutland, where it originated in 1850, through Schleswig and Holstein. 267 cases occurred, 125 in the town itself, most of them shut up and closely observed. Cases of both raging and dumb madness occurred, both apparently depending on the same poison. Dr Zimmerman gives a good account of the post-mortem appearances, as these differ in no respect from those formerly published in this Journal, we pass to a more interesting point, viz., its communicability to man. He knew of thirty people bit by mad dogs, not one of whom became hydrophobic, not from treatment preserving them, for only a few were treated, and they only with the usual external appliances, and he and his friends heard of many more who had likewise been bitten without any bad consequences, and without employing any, even the simplest, preventive measures. In fact, during the whole course of the epidemic, the only man who became *hydrophobic*, and died from it, was bitten by a dog *not mad*. The dog was killed, and examined by Veterinary Surgeon, Schräder, who had made 120 dissections of dogs during the epidemic, and who distinctly asserted there was little or nothing wrong with any organ in the body, and certainly not a trace of the appearances proper to *rabies canina*. Zimmerman says, that in all

[1] G. Meissner: Beitraege zur Physiologe der Haut. Leipzig, 1853.

known cases the disease found its origin only in the bite of a dog already mad, and that other animals, as horses, so bitten, likewise became mad. The last opinion seems to require confirmation ; the former is not borne out by his facts of fifteen dogs bitten by a known mad one, two only became mad; two ran away, eleven remained healthy. He instances also the gradual dying out of the epidemic, after orders had been given to destroy all dogs found at large; but surely this cessation was to be expected at any rate, after a nearly two years' duration of the epidemic, and the destruction, moreover, of 1100 dogs. The fact also of the dogs inhabiting the islands of the Elbe, escaping while all the neighbouring country was ravaged by the disease at the most, only shows the disease to have been contagious and does not necessarily show the bite to have been the means of rendering it so. And, besides, such unaccountable immunities are continually presenting themselves even in diseases known to be epidemic, and we here use the word as the contrast of contagious; while the escape of at least eleven out of fifteen bitten by a dog known to be mad, shows that even to its own species the bite is not so fatal as supposed; and to man, how much less. He does not believe that an animal poison can remain latent and local for days or months, and then develop itself. He is likewise of opinion that hydrophobia in man, possesses no characters common to that of the dog, but is truly a species of tetanus, and probably often produced by mental emotion; though, doubtless, the bite of a mad or even angry dog may cause it. Both from fright, and also, from perhaps some alteration in the fluid of a nature similar to that caused by anger or anxiety in the breast milk of nurses, which many a time has killed the child by convulsions or tetanic spasms.—*Casper's Vierteljahrschrift für gerichtliche medicin.* Band. 4, Heft. 1.

DR DRESSLER OF WURZBURG ON A CASE OF INTERMITTENT ALBUMINURIA AND CHROMATURIA.

A weakly boy, æt. 10½ years, was seized every forenoon with a feeling of intense cold accompanied by chilliness of the extremities, a small weak pulse, pale face, and blueness of the nose and ears. This lasted for some hours, and terminated without the occurrence of heat. Sleep and appetite were not impaired by the attack ; but vomiting sometimes occurred during it. On the 27th December, the urine passed immediately before the attacks, was observed to be small in quantity, and of a dark reddish-brown colour ; it deposited an amorphous, granular, dirty-brown sediment, which contained no blood corpuscles. It was very faintly acid in reaction ; and when treated with nitric acid it yielded a dark flocculent precipitate, of albumen combined with colouring matter. The urine voided before and after the attacks displayed the same peculiarities. A few days after the occurrence of the attacks, and after the commencement of medical treatment (which consisted of the administration of quinine and flesh diet), viz., on the 1st January, the urine contained no albumen, but deposited a granular pigment. Two days after this the albumen reappeared, in combination with pigment and urate of ammonia. For several days the urine contained albumen ; the morning urine was clear and transparent, while that passed at night was brown coloured, and deposited a sediment. The symptoms meanwhile underwent a change, by the paroxysms of cold subsiding, and a feeling of general unpleasantness, accompanied by pain in the feet, succeeding in their stead. The albumen and pigment continued to be found in the urine for several weeks; and the patient finally recovered by the persevering employment of quinine, decoct. Cinchonæ, and flesh diet, with latterly a little wine. Virchow obtained a large quantity of blue pigment or aroglaucin (*Harnblau*) from the urine of this boy.—*Virchow's Archives für Pathol. Anatom., Bd.* vi. Hft. 2.

PROFESSOR MAUTHNER ON THE TREATMENT OF ALBUMINURIA OF CHILDREN.

Formerly it was customary to treat this and all other dropsical affections with diuretics. Mauthner, however, in the dropsy which occurs after scarla-

tina, and is accompanied by bloody and albuminous urine, sedulously avoids whatever might overstimulate the kidneys, as likely to cause an acute renal inflammation, or an attack of Bright's disease. He has seen many cases where the swelling increased daily in spite of the administration of squills, digitalis, bitart potassae, etc., in which a cure was effected by a simple regulation of the diet. In such cases he advises the exclusive administration of milk, and rice and milk, or at most of *decoct. althecæ*, or of *decoct. lini. sem.* By the exclusive use of milk-diet, he has seen the action of the kidneys so regulated, that a copious secretion of urine, free from albumen, was established, by means of which the dropsical condition was removed. When the administration of milk fails to effect a cure, he endeavours by means of alkaline agents, to alter the condition of the urine; and, for this purpose, he prefers using the alkali of the urine itself. He accordingly administers *urea* in small doses (either in the form of pure urea, or of the nitrate of urea), giving generally the third of a grain for a dose, in combination with a little powdered sugar. After six or eight grs. of the urea had thus been taken, he has frequently seen profuse diuresis occurring, followed by the speedy disappearance of the dropsical affection.—*Journal für Kinder Krankheiten*, 1854. *Hft.* 1 *and* 2.

PROFESSOR ALBERS ON THE ACTION OF THEINE AND CAFFEINE.

From experiments made upon frogs and rabbits with the citrates of caffeine and theine, Professor Albers has acertained the following to be the physiological actions of these substances :—

1st. Both salts are capable of producing in frogs a tetanic rigidity of the body as complete as that induced by strychnia. 1 gr. introduced below the skin has this effect.

2d. They affect not only the external parts, but also the action of the heart, and in this respect theine is the more active narcotic.

After the administration of both alkaloids, the heart was found to be much contracted, a circumstance which distinguishes their action from that of other narcotic alkaloids. Their action occurred more violently in the cold-blooded than in the warm-blooded animals; strychnia, on the other hand, affects both in the same degree. Caffeine produces the same effects when administered internally as it does when introduced into a wound. It is worthy of note that although caffeine affects thus violently the nervous system of frogs, it acts, when given to men, or especially to women, as a most powerful sedative and anodyne, especially in cases of neuralgic headache. The dose in such cases should not be large; from 3 to 5 grains is rather too much; 2 gr. is quite sufficient to administer.—*Deutsche Klinik.* 51.

PROFESSOR MAUTHNER OF VIENNA ON THE TREATMENT OF CRUSTA LACTEA AND INFANTILE ECZEMA.

The following treatment is adopted for cases of crusta lactea in St Anne's Hospital at Vienna, by Professor von Mauthner. The crusts on the head are softened by the application of oil, and the scalp is covered during the process with oiled silk, or with a portion of an ox's bladder. In some cases the hair is cut, and the parts are washed with tepid water and soap. To prevent the eczema spreading to the forehead and eyes, he circumscribes its progress by means of a boundary of collodion. When eczema, affecting the face and extending under the ears, is discharging much matter, he orders it to be dusted with hair-powder; and when it is accompanied by great itching he applies the *ceratum cetacei*, according to this formula, ℞ *cetacei* ʒvj.; *cerae alb.* ʒij.; *ol. olivar. opt.* ʒj. *Misce.* He has found warm baths of great service, but the use of cold water—in the form of baths, douches, or epithems—although it sometimes has a good effect, generally predisposes the children to severe diarrhœa, dropsy, and hydrocephalus. With this external treatment Mauthner always combines the internal use of an aqueous extract of walnut-tree leaves, in half-drachm doses dissolved in water. Cod-liver oil he uses occa-

sionally, but he does not find it so beneficial, except where there is a complication of scrofula or rachitis.—*Journ. für Kinderkrankh. Hft.* 9 *and* 10, 1853.

G. MEISSNER ON CONFERVOID GROWTHS UNDER THE NAILS.

It has long been known that the external skin, like the mucous membranes of the digestive and respiratory passages, may be the seat of vegetable growths analogous to confervae, or to elementary excrescences. G. Meissner, a student of medicine at Göttingen, has recently described a formation of this sort which occurrred below the nails of a man aged 80. The nails of this man were much curved ; they were a line in thickness, and were of a yellowish colour, here and there approaching to brown. The nail of the forefinger of the right hand was the only one which was normal. When sections of the nails, treated with caustic potash, were placed under the microscope, Meissner observed a rich network of confervoid filaments lying among the cellules of which the substance of the nail was composed. The growth resembled that of porrigo lupinosa, or of pityriasis versicolor. The filaments measured from $\frac{1}{900}$ to $\frac{1}{700}$ and sometimes only $\frac{1}{1000}$ of a line in breadth. In the midst of these filaments, which articulated and ramified together, composed the mycelium, the author found sporangia, in the form of larger filaments, swollen and club-shaped ; and among these sporangia were scattered a multitude of greenish granules which represented the spores. Hence this parasitic growth was in some measure incorporated wiih the nail, and vegetated in its interior.—*Archiv. für Physiol. Heilkunde,* 1854.

KONOPLEFF ON CROTON-OIL IN DYSENTERY.

On the supposition that dysentery is generally dependent upon some serious obstruction in the bowels, the above author administers croton-oil for the cure of this disease. He adds from 1 to 3 drops of the oil to an emulsion composed of Aq. menth. pip. ʒvj., and Aq. Laurocerasi ʒj.—ʒij. ; and of this mixture he administers to adults a table-spoonful, and to children a tea or dessert spoonful every half hour. After three or four doses copious stools are passed, at first with and thereafter without blood; and by carefully regulating the diet and administering mucilaginous drinks, after the use of the above mixture, the patient is speedily cured. Should severe diarrhœa follow the use of the emulsion, he alleges that it is easily removed by the Decoctum Salep combined with Aq. Laurocerasi, or Vinum Opii. In the event of the mixture causing irritability of the stomach and vomiting, he administers the croton-oil in the form of an enema, *without* Aq. Laurocerasi, but combined with from 3 to 10 grs. of Ext. Hyosciami. Konopleff has administered a mixture containing one drop of the oil to ʒiv. of emulsion to infants at the breast, with the best results. He affirms that by means of this treatment he has never lost a single patient.—*Med. Zeitung Russland,* 48. 1853.

DR DEITERS ON THE USE OF CUBEBS IN INFANTILE ENURESIS.

This author has found cubebs more effectual than any other remedy in curing the incontinence of urine so common among children. This complaint may depend upon atony of the bladder, or on the presence of intestinal worms. In the former case the cubebs acts as a tonic, in the latter as a valuable anthelmintic. The medicine requires to be given in considerable doses ; two pinches (*i.e.* a few grains or *Zwei Messerspitzvoll*) for infants, and half a tea spoonful twice or thrice daily for children of a somewhat more advanced age. Its effect is speedy and permanent ; and although occasionally it happens that during its administration the incontinence returns at periodical or irregular intervals, these recurrences gradually become less frequent, and eventually disappear altogether. To effect a radical cure, the author has often found it necessary to continue its use for a period of from three to eight weeks, and he has never observed any injurious effects from its administration.

Deiters observes that he has found the same remedy most efficacious in

checking nocturnal emissions in cases of spermatorrhea.—*Preus. Verein. Zeitung, No.* 16. 1853.

BLASIUS ON THE USE OF BIRCH-OIL IN ECZEMA.

For upwards of fifteen years Blasius has been in the habit of using *Oleum Rusci*, the empyreumatic oil of the common birch (*Betula Alba*), with great success for the treatment of eczema. The affected parts are well smeared with the oil every day, and then enveloped in linen cloths; after this has been done for a few days they are well cleansed with soap and water, and then the application of the oil is recommenced. This treatment is continued until not only the formation of vesicles and secretion of fluid are arrested, but until the skin resumes its normal whiteness, smoothness, and softness. Blasius does not recommend the application of the oil so long as the eczema continues to be *acute*, and attended with inflammation; and when, in *chronic* cases, it occasions pain, burning, and itch, attended by swelling and redness of the parts, he discontinues its use for a few days, until these troublesome symptoms have quite subsided. This oil is called "Dagged" in Russia and Poland.—*Deutsche Klinik, No.* 29. 1853.

FRERICHS' CASE OF SUGAR FOUND IN THE FLUID OF ASCITES.

In a case of fatty-liver (*speckleber*), which occurred in the Medical Clinique at Breslau, ascites existed to a considerable extent, but there was no indication of hepatic carcinoma. In the acitic fluid, removed by the trocar, Frerichs found abundance of sugar, which has never hitherto been discovered in this effusion, when occurring separate from a diabetes. In other cases of ascites, resulting from cardiac disease, etc., this author tried to detect sugar in the effused fluid, but without success. In the case related he attributes the presence of the sugar to the influence exercised by the liver over the formation of sugar in the animal economy; which process, he has observed in many cases, is not interrupted by the occurrence of ascites. In cases of this disease, he is of opinion that sugar should always be looked for in the abdominal effusion, inasmuch as a knowledge of its presence therein, is useful in forming our diagnosis.—*Wien. Med. Wochensch.*, 6, 1854.

GROHE ON THE COMPOSITION OF EXUDATIONS.

This author has succeeded in demonstrating the presence of urea in all abundant exudations occurring on the pleuræ or the pericardium, where there existed no venal disease or complication of uraemia. He formerly showed, by negative proofs, that there existed, in the flesh of frogs, the materials for the formation of crystals of nitrate of urea. Like Scherer, he was unable to find any urea in the exudations within the abdomen, or in the liquor amnü.

He found sugar in the pleural and pericardial exudations in a case of epilepsy, and also in a case of chronic pneumonia, combined with suppuration and infiltration of the lymphatic glands. As to the cause of the sugar in the first of these cases he offers no opinion; but in the latter he thinks its presence was occasioned by the interruption of the respiratory functions by the disease.—*Oerhaudl, d. physic—med. Gesell, zu. Würzbürg.* iv. 2.

PROFESSOR MAUTHNER OF VIENNA ON THE APPLICATION OF CONIIN IN SCROFULOUS INTOLERANCE OF LIGHT.

In the non-inflammatory blepharospasm which so frequently occurs in scrofulous children, this author has found the external application of coniin to be productive of the greatest benefit. He gives the following formula for its use: ℞ *Coniinae gr. ss: Ol. Amygdal. dulc. ʒj Solve.* Sig. The eyelids to be pencilled with this emulsion twice or thrice daily. By the use of this preparation, he alleges that the most obstinate cases of blepharospasm may be cured in from eight to fourteen days.

In hard indolent glandular swellings of the neck, he has found this remedy

more efficacious than the application of iodine ointments.—*Journal für Kinder Krankheiten*, 1854. *Hft.* 1 *and* 2.

BREITHAUPT ON NITRATE OF SILVER IN INTESTINAL ULCERATIONS.

A musketeer, suffering from abdominal typhus, was attacked, in the 5th week with severe dysentery. The use of acetas plumbi and opium subdued the dysenteric symptoms in three weeks, but the fever still continued; the stools were mixed with pus and blood, bed-sores had formed on several places, and the patient was extremely emaciated. All other medicines had failed, when the administration of $\frac{1}{12}$th grain of nitras argenti, four times daily, dissolved in distilled water and combined with opium, was found highly beneficial. The dose was subsequently increased to $\frac{1}{6}$th grain, and the patient recovered completely. He took, in all, 48 grains of the salt. No evil results followed the use of the silver, and the colour of the skin was not in any way altered.—*Med. Zetung des Vereins für Heilk. in Preussen.*, 1853., *No.* 20.

DR HÖRING ON THE EXTRACTUM SANGUINI BOVINI.

This somewhat novel remedy is now beginning to be pretty much used by German physicians. It is prepared according to Mauthner's process, by straining the blood of the ox through a fine sieve, and drying and powdering the residue. Of this medicine Höring gives from 10 to 30 grains in the 24 hours, according to the age of the patient. He relates the particulars of three cases in which he administered it; the first, a case of curvature of the lower part of the spinal column, with loss of power of limbs; the second and third, cases of tuberculosis, occurring in a child aged 3, and a man aged 40. The treatment was successful in every case, the curvature being arrested in the first, and the cough and emaciation greatly improved in the second and third. —*Würtemb. Corresp. Bl.* 32, 1853.

SNELL ON CUTANEOUS ANÆSTHESIA IN LUNATICS.

A lunatic received a severe injury of the great toe by the fall of a heavy piece of wood, so that the nail was torn away. The author on examining the patient, remarked that he seemed scarcely to feel this injury, ordinarily so very painful. On examining the case more minutely, he found that this lunatic had completely lost cutaneous sensibility. This circumstance led him to examine all the lunatics in his establishment, and he found that out of 180 insane persons (100 males and 80 females), 18 (17 males and 1 female) had complete anæsthesia of the skin; and that in 6 others, who were males, the sensibility to pain was very much decreased.—*Zeitschrift für Psychiatrie, X.* 2.

ESPENBECK ON THE USE OF BELLADONNA IN PROFUSE SALIVATION.

A woman who had been treated profusely with mercury for the cure of enteritis, had violent salivation from the use of the drug. Extract of belladonna was ordered by Espenbeck, in doses of $2\frac{1}{2}$ grains in an emulsion; and the following day the salivation was found to be completely arrested, and the mouth dry. When the administration of the belladonna was suspended, the ptyalism returned; and again it disappeared when the use of the drug was resumed. Espenbeck has employed it successfully as a prophylactic against salivation.—*Hannover, Corresp. Blatt., June* 1853.

GARCIA OF ALVAREZ ON THE UTILITY OF FLOWERS OF SPARTIUM SCOPARIA IN ANASARCA.

Rayer recommended a trial of this remedy in albuminous nephritis, and the above author has found it very serviceable in general anasarca following this disease. The patient was a man, æt. 40, and the anasarca was completely removed, and the albuminuria cured, by the administration of an infusion of the flowers of this plant for a period of fourteen days.—*El. Siglo. Medico., Feb.* 1854.

SURGERY.

FÜHRER ON INFLAMMATION OF JOINTS.

The chief inflammations which attack the capsules and cartilages of joints are the traumatic, the rheumatic, the podagric, the fungus, the arthritic, and the tuberculous.

Under *Rheumatic* inflammatory joints Führer not only ranks the simple acute articular rheumatism, where the cavity of the joint is filled by thin flocculent synovia, where its living membrane is slightly injected, and the cartilage is partially affected—but also two other varieties. The first of these is acute, and is characterised by rapidly occurring purulent formations, by plastic exudations on the vascular synovial membrane, and by softening of the capsule of the joint. In the other form there is manifested a tendency towards necrosis; the interior of the capsule is found covered by a blackish-green exudation, the internal ligaments are affected, and the bones are discoloured and carious. There is also thickening of the external capsule, as well as of the periosteum and neighbouring parts. The cartilage, nevertheless, remains unaffected for a considerable time.

In the first stage of *arthritic* inflammation we find exudation into, and softening of, the spongy bone-substance; in the second, we have infiltration of fat, and abnormal ossification; and in the third, atrophy and disfiguration of the ends of the bones. In the *acute* form of this inflammation the spongy substance is swollen and vascular, the medullary cavity is filled with a reddish gelatinous exudation, and the shaft of the bone itself is thin and easily cut. The cartilage also is thinned, and often considerably broken: the synovial membrane is pale, shining, and devoid of epithelium; and the cavity of the joint is filled with a thin, reddish-brown, and fetid fluid. In the *chronic* form, the substance of the bone is rendered less compact, and is infiltrated with oleaginous medulla. Sometimes the cartilage is loosened and hypertrophied; at other times it is thinned, full of perforations—and often it is wholly wanting. There is no exudation into the joint; but shaggy vascular excrescences or fatty polypi are found fringing the synovial membrane at certain points. The bone here becomes absorbed; but externally, and also at the border of the cartilage, new masses of bone are deposited in the form of stalactites, and the cartilages become ossified. Moveable cartilages (Gelenksmäuse, or Corpuscula Mollia), are found within the joint, which consist probably of thickened fringes of the synovial membrane, or pieces of cartilage, which have become detached.

The pathological anatomy of *podagric* inflammation of joints is very insufficiently understood. It generally occurs after the fortieth year, and is characterised by an abundant secretion of the urates. In its *acute* form it affects principally the small bones of the hands and feet; the ligaments and cartilage are not changed; but the surface of the joint is covered with a chalky exudation. In the articular cavities, as well as often in the neighbouring bursæ mucosæ, there is found a thick white fluid, which is rich in uric acid and urates. In its *chronic* form it occurs as an anomalous gout, and causes destruction of the articular cavity, change (*obsolescence*) of the cartilage, and ivory-like thickening of the ends of the bones. No new bone-growth seems to take place.

The *fungous* and *tuberculous* inflammations of joints constitute the so-called *tumor albus*, or "white swelling." The *former* variety occurs principally in the joints of the knee, the tarsus, and the hand; its course is chronic and painless. The joint admits of an abnormally great degree of passive motion; and fistulous openings enter the bones of the articulation without proceeding further. The synovial membrane and cartilage are covered with soft greyish-red granulations, resembling atonic ulcerations, and similar appearances are also observed on the periosteum, and in the fistulous openings. The surrounding parts have a doughy feeling; the fasciæ and muscles are filled with exudation, and are often changed into a firm texture, abounding in fatty constituents. When healing occurs the granulations are changed into pseudo-membranes, which cover the articular cartilage, and often become chalky or ossific. In

tuberculous inflammation the articular cartilage remains unaffected for a long time ; but eventually it becomes perforated here and there by fistulous openings, which communicate with deep-seated abscesses in the bone. Thereafter pus enters and fills the cavity of the joint, which soon macerates the remaining healthy cartilage. The diseased epiphyses, before this takes place, are covered with a cheesy exudation, of a shining yellowish or mother-of-pearl-looking appearance. If this exudation becomes purulent, the joint is filled with flaky pus, and numerous smooth but discoloured fistulous openings extend from the interior of the cavity to the external surface of the skin.

Acute empyema of joints occurs chiefly in the puerperal state, but not unfrequently during pregnancy. It takes place also in phlebitis, and in pyæmia ; in the desquamative stage of scarlatina, and after typhus fever. It is characterised by its rapid progress into caries of the articular extremities of the bones. —*Archiv. fur. Pathol. Heilk. Bd.* v. Hft. 1.

DR STURM'S CASE OF VESICO-INTESTINAL FISTULA.

A man, aged 40, who had always enjoyed good health, experienced, in 1844, the first symptoms of a hemorrhoidal affection. His stools were irregular and constipated ; there were pains in the sacral region, and there was considerable distension and enlargement of the veins surrounding the anus. Shortly after this time he felt severe shooting pains of a peculiar character, extending from the root of the penis to the extremity of the glans, and recurring at brief intervals. In December 1844 he observed that during micturition, and sometimes independent of this act, gas escaped through the penis. Soon afterwards he discovered in his chamber-pot fragments of some almonds he had eaten the preceding day ; and he found that portions of feculent matter accumulated in his urethra, and for a while obstructed the passage. Gradually his alvine evacuations ceased to be passed by the anus. Catheterism of the bladder and of the bowel failed to reveal any communication between the bladder and the intestine. In 1845 Dieffenbach was consulted, but his prescriptions did not do much good, and the only relief the patient experienced was obtained by drinking large quantities of water. His general aspect for a time continued good ; his appetite was unimpaired ; his strength was little diminished, and his emaciation was but slight and tardy in its course. The pains, however, latterly increased in severity, becoming quite intolerable, and occasioning maniacal excitement. In April 1845, sixteen months after the commencement of his illness, he died from peritonitis.

Autopsy.—On laying open the abdomen, there escaped a large quantity of fœtid brownish fluid, mixed with purulent flakes. The ascending and descending portions of the colon were adherent to the other intestines, and also to the thickened mesentery. The liver, spleen, and gall bladder were covered by firm pseudo-membranes, but their textures were unchanged. The ascending part of the colon was distended by gas, and contained feculent matter ; the descending portion, at its inferior part, was so firmly adherent to the bladder and the adjoining organs that it could not be separated from them. About eight inches above the anus, there existed a still firmer adhesion between the bowel and bladder to the extent of two inches in diameter ; and in the centre of this adhesion there was found an aperture of an inch in diameter, by which free communication was established between the bladder and the intestine. The bladder was filled with yellowish fæcal fluid ; its mucous membrane was thickened, of a bluish marbled colour, covered with yellowish mucus, and presenting numerous very prominent varicose dilatations. There was no diminution of the calibre of either the colon or the rectum ; the mucous folds of the rectum were very much developed ; and immediately above the anal sphincter there was a circle of dilated veins. There existed no traces of cancer or tubercles.

Dr Sturm considers the fistula to have resulted from the ulcerative inflammation of a vesical varix, which had first occasioned an extensive local adhesion, and had thereafter proceeded to perforation.—*Deutsche Klinik.*, 1854

NUSSBAUM AND PAULI ON ARTIFICIAL CORNEA.

Monographs on this subject by each of the above-mentioned authors have recently attracted a great deal of interest and attention in Germany; we therefore offer a brief analysis of their treatises, in hopes that it may not be uninteresting to British surgeons and oculists.

Nussbaum's operation consists in making a longitudinal slit in the cornea, which he lays open horizontally to the extent of $1\frac{1}{2}$ lines. Into this he introduces a plate of glass (having a grooved margin 1 line in length, and $\frac{3}{4}$ line in breadth), the central or free portion of which is $3\frac{7}{8}$ by $1\frac{3}{8}$ lines. The border of the glass embraces the margins of the cornea, as a shirt-button is grasped by the button-hole. According to Dr Nussbaum, the reaction of the cornea and the other parts of the eye is very trifling, and is easily subdued by antiphlogistic measures; and he alleges that the glass when once placed *in situ* remains fixed without irritating the eye. *The object of the operation is to introduce into a dim or opaque cornea, a fragment of transparent glass of very small diameter, but of sufficient size to render vision possible.*

The appearance of an eye having this glass apparatus in the centre of the cornea is not repulsive. A whitish (leucomatous) thickening occurs around the margins of the glass, which is traversed by numerous vessels arising from the margin of the cornea. Nussbaum's experiments have as yet only been performed on rabbits.

Dr Pauli undertook a series of experiments on this subject, and in detailing them he criticises Dr Nussbaum's assertions. He cannot understand how a plate of glass, difficult to handle on account of its small dimensions, could be introduced so easily without pressing on the iris, and without hurting, by contact, the iris, and other delicate textures of the eye. Nor can he understand how scarcely two drops of the aqueous humour escape during the operation. The glass, he says, would not close the orifice completely, but would leave a sufficient aperture for the slow discharge of the aqueous humour, and afterwards excite traumatic iritis. Dr Nussbaum considers that an opaque cornea would bear the apparatus better, because, possessing less vitality, the reaction would not be so great. But this is purely hypothetical, inasmuch as we know nothing *positive* as to the vitality of the cornea when thus altered, or of the effects which would be produced upon it, and the neighbouring parts of the eye, by the presence of a foreign body. Moreover, numerous attempts have been made to substitute the healthy cornea of an animal for that of a man, when changed by disease, but the cornea in most cases, where at first the operation seemed successful, has generally been observed to become opaque and unfit for sight. Dr Pauli offers a prize of 100 Louis to any one who can cite a case of this kind which was followed by authentic success. We subjoin the references of the original papers.

Vide Nussbaum. *Siebold and Kölliker's Zeitschrift f. Zoologie.* iv. 2 and 3.
—— Pauli. *Deutsche Klinik.* 44.

DR NICHOLL'S CASE OF EXTENSIVE VAGINAL FISTULA OF THIRTEEN YEARS' DURATION SUCCESSFULLY TREATED WITH THE ACTUAL CAUTERY.

Anne Shanly, aged 37, was admitted to the Infirmary on the 2d of July 1851, in a most pitiable condition. Her thighs were quite excoriated from the incessant dribbling of urine; the smell from her was most offensive; she was reserved and sullen, as if her mental faculties had partially given way from her long and hopeless suffering. Her statement was, that thirteen years previous she had a severe and tedious labour; for three days and nights the head remained blocked in the passage, and was ultimately delivered by instrumental assistance; shortly after the urine commenced dribbling from her, and continued to do so, though she had been in some of the city and country hospitals. On making an examination, I found a large oval communication between the bladder and vagina. The mucous membrane of the anterior wall of the bladder somewhat protruded. Cauterization seemed to me the only

chance of relief; but the opening being so large, I apprehended the cautery would injure the anterior wall of the bladder. After some deliberation, I considered that with catheters I might be able to protect the anterior of the bladder whilst applying the cautery. Having provided two large-sized silver male catheters, I introduced them by the urethra, then through a two-bladed speculum cauterized freely the edges of the fistula.

This I continued to do once a fortnight. After a few applications there was a decided improvement, as she could retain her urine three or four hours, and was relieved of the stench and excoriation. Her mental condition was also improved, as she became more cheerful and communicative.

It is unnecessary to trespass on your columns by a tedious detail of the progress, as my object is to show that a case, apparently hopeless, should not be abandoned as quite beyond relief; suffice it to say, that on the 21st of February 1852, she was discharged from the Infirmary, being able to retain her urine from night to morning, as clean-skinned and cheerful as any woman in the workhouse, where she remained nearly a year after, and never complained of any return of her distressing malady.—*Dublin Med. Press.*

FALCK ON DENTAL CARIES AMONG DIABETICS.

This author attributes the frequency of caries of the teeth in diabetes to the presence of free lactic acid in the saliva, dissolving the mineral part of the dental substance. He has proved by experiments that the substance of the teeth is unchanged by the action of pure solutions of sugar. The presence of lactic acid he considers due to the action of the saliva-ferment on the grape and diabetic sugar in the economy—*Dentoche Klinik, No.* 3.

MIDWIFERY.

CASE OF ANTEVERSION OF THE UTERUS OF TWELVE YEARS STANDING, CURED BY THE INTRA-UTERINE PESSARY. BY DR MAHER, CHIEF NAVAL SURGEON AT ROCHEFORT.

While the celebrated discussion on the utility or uselessness of intra-uterine pessaries was going on in the Académie de Medécine at Paris, there appeared in the *Union Médicale,* the following case of anteversion cured by instrumental means. We quote it as a sort of corollary to the discussion of which we have given a report in the two previous numbers.

Dr Maher says :—" In February 1853, I was consulted by Madame L., æt. 38, a woman of a good constitution, who had been married eighteen years. In 1838 she had borne a child at the full time, and in Jan. 7, 1831, another at the seventh month of gestation. Since then she had been always very ill, and had submitted to innumerable modes of treatment, which, instead of ameliorating, only aggravated her sufferings." (Dr Maher here gives a long extract from an account of the patient herself, describing her symptoms in a very graphic manner. She was very feeble, and never free from pain, especially in the left side ; she had insupportable headaches; copious leucorrhea, and her eyesight was much impaired. She suffered more in winter than in summer, and especially before the catamenial periods. Latterly, she was so ill that she could only move by supporting herself on both hands). " The speculum and manual examination enabled me to detect anteversion of the uterus, with notable engorgement of the cervix, which was red and hard, but without ulceration or granulations. The body of the uterus was easily reached by passing the finger along the anterior wall of the vagina ; the cervix was inclined deeply backwards, and a little inclined to the left side, as if there had been a combination of latero and ante-version. Dr Guillard, chief naval surgeon, confirmed the accuracy of my diagnosis ; and in his presence I introduced the uterine sound, which occasioned a sharp but transitory pain during its passage towards the internal orifice of the cervix, but not a drop of blood was lost. By means

of the sound I ascertained that the uterus was quite mobile, and free from any adhesions, and that I could place the cervix in its normal position. I then withdrew the sound, and recommended the patient to be kept quiet for the rest of the day. Three days afterwards I found the uterus as at first. I introduced M. Valleix's instrument (*redresseur*), which was well borne, but by way of using every precaution, I withdrew it again in the evening. Two days thereafter I introduced it again, and kept it *in situ* for twenty-four hours. By and by, as it was occasioning no inconvenience, I allowed it to remain in the uterus for six days at a time. Menstruation came on, and the lady removed the instrument herself. Two months and a half after the commencement of the treatment, M. Guillard and I had the satisfaction of beholding the displacement as well as the engorgement gradually disappearing, and the strength of the invalid much improved. (She says :—" As soon as the instrument was introduced—after the pain I felt at first had subsided—I felt better and could walk about. My headaches and pains in the side have disappeared. I have no leucorrhea ; my menstruation is regular, and not more painful than is usual in health ; and for *the first time for ten years* I can read and sew without using spectacles "). " In May last," continues Dr Maher, " I saw Madame L. She could undertake long journeys on foot, and could attend to all her household duties. She ate and slept well. The uterus was no longer displaced, and the cervix was flexible (*souple*), and of normal aspect.

" I would not, from this case, advise the introduction of a bougie into the cavity of so delicate an organ as the uterus in *all* cases ; but in cases like this, where all known modes of treatment have failed to relieve the distress of the patient, we are bound, in humanity, to have recourse to mechanical means."

HAMER'S CASE OF CÆSARIAN SECTION, WITH RECOVERY OF MOTHER AND CHILD.

The particulars of an interesting case in which Hamer of Ommerschans was operator, have just been communicated by Dr Ranneft of Frederiksoord :—

The patient was a primipara, æt. 30, who in early life had been affected with rachitis. Labour pains commenced, at the normal end of pregnancy, on the 2d February 1853, which were regular and normal, both as to length and frequency. At 8 P.M. the membranes burst; but owing to the head not then descending into the pelvis, and the pains becoming arrested, it was thought advisable to resort to turning. The hand could not be introduced, however, for want of space between the pubis and the promontory of the sacrum, the distance between them being only $2\frac{1}{2}$ inches. To save the life of the child, the Cæsarian section was performed at 11 P.M. The external incision was made along the linea alba, but on laying open the uterus it was found that the knife had divided the placenta, thereby causing a considerable amount of hæmorrhage. The extraction of the placenta and the child occupied only a few seconds, and the uterus thereafter became firmly contracted. The child, a boy, survived the operation. The edges of the incisions were united by sutures, and a bandage was applied. Next evening, considerable pain was felt in the hypogastric region, but emptying the bladder by means of the catheter gave instant relief. This was afterwards done frequently with great benefit to the patient. On the 4th Feb. the abdomen was tympanitic, (meteorism), the tongue was dry, and the pulse frequent ; but the lochial discharge continued quite normal. Enemata were ordered, and also the application of ice. Next day there was considerable abdominal tenderness, and vomiting of green matter ; the violence of the vomiting caused three of the sutures to give way, whereby the protruding uterus was exposed at the bottom of the deep gaping wound. Twenty leeches were applied to the tenderest part of the abdomen, cold applications were ordered, and the edges of the wound were brought together by means of adhesive plaster. The enemata formerly ordered having failed in giving relief, an elastic tube was introduced into the rectum, which evacuated a great quantity of gas ; and an enema was thrown up by means of it into the bowels, which caused a copious dejection of fæces. Thereafter the meteorism was diminished,

the pain abated, and the vomiting ceased. A quarter of a grain of morphia was then administered, which induced sleep. The strips of plaster being insufficient to maintain effective union between the edges of the abdominal wounds, the following device was adopted to ensure their adequate support :— Two pieces of wood, having 3 silk threads attached to each, were rolled in long strips of plaster. These strips were so applied, from the lumbar region, that the pieces of wood lay longitudinally on the mesial line, closely embracing both sides of the wound. The silk threads were then tied, and the aperture was completely closed. Next night the patient was easier and free from pain ; the secretions were once more established, and in six weeks after this time her recovery was complete.—*Original paper in Schmidt's Jahrb.*, 1854. *Hft.* vii.

[There were several elements of success in this operation. It is worthy of remark, 1st, that the previous labour had not been protracted, nor was the patient's strength exhausted; and 2d, that the uterus contracted satisfactorily after the extraction of its contents, preventing any hæmorrhage into the peritoneal cavity. The division of the placenta might have been avoided, we think, by previous careful auscultation. The result shows satisfactorily that in young subjects, where the powers of the system have not been exhausted before its performance, the operation of hysterotomy may be resorted to with a tolerable chance of success.]

DR BAZZONI'S CASE OF CAESARIAN SECTION, NECESSITATED BY A SACRAL TUMOUR, FOLLOWED BY ANOTHER PREGNANCY REQUIRING INDUCTION OF PREMATURE LABOUR.

The patient, who was a strong, well-made woman, unaffected by rachitis or any other constitutional malady, was first delivered of twins. Thirteen months thereafter the author delivered her again of a child, which, from its presenting with the shoulder, was extracted by turning and forceps. In her next labour, which happened two years after this, Bazzoni found the diameter of the pelvic outlet contracted to $2\frac{1}{2}$ inches by a tumour firmly attached to the promontory of the sacrum. The cæsarian section was performed, and a dead child, with a small but well-formed head, extracted. The mother, then in her twenty-third year, was bled, and ice applied ; and in 29 days she had quite recovered. Another pregnancy occurred, and the author, examining her in the fourth month, found the tumour increased in size, and the os uteri situated very high in the pelvis. He induced premature labour by rupturing the membranes, and in a few days the patient was quite well.—*Gazz. Lomb.*

PROF. RETZIUS' CASE OF DOUBLE PLACENTA.

This occurred in the case of a primiparous woman, who gave birth to a single child. Each placenta was of half the usual size of the after-birth, and of a spherical form. No connection existed between them, their margins being fully two inches apart. The funis was inserted into neither of the placentæ ; but, about two and a half inches from their approximated margins, the umbilical artery divided, sending a branch to each, and becoming lost in the membranes. Several small venous branches left each of the placentæ and became united at the before mentioned distance from the margin, to a large branch of the umbilical vein. The child was not very strong when it was born.—*Zeitsch. für Geburtsk.*

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

DR CLEMENS OF FRANKFÜRT ON MAINE, ON CAPITAL PUNISHMENT, AND THE MODES OF ITS INFLICTION.

This author has recently published a most masterly essay on this interesting subject ; and after a preliminary account of capital punishments, considered his-

torically, he proceeds to state the cases which he regards as deserving their infliction. These are the following :—

Crimes deserving capital punishment.

1*st*, Premeditated and cold-blooded murder, whether committed by the hands of an individual himself, or by him through the instrumentality of others.

2*d*, The murder of relatives—as parents, brothers, sisters, husband or wife—even where there has been no premeditation.

3*d*, Infanticide. This crime he considers deserving of death in *all* cases when committed by the *father*. When the mother is guilty, he considers the degree of punishment should be of a three-fold character, according as the crime has been committed before, during, or after the birth of the child.

4*th*, Unpremeditated murder, when committed under circumstances of great brutality and cruelty.

5*th*, High treason, in the highest degree ; and all attempts upon the life of a sovereign, or ruler of a state. Treason of a lower order, however, he considers undeserving of capital punishment, being a sort of political fanaticism.

6*th*, Duels, with fatal terminations, when it is ascertained, upon judicial investigation, that all the requisite forms had not been complied with.

7*th*, As to military offences, he expresses no opinion, seeing that they are not breaches of the civil law, nor are they tried in criminal courts.

8*th*, Arson, when perpetrated with the design of taking away life. Clemens recommends the greatest caution in admitting a plea of pyromania.

He considers that modern psychology has thrown little new light on the homicidal mania described by Esquirol. In latent insanity (mania sine delirio) men are irresistibly impelled to commit the wildest and most savage acts, in the face of all law and propriety, and often with apparent deliberate premeditation. Hence Rousseau said, "Aucun intérieur humain n'est si pur qu'il ne recéle quelque vice odieux." The functions of the brain, like those of other organs, may undoubtedly often become sympathetically deranged very suddenly, and without the manifestation of any premonitory symptoms. Individuals who are truly thus deranged in mind are not entitled to legal punishment.

Occasionally there occur cases of reciprocal murder, (*wechselmord*) when two individuals, generally lovers or married people, mutually agree to destroy one another. The females in these cases, usually exhibit most firmness. In the event of one surviving, Clemens thinks that the punishment of death should not be inflicted when it is ascertained judicially that the assent of the one who has perished had previously been freely given.

Our author next proceeds to consider the

Crimes undeserving of capital punishment.

1*st*, All impure and licentious actions, the so-called "carnal sins."

2*d*, Crimes against private property, when no man's life is thereby endangered or meditated.

3*d*, Manufacturing or circulating false coins, bills, or paper money.

4*th*, All offences originating from fanaticism or religious intolerance, enthusiasm or proselytising.

Clemens repudiates the extension of punishment to the family of the executed man, considering that as the taking away man's life is the greatest of all earthly punishments, society should be content with the infliction of it, and should not injure his innocent family, by confiscating their goods or otherwise.

As to the best mode of carrying into effect the extreme penalty of the law, he is clearly of opinion that public morality is not improved by witnessing public executions ; and he thinks that death should be inflicted in the quickest and least painful way. In Prussia (according to the statute of 1st July 1851), the execution takes place in a closed room, in presence of a clergyman, five officials, and twelve respectable members of the parish. At

the conclusion a bell sounds, to announce to the world that all is over. A similar mode is adopted in Oldenburg and in Würtemberg. No exhibition of the condemned convict should be allowed to gratify morbid curiosity, and only his near relatives, and his spiritual adviser should be allowed to see him. The respite between the sentence and the execution should be as short as possible. Mental and corporeal sufferings should both be abbreviated to the utmost, and all the aggravations of capital punishment, devised and practised by the old criminal law, should be discarded as superfluous and inhuman.

Soemmering has asserted that hanging is the mildest kind of death, but this seems very doubtful, and Clemens greatly prefers the guillotine. The Italian "manaja," and the "doloire of Toulouse," are merely modifications of this instrument. Guillotin, in inventing the instrument named after him, endeavoured to devise a means whereby life might be taken away painlessly. The statement which he made, however, that in death by the guillotine, "the delinquent merely felt a slight cold sensation on the neck," seems to be much exaggerated. The instrument is preferable to decapitation by the sword, as in the former, the axe falls by mechanical means, and in the latter, feeling or nervousness may render the executioner's hand unsteady, so that, (as in the case of Mary, Queen of Scots), several blows may be necessary to sever the head from the body.

Oelsner and others, in recent times, have proposed the use of hydrocyanic acid as a means of destroying the life of condemned convicts; but Clemens justly asks what criminal would willingly drain off the poisoned goblet?—*Henke's Zeitsch. für Staatsarzneikunde, Bd.* 34, *Hft.* 1. 1854.

SCHROFF ON ACONITE, IN A PHARMACOGNOSTIC, PHARMACEUTIC, AND TOXICOLOGICAL POINT OF VIEW.

One of the most complete and elaborate monographs which has yet appeared on this subject has recently been published by Professor Schroff of Vienna. Its extreme length precludes us from attempting an analysis of its contents; we therefore merely give the general results arrived at by the author, referring such of our readers as are interested in the subject to the original valuable memoir.

1. For pharmacognostic and pharmaceutical purposes, all the blue-flowering aconites may be reduced to two principal varieties, viz., the aconitum napellus (*Linn.*), and the aconitum variegatum (*Lamarc*), which includes the aconitum cammarum.

2. The plants of the *aconitum napellus*, and its sub-species, are much more active in all their parts than those of the *aconitum variegatum*. This is true of the plants, both in their wild and in their cultivated states, and whatever may be the locality of their growth.

3. The wild plants of both species possess more of the active principle than those that are cultivated. The *aconitum storkianum*, as cultivated in Reichenbach's gardens, is possessed of very little activity, as has been proved by physiological experiments performed with the plant, both in its fresh and its dry state.

4. The active principle exists in all the parts of the plant, but is most energetic in the roots, whether old or young; next to them, in the plant before flowering; and it is least powerful in the seeds.

5. The plant is far more active before flowering than after it, and at this time the root is at least six times more active than the herb.

6. When carefully dried and excluded from damp air, the plant retains its activity for a long time, if its green colour is preserved. But if it be laid aside while damp, it soon loses its active properties in dessication, and becomes of a dirty colour.

7. Extracts obtained by expressing the fresh juice, are less active than those of an alcoholic nature, which contain all the active properties of the plant. The alcoholic are four times stronger than the aqueous extracts.

8. Aconitine represents the narcotic property of the aconite; but the latter contains also an acrid principle, which has never yet been isolated, and which exists in sufficient quantity to occasion an inflammation, throughout a great extent of the alimentary canal. Aconite, consequently, should be placed among the narcotico-acrid poisons and drugs.

9. Aconite, and particularly aconitine, when applied to the exterior of the eye, or when taken internally in sufficient quantity, occasions *dilatation of the pupil*, a result which is in direct opposition to the opinion hitherto entertained and expressed by pharmacologists.

10. Aconite, as well as aconitine, when taken internally, appears to exercise an elective and special action on the trigeminal nerve, and in all the parts supplied by the sensitive branches of this nerve it produces peculiar sensations, generally of a painful character.

11. Aconite and aconitine, when given in sufficient quantity, produce, in healthy men and in rabbits, an extraordinary augmentation of the urinary secretion.

12. Aconite and aconitine both exercise a very depressing effect on the activity of the heart and great vessels, which may either occur immediately or after a brief acceleration of the cardiac movements. This effect is of considerable duration, and consequently differs from that of atropine and daturine, which, when given in larger doses than aconitine, occasion an acceleration of pulse much above the normal standard, which, however, is preceded by a diminution of its force of short duration.—*Prag Vierteljahrschrift für die Praktische Heilkunde*, 1954, *Bd. II.*

[Most of these conclusions have been previously arrived at by Professor Fleming of Cork.]

SCHNEIDER ON THE MEDICO-LEGAL IMPORTANCE OF CICATRICES.

The appearance of cicatrices is important in medical jurisprudence, inasmuch as it depends on the nature of the injury received, and on the sort of instrument by which it was inflicted.

Cicatrices after incised wounds, when these have been properly treated and there has been no deep-seated suppuration, generally present a more or less rectilinear form. But the elasticity and stretching of the skin, the convexity of the subjacent parts, and the softness of the cellular tissue, may change a rectilinear cicatrix into an elliptic, or even into a circular one. *Cicatrices from stabbing instruments* are often of a triangular form. Those from *gun-shot wounds* are generally of a rounded or grooved character. If the shot has been received from some distance, there is a considerable mark (*scheibe*) with depression of the skin in the centre, and puckering at the margins; if the shot has been fired close at hand, the cicatrix is deeper, its edges are irregular, and it is of a bluish colour. In wounds of this description, also, the point of entrance of the ball is shown by a depressed, and the point of its exit by an elevated, cicatrix. *Bruises*, when unattended with lesion of the integuments, leave no cicatrix; but when occurring with this complication, they leave cicatrices resembling those caused by incised wounds, or injuries accompanied with loss of substance. *Bites of mad animals* are distinguished by no peculiarity of scar. *Burns* of the first, or minor grade, leave no mark; those of the second grade occasion scars like those of blisters; those of the third grade cause white, glistening, and tolerably smooth cicatrices; and those of the fourth, or most severe grade, leave dense, hard, irregular, contracted cicatrices, with abnormal union of approximated surfaces, and very great retraction of the cellular tissue. *Fluid cauterants* give rise to superficial, and *solid cauterants* to deep circumscribed cicatrices, with contracted centres, and these may in certain cases be variously coloured by the chemical nature of the irritant substance. *Scrofulous ulcers* leave cicatrices which are irregular and furrowed,

with smooth deep depressions, and surrounded by hard uneven margins. The *scorbutic cicatrix* is dark bluish-red in colour, soft to the touch, somewhat elevated, and rather painful; in course of time it becomes flatter, of a reddish-brown colour, approaching to green in the centre, and very thin and easily injured. *Arthritic scars* are of a reddish-brown, a bluish, or an ash-grey colour; they are studded with elevations and depressions; while their circumferences are dark brown, varicose, and often inflamed and erysipelatous. *Herpetic cicatrices* are wider spread, irregular, superficial, and of a dirty reddish-brown colour; in the centre they are often of the same colour as the old skin, and they gradually merge into the surrounding integuments. *Syphilitic cicatrices* are characterised by great loss of substance; they approximate the lips of the deep ulcers before their granulations have had time to reach the surface. *Glandular cicatrices* are irregularly tumified; generally deep, hardened, and of a reddish-brown colour. The cicatrices of *true small-pox*, are flat, and irregular, with indented margins, giving to the surface the appearance of a citron; while those of *pseudo-variola* have smooth even borders, and have hairs growing in them.

As regards the *age of a cicatrix*, we must be guided by its colour, size, and thickness, in forming our opinion. The fainter its colour, the larger its extent, and the greater its sensibility and tenderness, the more recent is its formation, and the more imperfect is its organization. As its age increases it grows smaller, thicker, more shining, and less sensitive.—*Prag. Vierteljahrs. Bd.* ii., 1854.

GÄRTNER OF STUTTGARD ON POISONING BY COMMON COAL GAS.

A case of poisoning by the escape of gas in a dwelling house has been recorded by the above author. The gas affected a lady, her servant-maid, and also an English pointer dog. The lady was first seized; her illness began with an affection of the head, sickness, vomiting, and purging of thin rice-water-like stools, in which whitish flakes were observed. After twelve hours she recovered but felt very drowsy. On the 5th day she experienced pain in the back part of the head, lassitude, vertigo, tinnitus aurium, and loss of appetite, accompanied by a loaded tongue, a small pulse of 90, and cessation of the menses. Blood of rather a dirty dark-red colour, presenting no buffy coat, was abstracted from a vein. Next day the patient was worse; she was quite insensible, and lay with closed eyelids; the eyes were turned up, the pupils were much contracted, and unaffected by the influence of light; the face was red and swollen; there was trismus; the arms were flexed at the elbow-joints; the respiratory movements were very feeble, the pulse was hardly perceptible, and the skin warm and insensible to touch. Venesection having been repeated, and suitable restorative remedies administered, the patient revived in three hours, and was quite well in a few days.

In the servant girl similar symptoms occurred, but not with such severity, which may be attributed to the fact that the atmosphere of her chamber had not been so strongly impregnated with the gas. She had severe cramps of the extremities, great jactitation of the hands, flexion of the arms at the elbows, great restlessness, and inclination to yawn. Her blood presented no buffy coat. Latterly she had a non-febrile bloody diarrhœa. She recovered in fourteen days from the date of her seizure.

The dog was found insensible, and quite stiff, as if dead, but it soon recovered. —*Würtemb. Corresp. Blatt., May* 1853.

CASE OF APPARENT DEATH BY M. ANCELON.

On the 24th December 1830 M. Ancelon was sent for in great haste to visit a person about eight miles off. On arriving he was informed that the patient, a woman 34 years of age, had been dead for more than half an hour. On examination no signs of respiration could be detected,—the pulse could not be felt, the surface was livid, the belly soft and placid, and the extremities cold. On being informed that the woman had been suffering from uterine hemorrhage for a length

of time, and that it had continued up to the moment of her death, M. Ancelon made a vaginal examination. He found in the neck of the uterus the head of a fœtus of four months old, the soft parts were entirely absent. At the moment that he touched the head of the fœtus the midwife thought she perceived a bubble of air at the woman's mouth. Encouraged by this appearance M. Ancelon proceeded to disembarrass the uterus of its putrefied contents, and in proportion as he advanced in this the woman appeared to be restored to life. The respiration, manifested at first by air bubbles which succeeded one another on the lips, and then the circulation became established with extreme slowness. She did not recover her intelligence for more than three days. During this time frequent injections were employed to clean out the putrid matters from the uterus, and ammonia was administered internally. During convalescence she suffered much from an immense number of abscesses over the whole body. Eighteen months afterwards she was delivered of her tenth child.—*Bulletin Général de Therapeutique, November* 15.

[This case shows the importance of an accurate acquaintance with the signs of death. Of these the only one on which confidence can be placed, in very recent cases, is cessation of the beatings of the heart as determined by the stethoscope. The other unequivocal signs, such as cadaveric rigidity, putrefaction, etc., do not occur for a longer or shorter time.]

DUMREICHER'S CASE OF DEATH BY THE INHALATION OF CHLOROFORM.

Professor Dumreicher related the following case at the meeting of the Vienna Society of Physicians, held on the 16th November 1853. A lad, aged 19, was placed under the influence of chloroform, in order to have his anchylosed knee-joint forcibly extended. The patient was laid in the recumbent posture ; and half a drachm of chloroform having been poured upon a sponge placed in a vessel resembling a snuff-box, the anaesthetic was held to his nostrils, in such a manner as to allow of free respiration by the mouth. The inhalation was continued for 50 seconds, and the patient had spoken a few seconds previously, when Professor D. observed that the pulse had become frequent and undulating. He immediately suspended the administration of the anaesthetic. Trismus occurred ; the respirations became irregular; the face turned livid, and he foamed at the mouth. Insufflations of air, aspersions of cold water, compression of the thoracic walls, frictions, irritating vapours to the nostrils, and latterly the abstraction of ʒviij. of blood from the external jugular vein, were all tried in vain. In a quarter of an hour he was dead, and during this space of time a feeble attempt at inspiration was once observed. The patient had been much addicted to masturbation, and had indulged in that vice shortly before the operation. On *post-mortem* examination there were found no anomalous appearances, except some hemorrhagic erosions of the stomach. The chloroform was analysed, and found to be quite pure.—*Zeitsch. der Gesells. der Aertze zu Wien.* 1854, *Hft.* 2.

Part Fourth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

Meeting XI.—*April* 8, 1854. Dr Bennett, President, in the Chair.

NEW MODE OF PREPARING SKELETONS.

Dr Cobbold showed, in the name of Robert Daw, Esq., who was present as a visitor, specimens of the skeletons of small animals, which had been prepared

by a gentleman in a peculiar manner. The bodies of the animals had been put in pools of sea-water, where the soft textures had been removed by the action of the water, and of the minute animalcules, chiefly crustacean, which inhabit these pools. The skeletons were remarkably white and clean; but it is plain that detached bones or portions of bone would be very apt to be lost by this mode of preparation, and the skeleton thereby rendered incomplete.

CANCER OF THE PERICARDIUM.

Dr Haldane showed a specimen of cancerous deposit on the pericardium covering the left ventricle of the heart. The deposit was situated at the base of the ventricle, and extended upwards on the external surface of the aorta and pulmonary artery. Cancerous nodules were found in the liver, lungs, spleen, and kidneys of the same subject. Dr H. remarked on the great variety of urinary cancerous affection of the pericardium ; in most cases of cancer of the pericardium, the deposit had commenced in adjoining organs and involved the pericardium by extension ; but in this instance the cancerous exudation had affected the pericardium directly.

Dr Bennett pointed out that the fat was abundant on the surface of the heart, as confirmatory of his opinion, that the fatty is connected with the cancerous diathesis.

Dr Gairdner alluded to several preparations in museums, illustrative of cancer of the pericardium and endocardium, and expressed his doubts as to the true cancerous nature of the deposit in certain specimens which had been put up as cancer of the endocardium and valves of the heart in some of the London museums.

CUTANEOUS ERUPTIONS.

Dr Gairdner gave some details of a case of cutaneous eruption closely simulating small-pox, which had been produced by large doses of hydriodate of potash. The eruption had been brought out several times in succession by the renewed administration of the medicine. The case was one of syphilitic disease.

Dr Gairdner also related a case of *scabies*, where an attack of small-pox came on soon after the patient came into hospital. During the attack, the scabies disappeared, and remained away for three weeks after recovery from the small-pox, when the eruption of the itch again returned to a small extent.

Dr Bennett considered that in this case, the itch insects had been killed during the attack of small-pox ; but that their ova which had not been destroyed, had reproduced the disease after the recovery from that disease.

LIVER AND BRAIN OF THE GIRAFFE.

Dr Cobbold exhibited two plaster casts of the liver and brain of the giraffe, and made the following remarks :—

At a former meeting, when several incomplete dissections of the viscera and other structures from the body of a male giraffe were brought before the society, I stated my intention of communicating, at a future time, any additional details which a further examination of the parts might suggest. To-day I have placed on the table a series of preparations containing each a minutely injected specimen of the fasciola gigantica described at the last meeting. As the large size (relatively) and semiputrid condition of the liver rendered its preservation inexpedient, I have retained only a cast of it; this, I have brought with me, to show the position of the orifices of the two primary divisions of the common bile duct, whence the entozoa were obtained. Here is also a cast of the brain, which organ was only removed from the cranial cavity piecemeal, as it was intended to preserve the cranium entire. The cast, though not perfect, illustrates the small size of the organ as a whole, its flattened form, and the relative disparity between the cerebrum and cerebellum; the latter, it may be observed, is in no degree overlapped by the former, the cerebellum being placed immediately behind, and on the same level.

PROFESSORS KÖLLIKER AND BENNETT ON THE DISCOVERY OF LEUCOCYTHEMIA.

Some time ago we prepared a lengthened reply to a letter addressed to us by Professor Virchow of Wurzburg, concerning his claims to the discovery of leucocythemia. The result of our investigation of those claims at that time was, that Professor Virchow had purposely misrepresented the published writings of Dr Bennett, or that he was profoundly ignorant of the English language. In either case we considered that the discussion of the subject would little interest our readers, and would be of no benefit to the cause of medical science. Hence, we contented ourselves with the statement, which we took occasion to make when noticing Virchow's Hand-book of Pathology. Having, however, been requested by Professor Kölliker of Wurzburg to publish the following letter, we do so, together with Dr Bennett's reply, which renders any further comments from us unnecessary.

To the Editor of the Monthly Journal of Edinburgh.

SIR,—Amongst the notices of new books in the June number of the *Edinburgh Monthly Journal*, there is one (p. 547 to 569) which, professing to make mention of the Manual of Pathology and Therapeutics now in course of publication under the editorship of Professor Virchow, is chiefly taken up with an account of the history of leukæmia, and in which, occasion has been taken to cast several personal reflections upon the professor of pathology at Wurzburg.

To that gentleman I must leave the right of answering such personalities, whilst on my part I feel myself called upon to lay before the English scientific medical world, the history of this discovery as understood in Germany, in order that an unprejudiced judgment may be formed upon the relative merits of the authors who have occupied themselves with this question.

The first observations on leukæmia (Virchow, or Leucocythemia of Bennett), occur in the year 1845, and take their origin from a case of disease of the spleen observed by Dr Craigie, in which a microscopical examination of the blood was made by Dr J. Reid. This case was not immediately published, but shortly after, Craigie having been present at the examination of a similar case by Professor Bennett, both cases appeared in the October number of the Edinburgh Medical and Surgical Journal for the year 1845 (see p. 400, "Case of disease of the spleen in which death took place in consequence of the presence of purulent matter in the blood," and p. 413, "Case of hypertrophy of the spleen and liver in which death took place from suppuration of the blood.") From Craigie's case it appears that the connection between a certain diseased state of the spleen and a changed condition of the blood was clearly recognised ; but that, on the other hand, this condition of the blood was not correctly interpreted. In the second case (Bennett's) nothing further was elucidated, for although Bennett happily remarked that the colourless cells observed in the blood might be taken for the colourless blood corpuscles themselves, yet he objects that "with regard to the colourless corpuscles of the blood we know of no instance where they existed in the amount, or ever presented the appearances described." Rejecting this supposition, therefore, he then declares the corpuscles in question to be pus-globules, and as no signs of phlebitis could be found, comes to the conclusion that the spontaneous formation of pus (so called zymosis) had taken place in the blood. The communication of Professor Virchow appeared in Froriep's Journal (Froriep's neun Notizen) of November 1845, a month after the publication of Bennett's case. Virchow's interpretation of the morbid appearances differs so widely from that of Bennett, that it is a matter of perfect indifference whether the one or the other were first published, though, indeed, I may be allowed here to ask whether this October number of the Edinburgh Journal really appeared on the first of October, or (as often happens in the periodical literature of Germany) at a later date. Be this as it may, it is quite

certain that Virchow knew nothing of the publication by Bennett, or he would not have allowed this opportunity of combating the views entertained by Bennett to pass by unemployed. Virchow expressly declares the colourless cells observed in the blood to be identical with the colourless blood corpuscles, and gave to this form of disease the name of white or colourless blood. At the same time, he remarks that the pus-globule is not distinguishable under the microscope from the colourless blood globule. When Virchow nevertheless declared the cells found in the case under examination to be colourless blood cells, he did so on the express ground that no signs of inflammation in the veins were any where discoverable, and that it was contrary to his pathological views to entertain the hypothesis of any spontaneous formation of pus in the blood. Virchow further rejected the assumption of an existing pyæmia, on the ground that this disease is not characterised by the presence of pus in the blood, but rather by a decomposition and melting together of its component parts, as also by its tendency to become exuded and to undergo purulent metamorphoses.

As regards the specific causes of leukæmia, Virchow expressed himself in this his first communication with becoming reserve; nevertheless, he distinctly points out as primary, the condition of the spleen, and entertains the idea of an increased formation of the colourless blood cells. In the year 1846 various cases of leukæmia were observed in England (Fuller, Chambers, Walshe, Douglas), but though valuable contributions (for instance the diagnosis by Fuller in the living subject) still nothing further of importance in respect of the peculiar nature of this disease was elucidated. On the other hand, Virchow in the same and following years (Medicinishe Zeitung, Berlin, 1846, August and September 1847, January), made great advances; having become in the meanwhile acquainted with the published cases of Craigie, Bennett, and Fuller, and compared their results with his own, he combats most positively in his communications of August and September, the view put forward by Bennett, that this affection had its origin in the formation of pus in the blood, and expresses still more decisively than formerly his opinion that the whole disease consisted in an increase of the colourless blood cells of the blood, thus distinctly claiming for it a name and place in pathology.

In his article of January 1847, Virchow brings forward from the literature of an older period (which he had meanwhile searched through), nine cases which might be referred with more or less probability to leukæmia, and discusses in detail the question of the connection of this disease with, and its relation to the spleen. Although this relation to the spleen had occurred to Craigie and after him to Fuller, yet neither of these gentlemen nor Bennett could have attained to the conviction that it had any near connection with an alteration of the blood, inasmuch as they looked upon the affection as a spontaneous formation of pus. Virchow on the other hand, who from the commencement had maintained the doctrine of an increase of colourless blood cells, stood on more favourable ground, and basing himself upon the well known views of Hewson and Donnè regarding the function of the spleen in the preparation of the blood, readily and naturally came to the further conjecture, that from an altered relation of the spleen to the blood cells during their formation, might be derived the conditions under which leukæmia would be produced, and thus the first step towards clearing up this interesting affection was made.

In an article in the Archiv. of Path. Anat. of the same year (1841), Virchow describes a new form of leukæmia (herein he has also the priority), in which the spleen presented no changes whatever, whilst the lymphatic glands were enormously enlarged, and now he declares in direct terms (page 570, Archiv. of Virchow and Reinhard, 1847), that leukæmia proceeds from a primary affection of the spleen and lymphatic glands, as a direct consequence of which an increased development of colourless blood cells takes place; and adds further (page 571), that the physiological functions of the spleen and lymph stand as eliminating organs of the blood, the significance of which had hitherto

been so hypothetical, was now clearly established. In addition to these articles Virchow followed up the subject further in the 2d vol. of his Archiv., page 587; as also in the 2d vol. of the Transactions of the Med. Soc. of Wurzburg, page 325, in which latter place he draws a distinction between lienal and lymphatic forms of leukæmia.

Four years after Virchow had correctly interpreted the nature of these morbid appearances, and arrived at a correct appreciation of the physiological importance of leukæmia, after full investigation and examination of numerous cases, partly collected from authors, partly falling under his own observation—we find Bennett again in the field. In the year 1851, (*Monthly Journal*, numbers for Jan., April, Aug., Oct.; and in the year 1852, in a separate work entitled Leucocythemia, or white cell blood), Bennett, without alluding in any manner to his former views of the nature of leukæmia, and without the slightest notice of the labours of Virchow, or the results obtained by him, now adopts in full the opinion and views of that gentleman, with the single and sole exception of the name which he changes to leucocythemia.

Although the general doctrine of leukæmia is not in reality advanced a single step by this second performance of Bennett, no one will deny him the merit of having contributed very materially to the proper understanding and elucidation of this disease by his bringing into one collective and connected view the cases hitherto observed, and by his production of fresh material, as well as the institution of chemical examination of the blood (which latter had been undertaken by Vogel and Robertson), and especially by his zealous advocacy and defence of views and doctrines which Virchow had first established.

I might here close the discussion of the history of leukæmia, but am unwilling to pass unnoticed the last and most important work of Virchow in the 5th vol. of his Archiv., wherein six new cases with minute chemical analyses of the blood, by Professor Scherer, are detailed, and in which Scherer found lactic, acetic, and formic acids, with hypoxanthin and gelatine. In this article the general bearings of the whole question are fully entered into, and attention particularly directed to the tendancy towards hæmorrhage so often observed in this complaint; and lastly, Virchow defends upon most sufficient grounds the correctness of the name leukæmia which he had given from the first to this disease.

The several investigations of Virchow and Bennett thus fall in the following chronological order.

October 1845, Craigie and Bennett—First microscopical examination of the blood in leukæmia explained by the authors on the assumption of purulent formation in the blood.

November 1845, Virchow—Describes and explains, under the name of leukæmia, a disease of the blood presumed to be an increased development of white blood cells, as no inflammation of veins was observed; and the supposition of a pyæmia was opposed to pathological views of the author.

1846, ——— Several cases of leukæmia observed in England, and diagnosed during lifetime of patient. Theory of zymotic origin of leukæmia advanced by Bennett, combated by Virchow, who enforces his own doctrine of an increase of colourless blood globules.

1847, ——— Virchow describes cases from the elder literature together with those observed in England, and others falling under his own notice—one of the latter occurring without hypertrophy of the spleen, but with enlargement of lymphatic glands, and followed by the distinct announcement that leukæmia was to be considered a disease of the spleen and lymph

glands, followed by development of white blood globules, and that to these organs is to be ascribed the physiological significance of eliminators of the blood globules.

November 1851-52, Virchow—Bennett gives up his former views of leukæmia as a suppuration of the blood, adopting the explanation of Virchow, without mention of his name, or allusion to the physiological function ascribed by Virchow to the spleen and lymph glands, and gives the altered name of leucocythemia to this affection.

After this statement it would appear unnecessary to point out that Virchow was not only the first (as Bennett indeed admits in Monthly Journal for June of this year), who declared the colourless cells of leukæmia to be identical with the colourless globules of the blood, but that he was also the first (which Bennett disputes), to whom a "correct generalization and elaboration of the whole subject" is to be ascribed.

Without Virchow's investigations it is very possible that Bennett might still look upon leukæmia as a formation of pus in the blood. In any case science is indebted to Virchow alone for the earliest correct explanation, and intelligible theory of this disease, as also its bearings upon physiological functions of the organs concerned—(from the year 1845-7).

From Professor Bennett, therefore, we expect that his sense of justice will lead him to a due acknowledgment of this point, whilst on the other hand no one will refuse to acknowledge the great credit which is due to him for the services rendered by him in the investigation of this disease.

With the request that you will permit the publication of this historical statement in the Edinburgh Monthly Journal.—I have the honour to sign myself your most obedient A. Kölliker.

Wurzburg, *August* 25, 1854.

REPLY TO THE LETTER OF PROFESSOR KÖLLIKER, BY PROFESSOR BENNETT OF EDINBURGH.

Professor Kölliker of Wurzburg having undertaken to lay before the English scientific medical world the history of the discovery of Leucocythemia *as it is understood in Germany*, I think it due to his scientific character and to the temperate tone of his communication, to correct the errors into which he has been led. In reply to his appeal to my sense of justice, I request from him a careful perusal of the following statement, in the confident belief that he will take some opportunity of informing such of his countrymen who may feel interested in the subject what are the real nature of my claims to the discovery in question.

It is said by Professor Kölliker that the first observations on this subject occur in the year 1845, and take their origin from a case of disease by Dr Craigie, etc., etc. Now, the fact is, that Dr Craigie's case occurred in 1841; and it is admitted by Dr Craigie himself that it would not have been published even four years afterwards but for the occurrence of mine. He says, "I kept it unpublished from the period at which it took place; and it is published at this time, chiefly because of the occurrence of a case in many, if not in all respects similar, to another physician in the same hospital, led me to anticipate similar results, and went far to confirm my conclusions deduced from the first case."—*Edinb. Med. and Surg. Journ.*, Vol. LXIV. p. 402.

Professor Kölliker takes great pains to show that Dr Craigie and myself held the same opinions as to these cases, and that in mine, which followed his, "nothing further was elucidated." On the other hand, he says Professor Virchow was the first to point out that "no signs of inflammation in the veins were

any where discoverable," etc. Now, exactly the contrary of this is the fact. Dr Craigie put forth two possibilities as to the cause of the blood disorder. 1*st.* He says, "It is barely possible that some *inflammatory action* had taken place in the tributary or constituent veins of the mesenteric trunks ; and that the *purulent matter and lymph thus formed* had been conveyed into their interior with the blood, and thence into the *vena cava*, heart, and vessels of the brain." 2*d.* He says, "Another opinion occurred to me, however, as more probable, and which various circumstances in the case induced me to regard as the most correct. Considering that the spleen had been for some time, that is, for several weeks, in a state of *chronic inflammation*, and taking into account the large vessels with which this organ is connected to other organs, it appeared to me that this *inflammatory process*, which had been continuing so long without abating, subsiding, or being subdued, was at length beginning to give rise to the formation of lymph and purulent matter, and that these substances, as they were formed, were immediately taken into the veins, and thus circulating with the blood, gave rise to the peculiar assemblage of symptoms which the patient presented during the few days preceding his death." (P. 409). From these extracts it must be clear that Dr Craigie considered the blood disease as secondary, and dependent on the absorption of pus from an inflammatory lesion either in the mesenteric veins or spleen.

The view taken up by myself was wholly different, viz. that the blood disease was primary, originating in that fluid itself, altogether independent of local inflammation, and especially unconnected with inflammation of the veins. This will appear from the following extracts from my paper :—"In the present state of our knowledge, then, as regards this subject, the following case seems to me particularly valuable, as it will serve to demonstrate the existence of true pus formed universally within the vascular system, *independent of any local purulent collection from which it could be derived.*" (Pp. 413, 414). And again, "pus has long been considered as one, if not the most characteristic, proof of proceeding acute inflammation. But in the case before us, what part was recently inflamed? *There was none.* Piorry and others have spoken of an inflammation of the blood, a true hematitis; and certainly if we can imagine such a lesion, the present must be an instance of it. But it would require no laboured argument to show, *that such a view is entirely opposed to all we know of the phenomena of inflammation.*" (P. 421) From these passages it must be clear that I then separated the state of the blood from pre-existing inflammation in any of the tissues, which had not been done by any preceding author. Thereby I established a new blood disease—one of a primary nature. I carefully described all the facts, which Virchow has only subsequently confirmed. I spent three entire days investigating the histological character of all the tissues in the body, and in demonstrating the important fact, that the colourless corpuscles in the blood were unconnected with inflammation. Notwitstanding all this, Professor Virchow has wilfully and pertinaciously endeavoured to persuade his countrymen that I regarded the case as one of ordinary pyæmia or purulent absorption; and Professor Kölliker, in his present communication, says of these laborious researches, that "nothing further was elucidated" beyond what had previously been determined by Craigie and Reid.

Here, it should be observed, that Dr Craigie was the editor of the Edinburgh Medical and Surgical Journal in 1845—that he read my paper before his own was written, and placed his own case first in the October number. Further, Professor Kölliker should be made aware that Dr Craigie was no histologist, and had never employed the microscope in the investigation of disease. To argue, then, that the discovery of this condition of the blood—a discovery altogether dependent on histological research—was made by him, seems absurd in the extreme. But it may be maintained that this part of the inquiry was carried out by Dr John Reid, because he stated in the register, kept by him as pathologist of the Infirmary, that the blood "contained globules of purulent matter and lymph." The few words now quoted constitute literally the whole of Dr Reid's observa-

tions on the matter. They would have been buried in oblivion, if I myself had not found them in the register of dissections, pointed them out to Dr Craigie, and indicated their importance. I have frequently conversed with Dr Reid himself on the subject, who had forgotten the circumstance of having examined the blood microscopically in Dr Craigie's case, or of having made a note of it. Certainly he paid no more attention to it, or in any way thought it more important than a host of other notes he made, which still exist in the pathological register, and in which some future controversialist may doubtless find many similar discoveries, as yet unknown. At all events, it is certain that neither Dr Craigie nor Dr Reid ever imagined to themselves that the "globules of purulent matter and lymph" seen by the latter originated independent of purulent absorption, or ever dreamed of claiming for themselves the discovery of leucocythemia. Who then did make it? Certainly not Virchow, who with Kölliker, in order to depreciate the value of my observations, claim it for these gentlemen. And if none of the three made it, the inference undoubtedly is, that the discovery belongs to me.

What then, it may be asked, does Professor Kölliker claim for his colleague? It cannot be the discovery of the facts, or of the existence in large numbers of colourless corpuscles in the blood, independent of inflammation. All the histological facts—the white appearance of the blood (white blood), its independence from inflammation, and its separation from all previously known pathological conditions—were described by me in the paper of the 1st of October 1845; whereas it is certain that Virchow's short and comparatively imperfect histological description of a case of white blood (the white appearance of the blood being the chief point he dwelt upon) was printed in the second number for the following November, although from the admission of Professor Kölliker, as to the practice which prevails in Germany, the actual period of its publication may have been much later. Hence all that can be claimed for Virchow amounts to this, that he puts forth *an opinion* regarding these facts different from mine, but the possibility of which I clearly indicated. For having described the peculiarities of the blood—the white coagulum, its structural characters, the colourless corpuscles, their relation to the red ones, and the absence of the inflammatory appearances in every tissue, not excepting the veins—the questions remained, What are these corpuscles? How are they produced? In reply, I remarked that "with regard to the colourless corpuscles of the blood, *we know of no instance* where they existed in the amount, or ever presented the appearance described." From this passage Professor Kölliker draws the inference that I denied that these bodies were the colourless corpuscles of the blood. But I need scarcely point out that the passage does not fairly bear that construction. On the other hand, it clearly shows that the possibility of their being these colourless corpuscles was fully entertained. At that time the whole subject was histologically new; and having shown that the cells observed closely resembled those of pus in their structural and chemical characters, I said so, and concluded they were pus corpuscles. But having also demonstrated that they could not have been derived from any inflamed tissue, it only remained to be concluded that these bodies were formed in the blood system itself, constituting a primary suppuration of the blood. Here, I contend, was the real discovery, which was at that time quite new, and remains up to this hour, in my belief, a correct generalization.

Whilst Professor Kölliker seems to attach no importance whatever to my careful histological examination of the blood and of the tissues, and wholly disregards the fact I was at so much pains to establish, that the colourless corpuscles I described, were not dependent on inflammation, he thinks it of the greatest importance that Virchow should have stated that these corpuscles were not those of pus. To me it has always seemed of little importance by what name these bodies were designated, so long as the facts regarding them were described with exactitude. It cannot be denied that I first discovered and described them, and pointed out their origin in the blood itself. What histolo-

gical difference there can be between pus cells independent of inflammation, originating spontaneously in the blood, and the colourless corpuscles of that fluid, I am at a loss to imagine. Yet this is the only distinction which Virchow made. But, what are pus corpuscles but cells presenting certain physical characters originating in an exuded blood-plasma? and what are the colourless corpuscles of the blood but similar cells originating in a plasma contained in the blood glands? I have yet to learn that there is any true histological difference between them; I believe still that the only distinction is that the same corpuscles originate in blood-plasma, sometimes outside, and sometimes within the blood system. If so, the controversy raised by Virchow, and maintained by Kölliker, is wholly one of words. Here I may mention, that, acting on the persuasion that the two kinds of corpuscles, hitherto separated, are really identical, I opposed the generalization of Mr Henry Lee, which set forth that pus brought in contact with living blood caused its coagulation. In conjunction with Professor Barlow of the Veterinary College, I injected considerable quantities of pus into the veins of an ass, in order to determine this point. I thus increased the colourless cells in the blood of the animal without producing any coagulation or inflammation whatever.—(*Monthly Journal*, January and March, 1853, pp. 80 and 272, 273). Moreover, it may be questioned, and indeed it has been questioned in a communication which I received from Professor Gluge of Brussels, and in an article by Dr Radcliffe (*Half-yearly Abstract of the Medical Sciences*, Vol. XVI. pp. 295), whether this distinction can have any real foundation, and if it have not, then the pretended discovery of Virchow sinks into nothing, as it is not founded on fact, but simply on opinion.

As to the subsequent progress of this inquiry, I have only to express my astonishment at the statement made by Professor Kölliker, that in 1851 in the *Monthly Journal*, and that in 1852, in my separate work, I made no allusion to my former views, and did not take the slighest notice of the labours of Virchow. It is most untrue. My views regarding this disease have always been the same, but never such as Virchow and Kölliker have represented them; and so far from denying the labours of the former pathologist, I have fully set them forth, and quoted all his facts and observations. I always have and still continue to estimate highly the value of the facts he has contributed in connection with this important subject. But what he has accomplished does not entitle him to the original discovery of leucocythemia, or to giving it a place in pathology.

Careful investigation into this subject will, I am satisfied, convince the candid inquirer that the discovery of leucocythemia and the subsequent progress of ideas regarding its nature may be divided into three epochs or stages as follows:—

1. PROFESSOR BENNETT.—*October 1st* 1845. Discovery of a new morbid condition of the blood, consisting of multitudes of colourless corpuscles, resembling those of pus, associated with hypertrophy of the spleen and liver, and presenting after death peculiar white coagula. Shown to be unconnected with inflammation in any of the tissues, and especially unconnected with phlebitis. Attributed to the development of the corpuscles in the blood itself.

2. PROFESSOR VIRCHOW.—*Series of papers from the 2d or 3d week of November* 1845 *to* 1847. Confirmation of the preceding facts, but the corpuscles said to be an increase in the colourless cells of the blood. New cases, and especially one of great value, in which a similar condition of the blood was associated with enlargement of the lymphatic glands without hypertrophy of the spleen. Origin of the colourless cells attributed to the lymph glands; proposed name of leukhemia or white blood.

3. Professor Bennett.—*Series of papers*, 1851. Systematic view of the whole subject. Additional facts and cases, with chemical analyses of the blood. Doctrine that the lymphatic and other ductless glands secrete the blood; proposed name of leucocythemia or white-cell blood, and the relation of this disease to other pathological conditions, and to practical medicine pointed out.

From this view of the case, it will be seen that I am far from undervaluing or wishing to hide Professor Virchow's contributions to the pathology of leucocythemia—whereas he, in order to make it appear that the origin as well as development of the whole subject was due to himself, has not hesitated to give the most erroneous and partial accounts of my facts and views. I very much regret that the note published in his Archives, dated August 1852, should have contained such injurious and insulting expressions. Had it been more temperately written, I should at once have replied to it, and so endeavoured long ago to place this controversy on its proper basis. I must be excused, however, from entering into literary warfare with revolutionary combatants, whose chief weapons are detraction and attacks on character. But since Professor Kölliker has descended into the arena as a champion for his colleague, I have no longer any objection to enter the lists with such an opponent—trusting that one so distinguished in the field of science, like the chivalrous knight of old, will conduct himself not only *sans peur*, but also *sans reproche*.

THE CHOLERA.

During the past month, the early part of which was very sultry, the cholera has been prevalent in many parts of England, Scotland, and Ireland. It has appeared, generally speaking, in the same localities, and been propagated by the same mysterious laws as during former epidemics. In London considerable panic was created by the extension and fatality of the disease, as upwards of 3000 individuals died of it in that city alone during the second week of September. We regret to say that continued experience, and the most anxious research have failed to throw any new light either upon its pathology or treatment, and the public continue to be distracted by the empirical remedies, confidently advanced by various members of a profession which boasts of its scientific principles. In some cases it is true, theories have been advanced more or less specious, or rather we should call them pure hypotheses, as in few instances are they the deductions from well-observed facts or careful investigations. But whether professing to be derived from experience or reasoning, all are apparently valueless. Whether we give sulphuric acid with Dr Fuller, calomel with Dr Ayre, sulphur with Mr Grove, castor oil with Dr Johnson, or sulphate of strychnia with M. Abeille, all which remedies have recently been strongly recommended by their proposers, the result is the same. The parasitic theory, the febrile theory, and the eliminative theory are all useless at the bed side. Still, amidst the opposing views and conflicting recommendations which surround him, the medical man, when called upon to act, must decide upon a course of treatment. The judicious practitioner will doubtless be governed by the circumstances of the case, and remembering that our ignorance is great concerning everything that relates to the pathology and treatment of this malady, he will act as his individual knowledge and past experience dictates. These, if we mistake not, will suggest to him the propriety of checking the discharges which so rapidly exhaust the vital powers; administering such stimulants and nourishment as will rally the sinking strength; giving such sedatives as will alleviate the painful spasms; and supplying such artificial heat as will counteract the deadly cold, and restore the languid circulation. The difficulty consists in bringing about these desirable results, and in choosing out of the many remedies that may be employed, those which are most appropriate to the individual case. Notwithstanding our uncertainty on these points, we believe

that our art, fallible as it is, may yet be of the greatest service to suffering humanity, and that an early application to the family medical attendant is the only judicious course for any one attacked to pursue. We deprecate all specifics, all nostrums, all exclusive systems of treatment, and although differences of opinion cannot but be expected to prevail, we have full confidence in the sound good sense of the profession at large, and in its power to do all that the present state of medicine admits, to mitigate this most terrible disease.

VARIETIES.

Birth of the Stethoscope.—It was Laennec's habit, when examining a newly admitted patient, labouring under pectoral disorder, to percute him in every part of the chest, both in front, and at the back, as well as on either side. After which he would apply the ear to any part which had resounded badly or imperfectly. One day (it was the 13th of September 1816, for I took a note of it) Laennec seemed dissatisfied with the result of his percussion and direct auscultation, in an interesting but obscure case before him; when turning to the circle of pupils around the bed, "Why," said he, "should we not avail ourselves of the help which accoustics yield to us, of making distant sounds more audible? The speaking-trumpet enables the dull of ear to hear the faintest whisper; the ascending tube in a warehouse conveys to the upper stories, in audible sounds, the muttered directions of the master below; the ticking of a watch placed at the end of a long beam, is heard loudly by the ear applied to the other extremity; a tube, therefore, applied over the lungs or to the chest over the heart, ought to instruct us more plainly through our ears, with the movements and sounds going on within;" and forthwith snatching the *cahier des visites* from the hands of the nearest *elève interne*, and rolling it up lengthwise to the shape of a cylinder, having a perforation through its axis, he applied it first to one side of the chest, then to the other, and again to the back, between and below the shoulder blades, and declared (what we all know to be true) that he could make out with greater distinctness, than with the naked ear applied over the parts, their inward condition, from their respective sounds which he then described. Such was the birth of that famous instrument, at first denominated by its inventor *pectoriloque*, which, slowly winning its way from the humble hospital, bearing the name of its founder, the unlucky minister of Louis XVI., to every corner of Europe and to the new world, has laid open to the medical man, the scenes of disease enacted within the thorax, which before baffled detection.—*Granville on Sudden Death.*

Pathology and Treatment of Chronic Bronchitis in the 16th Century.—It was believed that the thin fluid discharged was partly serous humour, partly condensed vapour, which descended from the brain into the lungs, not through the cavity of the windpipe, for if so, it would be coughed out during its downward passage, but through its coats, as water soaks through linen. This thin humour and vapour was supposed to be originally drawn into the brain by the increased rarity in the substance of that organ, caused by undue heat. Heat makes all things rare; and rarifaction in one part of the body, to express the idea roughly, produces suction in another. The treatment consisted in attacking the root of the disease, namely, the unhealthy temperature of the brain. With that view the head was to be purged through the palate, the nose and the sutures of the skull, especially the coronal suture. The following admirable prescription is given for procuring a good discharge from the nose:—Take of goats or cow's milk and of water, of each half a pint, mix and dissolve in them two grains of elaterium; let this be drawn through the nostrils, when the patient has an empty stomach.—*Morley's Life of Jerome Cardan.* The most enthusiastic worshipper of the ancients, must, we think, admit that medicine has advanced a little since such doctrine and treatment were in vogue.

Influence of the Physician.—"How many thousand faces must have passed before the doctor's eyes; how many pitiable tales of woe must have been poured into his ears; what awful secrets must find a repository beneath that black satin waistcoat! We may lie to the lawyer, we may lie to the confessor, but to the doctor we cannot lie. The murder must out. The prodigal pressed for an account of his debts will keep one back; the penitent will hide some sin from his ghostly director; but from the doctor we can hide nothing, or we die. He is our greatest master here on earth. The successful tyrant crouches before him like a hound; the scornful beauty bows the knee; the stern worldly man clings desperately to him as the anchor that will hold him from drifting into the dark sea that hath no limits. The doctor knows not rank. The mutilated beggar in St Celsus's accident ward may be a more interesting case to him than the sick duchess. He despises beauty—there may be a cancer in its bloom. He laughs at wealth; it may be rendered intolerable by disease. He values not youth; it may be ripe for the tomb, as hay for the sickle. He makes light of power; it cannot cure an ache, nor avert a twinge of gout. He only knows, acknowledges, values, respects two things—Life and Death."—*Household Words.*

The New Board of Health.—After King Stork, King Log. After a board of health which pleased nobody, we are to have a board of health to please everybody. The new board of health is ready to advise any towns in want of advice, but it abjures any dictation or control. It is to be all gentleness and complaisance. If Mr Chadwick was the vinegar cruet, Sir Benjamin Hall is to be the oil. Which do you like, dirt or cleanliness? is to be the question, and every town is to have its taste and its own way of it. People do not like to be clean by word of command, nor can a water-dog endure to be thrown into the water. Yet it does not follow that the freedom of choice will be followed by the adoption of cleanliness. The towns, dirty and contented, will have no vocation to consult the board about a state so satisfactory to themselves; and the towns which desire to cleanse themselves and live wholesomely, will think they understand their own peculiar circumstances and wants better than they can be comprehended by two or three gentlemen sitting at a board in London.—*Examiner.*

The Medical Council of the Board of Health.—Sir Benjamin Hall soon found that he could not proceed safely without medical advice. He has therefore called into consultation a medical board composed of physicians, surgeons, apothecaries, Mr Owen, and Dr Farr. These gentlemen are nobodies as to the executive, but are to receive a fee when their advice is wanted. On the whole, we have some doubts whether this system will place the profession in the honourable position it should occupy in its relation to Government. In the meantime, six additional medical inspectors have been appointed, and it is said where boards of guardians have neglected their duty in paying or employing sufficient medical men, the Board of Health are determined to enforce the law.

The Provincial Medical Association.—At the late annual meeting in Manchester, it was decided that Dr Cormack, in addition to being editor of the *Journal*, should also act as secretary, with a salary of L.350 per annum. From the long discussions which ensued, it is evident that the metropolitan element; the concentration of offices in London; and the enormous expenditure for the *Journal*, are exciting the lively anxiety of the founders of the institution. On the other hand, it would appear that the association was never so numerous, and future events must determine whether the new or the old policy is the better.

Law and Medicine.—One of the most horrible abuses in the practice of the law, is when a man has been proved to murder another by repeated beatings, to allow an eccentric physician to come forward and confuse the jury, by swearing that the victim died of some disease with a curious Greek or Latin name. We have been present at a trial where one man killed another by tightening and

twisting his neckcloth, and yet a medical gentleman swore that in his opinion the murdered man died of what he called "spinal apoplexy," which might be true in the same way that all people, whether murdered or not, die of want of breath.—*Belfast Northern Whig.*

Irish Appreciation of Histology.—"It is 'refreshing' to meet such a book as this (that of Dr Struthers), sick as we are of microscopical moonshine and transcendental typography; and weary as we are of cells and epithelium, analogues, homologues, and organisms. It reminds us that there really is such a place as a dissecting room, and such people as anatomists, actually cutting up dead bodies; mistaught knaves though they be, to bring a rude unhandsome corse between the wind and histological nobility."—*Dublin Medical Press.*

Honesty not Unappreciated.—Let it be a consolation to the better men among us, that the honesty which confesses the power of medicine to be limited and the skill which protects the patient from unnecessary interference, are not wasted on the thinking portion of mankind.—*Dayman's Address.*

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On some Diseases of Women, admitting of Surgical Treatment. By Isaac Baker Brown, F.R.C.S, Surgeon-Accoucheur to to St Mary's Hospital, etc. London, 1854. 8vo. Pp. 288.

Practical Observations on Mental and Nervous Disorders. By Alfred Beaumont Maddock, M.D.,etc. London, 1854. 8vo. Pp. 236.

Handbuch der Pathologie und Therapie von Dr C. A. Wunderlich, Prof. der Klinik an der Universität, Leipsig, etc. 16te, 17te, and 18te Leiferung, Schluss, Stuttgart. 8vo. 1854. Pp. 1772.

The Ear in Health and Disease, with Practical Remarks on the Prevention and Treatment of Deafness. By William Harvey, F.R.C.S., etc. London. 12mo. 1854. Pp. 235.

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Cholera: An Analysis of its Epidemic, Endemic, and Contagious Character, etc. By Henry Stephens, M.R.C.S.L., etc. Third edition. London, 1854. Pp. 32.

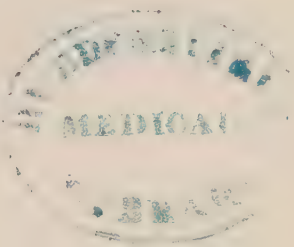
On the Use of Vegetable and Mineral Acids, in the Treatment, Preventive, and Remedial of Cholera, etc. By J. H. Tucker, Surgeon, etc., London. 1854. 8vo. Pp. 32.

Observations on the Nature and the Treatment of the Asiatic Cholera. By James Tucker, M.D., M.R.C.S.E. London, 1854. 8vo. Pp. 49.

Sulphur as a Remedy in Cholera and Diarrhœa. By John Grove, M.R.C.S.L. Second edition. London, 1854. 8vo. Pp. 35.

The Common Sense of Cholera. By a Practical Practitioner. London, 1854. 12mo. Pp. 68.

Our Students' Number.—In our next number we propose saying a few words to the Medical Students of Scotland. We are induced to do this by observing the extraordinary errors which have crept into the accounts of the Scottish Medical Schools in the students' numbers of the London Hebdomidaries, as well as by a sincere desire to prevent that waste of valuable time which too frequently results from an improper arrangement of prescribed lectures and other studies on the part of our young friends.



Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Development of the Blood and Blood-vessels*, being the Prize Essay for 1854 of the Edinburgh Harveian Society. By JAMES DRUMMOND, M.D., Edinburgh.

(*Continued from* p. 226.)

2. *Origin of colouring matter.*—The formation of the colouring matter of the blood corpuscle appears to be more a chemical, than a morphological process. Before therefore it can be determined from what source, or in what manner it is formed, it is necessary to ascertain accurately the various constituents of the blood, as well as the changes produced upon these when exposed to the influence of the different agents which may act upon them in the body. At present, however, such a knowledge is not possessed; it is not known with certainty whether the colouring matter is derived from those substances which pass into the blood for the nutrition of the different tissues, or whether it be formed from the effete matters derived from these. As little does it appear to be known in what part of the body it is formed.

The chief theories as to the source of the colouring matter or hæmatine are two in number. Neither of them, however, appears to be sufficiently supported by facts. The one is that it depends on the iron contained in the blood corpuscle, and this view seems to be strengthened by the generally admitted fact that the administration of iron in cases of chlorosis tends much to restore the blood to its normal condition. It appears, however, that certain other metallic tonics have much the same effect. Moreover, there is no reason to suppose that in the colourless corpuscle also iron does not exist. In the solids of the lymph and chyle, indeed, the proportion of iron appears to be as great as it is in those of the blood. Again, iron is also present in the comparatively colourless blood of the invertebrata. If the colour of the blood corpuscle in the vertebrata depends on iron, the latter substance must, after it enters the blood, exist in a different state of combination from that in which it exists in the lymph and chyle. The researches of Simon and Scherer, however, show that the colour of hæmatine does not depend so much on

the iron it contains, as upon its other constituents. According to these observers all the iron may, by means of concentrated sulphuric acid, be extracted, and yet the colour of the hæmatine remain.

The second theory in regard to the origin of the hæmatine is, that it arises from fatty principles.

The following facts would seem to indicate that the two are at least connected. In the young frog, as already mentioned, blood corpuscles of the first set contain much fatty matter. This exists in the shape of the round and angular granules which have been already described. In these bodies as soon as the colouring matter begins to be formed, the granules begin to disappear, and in the same proportion as the former increases, the latter decrease. So also in the corpuscles of the lympth and chyle, which on entering the blood constitute the white corpuscles, there are also more or less fatty granules present. In the conversion of these bodies into perfect blood corpuscles, their fatty granular matter gradually disappears, its place being taken up by colouring matter. The observations of Weber, already mentioned, that the coloured fatty matter of the yolk is, towards the termination of the embryonic life in the chick, carried to the liver and then goes to the formation of blood corpuscles, also show, if correct, the connection between the hæmatine and fatty matter. Between the colouring matter of the yolk and that of the young blood corpuscles, there certainly exists a great resemblance.

Most of the colouring principles in animals are at least associated with fatty matter. Thus the beautiful colouring matter in many insects, the red colouring principle in the shell of the crab and lobster, while insoluble in water, are perfectly soluble in all the media which dissolve fatty matters, such for example as alcohol or ether. The yellow colouring matter of the corpus luteum in the cow is also of the same nature.

Again, that the formation of the hæmatine is in some way or other connected with the respiration, is probable from the fact that in the frog it begins to be formed about the same time that the circulation in the branchiæ commences, and only after the respiration is fully established, does the colouring matter in the blood appear to be perfectly developed.

3*d*. *Source of the Serum and Fibrine.*—The embryonic corpuscles in the ova of all animals, are imbedded in a fluid which presents much the same chemical constitution as the serum of the blood. It forms the blastema from which the cells of the ovum are developed, and from which they derive the material for their nourishment. It performs the same office to the cells of the ovum which the serum of the blood performs to the different tissues of the perfect animal. The serum of the blood in very young embryos appears to consist merely of a portion of this fluid, just as the embryonic corpuscles constitute the first blood corpuscles, so also the serum of the blood is at

first formed of a portion of the fluid in which the embryonic corpuscles float. According to Prevost and Lebert, it is probable that the disappearance of the granules from the embryonic corpuscles is due to a liquefaction of the same, while the fluid so formed passes through the cell wall by a process of exosmosis, and constitutes the serum of the blood. It seems, however, more probable that the granules in question go to the formation of the hæmatine. In the mammal the serum must be chiefly derived from the maternal system by absorption through the medium of the placenta. As soon as the lymphatic system is formed, the serum of the blood must in the frog, at least, be chiefly derived from this source.

Zimmerman and others, regarding the fibrine as not intended to afford nourishment to the tissues, but as a product of their effete matter, have stated that the blood of the fœtus contains no fibrine. In the chick, however, on the third or fifth day of its development, I have sometimes removed from the heart a small mass of coagulated blood. In the mammalian fœtus pretty far advanced, I have also observed that when the blood taken from the umbilical vessels was allowed to stand for a short time a coagulum formed. This, however, is much looser and smaller compared with the quantity of blood than in the adult, and in the vessels and heart of the fœtus I have not observed firm coagula, such as occur after death in those of the perfect animal. The quantity of fibrine then in the blood of the fœtus appears to be much smaller than that in the blood of the adult, and in very young fœtuses it probably does not exist at all.

The presence of fibrine in the blood of the fœtus cannot be regarded as an objection to the views held by Zimmerman, Hughes Bennett, and others, in regard to the origin of this substance, inasmuch as we must expect that the process of disintegration goes on during embryonic life, in the same manner as in the growing animal, though perhaps in a less degree, and as a result of this process fibrine may be formed, though in smaller quantity than in the blood of the animal after birth.

SEC. II.—DEVELOPMENT AND STRUCTURE OF THE BLOOD-VESSELS.

In what follows on the development and structure of the fœtal blood-vessels, the same arrangement will be observed as in the preceding section. The mode of formation and structure of the greater and smaller blood-vessels will be described, first in the lower, and, secondly, in the higher classes of vertebrate animals. For this purpose the frog has been selected as an example of the lower vertebrata, the embryo of the ox and sheep as examples of the higher.

a. Lower Vertebrata.—In the frog the development of the blood-vessels has been studied by several observers, chiefly by Vogt, Prevost and Lebert, and by Kölliker.

In the tadpole, before leaving the gelatinous-like material in which it is at first enclosed, the branchiæ are observed as two small swellings near the cephalic extremity of the body. When examined at

this period, they are found to be entirely composed of the structures already described as forming the vitellus, and contain no distinct blood-vessels.

In animals about three lines in length, vessels are distinctly visible in the branchiæ with blood corpuscles circulating in their interior. It is difficult to ascertain exactly the mode in which these vessels are formed. According to Prevost and Lebert, the embryonic corpuscles become gradually diaphanous; the globules separate from one another towards either side, leaving between them a central space which is occupied by the vessel. They suppose it probable that there exists in the young frog a membranous expansion around the vitellus, beneath its outer envelope, to which they give the name "hemoplastic membrane," and from which they believe the blood vessels are formed. According to the observations of Kölliker, on the other hand, it would appear that the vessels in question must be formed by the transformation of cells in the manner stated by Schwann. As regards their structure, they appear at first as channels between the embryonic corpuscles: their margins are not straight and parallel to each other, but are more or less crenated from the projection inwards of the embryonic corpuscles, situated on either side. The vessels cannot however be regarded as mere intercellular spaces as some have stated, inasmuch as they have always a membranous wall distinct from that of the neighbouring cells.

In embryos measuring from five to six lines in length, the branchial vessels are much more distinct and the circulation within them more active. The vessels appear to be wider than before, and at the same time their margins are more straight and parallel to each other. The membrane of which their walls are formed is also much more distinct. It presents the aspect of a thin transparent homogeneous texture in which may occasionally be seen one or two more or less oval granular bodies presenting the character of nuclei. These give rise to a thickening in the wall of the vessel at the points where they occur. The existence of these nuclei would seem to indicate that the membrane constituting the wall of the vessel had been formed by the amalgamation of cells.

In the tail of animals about this period we can also observe the circulation partly established. It presents the following appearance: along the side of the chorda dorsalis we find one large vessel extending downwards from the cephalic extremity towards the end of the tail; here it becomes continuous with another large vascular trunk running parallel to it. In the former the current of the blood is from the head towards the tail, in the latter it is in the opposite direction. In the former also the rapidity with which the blood circulates is also seen to be greater than it is in the latter. The one of these vessels corresponds to the descending aorta, the other to the ascending cava. They are connected at different parts of their course by intermediate smaller vessels. The latter when traced from the artery are seen to pass outwards for a little distance to-

wards the circumference of the tail; they then bend upon themselves, and again passing inwards towards the chorda dorsalis open into the vein. These intermediate vessels thus form a series of lateral vascular arcs along each side of the chorda dorsalis.

As regards the structure of the vessel, the two large vessels are so imbedded among pigment cells that it is difficult to observe clearly the elements of which they are composed; so far, however, as this can be done, they appear to consist of spindle-shaped corpuscles, some arranged longitudinally in the long axis of the vessel, others more or less transversely, and constituting several layers. Towards the point where the two become continuous, we find that they present the appearance of being composed of a homogeneous membrane. From what we know then of the relations of the larger to the smaller vessels in the perfect animal, it is probable that the homogeneous membrane in question constitutes the inner layer of the vessels throughout.

The smaller vessels which have been described as forming a series of intermediate vascular arcs between the two larger trunks, are at first only of sufficient calibre to admit the blood corpuscles in single file, so that in this respect they resemble capillary vessels. They are also similar to the latter in structure, consisting of a delicate homogeneous membrane with nuclei here and there imbedded in it. Like the vessels in the branchiæ, they cannot be regarded as mere intercellular spaces.

In several of them the margins run parallel to each other, so that the vessel is of equal breadth throughout. In others they are approximated towards one another more at one point than at another, so that the vessel presents a contracted and dilated portion. It is generally at this dilated portion that the nuclei already noticed exist. From this dilated portion of the vessel, there is also not unfrequently observed a process passing outwards for a short distance in the direction of the circumference of the tail.

In tadpoles farther advanced we find a second series of vascular arcs formed external to the first. The vessel of which each of these secondary arcs is formed, may be described as arising from one of the first series of arcs passing outwards towards the margin of the tail, and then bending upon itself again runs inwards and joins the next primary arc below that from which it arose. The vessels in the second series of arcs resemble in structure those of the first. The latter, in animals about this period, are considerably wider and of much more uniform calibre than in younger embryos.

The changes which take place subsequently, consist merely in the formation of new vascular arcs, intermediate and external to those already formed, until at length they constitute a complete network which extends over the entire breadth of the tail of the animal.

In regard to the structures from which these vessels are formed, there can be little doubt that these are the embryonic cells. The tail of the young tadpole presents the following structure: Exter-

nally is a layer of dark pigment cells the same as those composing the vitelline membrane already described; along the middle line runs the chorda dorsalis, and on each side of it a number of more or less elongated embryonic cells in process of being developed into muscular fibre. The rest of the tail is entirely composed of oval and round embryonic corpuscles filled with granules, and presenting the same character as those already described. By degrees it becomes more transparent by the partial disappearance of the pigment from the cells forming the external layer, as well as by the separation of these from one another by the development of new cells between them which contain little or no pigmentary matter. At the same time, the granular matter contained in the embryonic corpuscles gradually decreases, and at the same time these latter send off two or three processes in different directions, so as to present the aspect of branching or caudate cells. The processes of neighbouring cells are often seen to be united (fig. 9). From these, then, according to the observations of Kölliker, the blood-vessels are formed—the nuclei which we have mentioned as seen in the walls of the latter, being the persistent nuclei of the former. The dilatation which we have described as occurring in the vessels when recently formed, corresponds to the cavity of the cell, while the constricted portions of the same correspond to its caudæ or processes. Of the same nature is the process which is seen passing off from one or more of the vascular arcs towards the margin of the tail.

FIG. 9.—Stellate cells in the tail of the tadpole, developing into capillary vessels. 250 diam.

b. Mammalia.—1. Larger Vessels.—If a portion of any of the larger arteries, such as the hypogastric, carotid, or aorta in fœtal calves about an inch in length be examined, its walls will be found to be composed of embryonic corpuscles and spindle-shaped cells in different stages of development.

Towards the interior of the vessel, there are several layers of embryonic corpuscles imbedded in a more or less homogenous intercellular substance (fig. 10). Proceeding outwards towards the exterior of the vessel, we find spindle-shaped corpuscles more or less perfectly formed. The more perfectly developed of these bodies are situated towards the exterior of the walls of them. These bodies present the same character as the bodies which we find in the walls of the vessels in the adult animal, and which have been described by Kölliker as organic muscular fibres. In the present case, however, I think, there can be little doubt, that they are merely transition stages in the formation of fibres (figs. 11, 12, and 13).

FIG. 10.—Inner layer of umbilical artery of calf 8 in. long. 250 diam.

FIG. 11.—Succeeding layer in the same vessel, composed of spindle-shaped corpuscles. 250 diam.

There is as yet no distinction into inner, middle and external coats.

As regards the direction of these spindle-shaped corpuscles, most of them appear to be arranged in a direction more or less obliquely to the long axis of the vessel. Some are arranged more or less at right angles to these. The latter are not collected together, so as to constitute a single distinct layer or coat, but are found intermingled with the others throughout the entire thickness of the coats of the vessel.

Figs. 12 & 13.—Layers more external in the same vessel, in different stages of development into fibres. 250 diam.

2. In fœtuses four inches long, the solid spindle-shaped corpuscles are increased in number, and towards the exterior of the vessel distinct fibres are seen formed by the union of two or more of these bodies. The layer of embryonic corpuscles with the homogeneous matrix, in which they are imbedded, presents the appearance of a more or less consistent membranous structure, lining the interior of the vessel.

3. In embryos pretty far advanced, as in those measuring about 18 inches in length, the arteries have the following structure. Lining the interior of the vessel is one or more layers of bodies resembling the primary or embryonic corpuscles. Proceeding outwards are found fibres presenting the following characters:—they are smooth, for the most part cylindrical, and measure from $\frac{1}{12,000}$th to $\frac{1}{10,000}$th of an inch in diameter. Many of them are branched and anastomose with neighbouring fibres. Mingled with these are considerable numbers of spindle-shaped corpuscles. Still further outwards the walls consist almost entirely of fibres, the spindle-shaped corpuscles either not existing, or being very few in number.

Besides the set of fibres above described which correspond to what is called a transverse coat in the artery of the full-grown animal, we find another set of fibres crossing the former at right angles. These consist of smooth, shining, cylindrical fibres, about the same breadth as those in the other set. They are not possessed of much inherent elasticity, inasmuch as they break across very readily. They are few in number compared with the others. In direction they correspond to the fibres which have been described by Henle as the longitudinal coat in the artery of the animal after birth. They are not, however, collected together so as to form a distinct and separate coat, but are found intermingled with the other set of fibres, throughout almost the entire thickness of the walls of the vessel; both sets of fibres constituting but a single coat, composed, however, of many distinct layers (fig. 14.)

Fig. 14.--Common carotid artery of calf 2 in. in length, showing different directions of the fibre cells. 250 diam.

External to this we have a layer of white and yellow elastic tissue,

the former predominating and forming a coat which corresponds to the third or external coat in the perfect artery.

In the smaller vessels down to about the $\frac{1}{1000}$th of an inch in diameter, the walls are composed of the same structures as those of the larger arteries.

In arteries below the $\frac{1}{3000}$th of an inch in diameter, the walls are composed only of homogeneous membrane in which round or oval nuclei are embedded. This membrane presents much the same aspect as that which lines the larger vessels.

The capillaries appear to be chiefly formed from stellate or spindle-shaped cells in the same way as already described in the tadpole. The development of these vessels is best studied in the membranes of the fœtal eye, or in the gelatinous-like substance which is found on the chorion and in the umbilical cord (fig. 15).

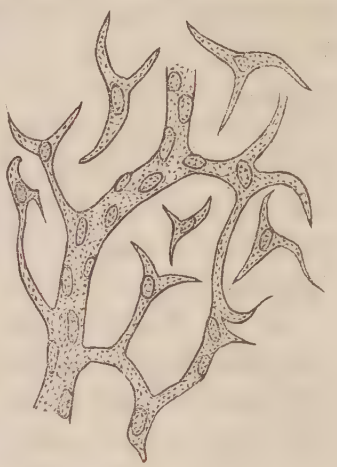


FIG. 15.—Capillary vessels in different stages of formation, from stellate cells, in the eye of fœtal calf. 250 diam.

There is a peculiarity in the arteries of the fœtus, which it may not be out of place here to mention. It has been already stated that the walls of the fœtal arteries are composed of many layers, each layer resembling the other in structure. These can be distinctly uncoiled continuously from one extremity to the other, the fibres either running into one another, or arranged in a more or less spiral manner (fig. 16).

A similar spiral arrangement may be frequently observed in the external coat of the fully-formed artery in the animal after birth.

From what has been stated, it seems to follow :—

1*st*, That the development and growth of the fœtal artery proceeds from the interior towards the exterior of the vessel.

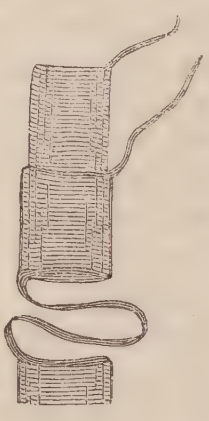


FIG. 16.—Umbilical artery of fœtal calf 12 inches long, partially uncoiled, showing arrangement of its fibres in layers, and also probably their spiral direction.

2*d*, That all the structures occurring in the walls of the vessels are formed from the embryonic corpuscles.

3*d*, That at first there is no distinct demarcation into inner, middle, and external coats.

4*th*, That the perfectly-formed fœtal artery appears to be composed, as stated by the older anatomists, of three coats. The inner is formed of a number of round granular bodies resembling the embryonic corpuscles embedded in a homogeneous intercellular substance. The middle is composed of a number of superimposed layers, each layer consisting of two sets of fibres intermingled together. The external coat consists of white and yellow elastic tissue intermingled.

5*th*, That arteries whose calibre is below the $\frac{1}{3000}$th of an inch,

have their walls formed of homogeneous membrane, with nuclei embedded in it, similar to that lining the larger vessels.

6*th*, That the capillary vessels also consist of homogeneous membrane, with nuclei embedded in it, and appear to be formed from branching cells.

Article II.—*On the Cerebro-Retinal Theory of Colour-Blindness.*

By George Wilson, M.D., F.R.S.E., Lecturer on Chemistry.

(*Continued from page* 240.)

On reviewing what has been stated in the three preceding sections (*ante*, p. 97), on the general colour of the retina; on the special colour of its yellow spot; and on the many variations in colour which appear in the choroid, it is manifest that though the light furnished by the sun, or by artificial luminaries, may reach a number of living eyes unaltered in quality, it will be so altered in colour within them, by the influence of the membranes referred to, that very different chromatic perceptions will be determined by the same primary light. Within the eyeball, unquestionably, there will be, to some extent, a neutralisation of coloured rays by their complements, ending in a conversion of these into white light, or in their entire extinction. Of this, one striking example is afforded by the eyes of herbivorous animals, where the light reflected from a green tapetum, afterwards passes through transparent red blood-vessels, and, doubtless, not without undergoing a loss; whilst another portion, traversing the less vascular parts of the retina, will mix with the rays reflected from the blood-vessels, and produce white light. But the green colour of the light actually emitted from the eyes of the animals in question, shows that these effects are only partial, as we might expect them to be. And neutralisation of coloured light, in either of the ways referred to, can only affect the light which has traversed the retina, so that, after making all allowance for its occurrence, the main conclusions urged in the preceding sections will remain unaltered.

The question then arises, may colour-blindness ever depend on such variations in the colour of the internal membranes of the eye as have been shown to occur? In reply to this question, I can refer to but one fact, and that does not favour the conclusion, that true colour-blindness can be occasioned in the way suggested. It is quite certain that Albinos are not necessarily colour-blind. In the very curious account of his own and his sister's albinism, furnished by Dr Sachs, there occurs the emphatic announcement,—"Leucaethiopes nec *achromatoblepes*, nec *acyanoblepes* sunt. Sibi quidem ipsi omnes colores, eosque non aliter ac ceteri plerique Homines, cernere atque discernere videntur."[1] Sachs's opinion is liable to the

[1] Hist. Nat. duorum Leucaethiopum auctoris ipsius et sororis ejus, à G. Sachs, M.D.—*Solisbaci*, 1812, p. 75, par. 150.

objection, that it is his own judgment upon himself, not unequivocally confirmed by a person of normal vision: and exception might further be taken to his conclusions, inasmuch as besides his albinism, his perception or conception of colours was singularly anomalous. Sachs was subject to what has since been called *hyperchromatopsy*, *i.e.*, a propensity more or less constant, to attach the idea of colour, not only to physical objects, which, to ordinary vision, appear coloured, but likewise to things from which the majority of mankind generally abstract altogether the conception of colour. Thus, he imputed colours to the letters of the alphabet, the notes of the musical scale, the ordinary numerals, and the days of the week. In the alphabet, for example, *a* was red, *i* white, and *u* black. In the days of the week, Sunday was *white;* (as if, like a Roman, he regarded it *dies creta notandus*); Monday was *grey* (recalling our school-boys' *black* Monday); Wednesday was *yellow*.[1] From the observations, however, of Cornaz and Wartmann, it appears that this rare affection is not necessarily an accompaniment of albinism, and that examples of it occasionally occur in those whose vision is otherwise normal.[2]

It is unnecessary, however, to discuss the exact value of Sachs's conclusions concerning his freedom from colour-blindness, as Wartmann has put on record the examination of four youthful Albinos, three females and one male. Two of the girls and the boy were of one family, and the third girl was cousin to these three. All four, Wartmann tells us, "ne présentent aucun Daltonisme." He ascertained this by direct trials, and mentions specially, that they distinguished correctly the closely approximating shades of the same colour. They also perceived all the tints of the spectrum.[3]

It thus appears that a total absence of pigment from the choroid, and a replacement of the dark surface found in ordinary human eyes by a brilliant red one, is not incompatible with a perception of colours, such as a skilful observer, like Wartmann, thoroughly alive to the existence of colour-blindness, could not distinguish from a purely normal perception. But the conditions of the Albino eye are very peculiar, and the small number of Albinos, and these very young, who were examined by Wartmann, cannot be considered as finally disposing of the question, Is the Albino vision of colour the same as that of the non-Albino eye? especially when the peculiarities of

[1] Op. cit., p. 82, par. 160.

[2] See, in illustration of this Mr W. White Cooper's comments in the article "Vision," Cyclopædia of Anatomy and Physiology; and Wartmann's Paper, p. 43, referred to in the succeeding Note.

[3] Deuxième Mémoire sur le Daltonisme, par E. Wartmann. Genève, 1849, p. 42. I am indebted to Mr W. White Cooper, of London, for a knowledge of this Memoir, and for the use of one of the few copies of it which have reached this country, where it is much less known than it deserves to be. Had I been aware of its existence at an earlier period, I should gladly have quoted from it; and I shall have occasion, in the sequel, to refer to it more fully. I infer, from its title, that it is a reprint from the Memoirs of 'La Société de Physique et d'Histoire Naturelle de Genève,' for 1848.

colour-vision exhibited by Sachs, the only adult, educated subject of albinism whose experience is fully on record, are taken into account.[1]

Nevertheless, I am not disposed to assert that colour-blindness, of the kind Dalton and his fellows exhibited, can be occasioned by such modifications in the colour of the membranes of the eye as I have drawn attention to. But an extreme chromatic equation, not always distinguishable in its practical manifestations from veritable colour-blindness, may certainly be occasioned by the varying condition of the membranes referred to, and on this account I have, in the introduction to this discussion, spoken of such manifestations as deserving to be ranked under a special chromatic theory of colour-blindness.

I close this section with the expression of the hope, that the colour of the membranes within the eye-ball will now be an object of more frequent and minute examination by physiologists and pathologists, than it has hitherto been. My friend, Dr Beddoe, has indirectly supplied much interesting information on this subject, in his little work recently published,[2] containing the results of an examination during life, of the colour of the eyes and hair of some 5000 of the Scottish people, representing nearly all the districts of their country. According to the observations of John Hunter, already quoted, in which physiologists generally concur, a dark choroid and dark hair go together, and *vice versa*. In the future enumeration, accordingly, of cases of colour-blindness, it is desirable that the colour of the hair should be recorded, as it cannot be expected or desired that the majority of the colour-blind should speedily become the subjects of pathological investigation. The colour of the iris generally, but not invariably, resembles in shade that of the hair, and a hazel or pale golden iris has been thought to be an index of colour-blindness. Of the truth of this particular conclusion no proof has been given; but it is certain that the amount of dark pigment on the back of the iris (*uvea*), increases or diminishes in proportion to the general abundance of colouring matter tinting the choroid; and it would be highly interesting to know whether the fair-eyed and the dark-eyed—apart from colour-blindness—attach a different chromatic value to the same colour. The proverbial difference between the tints preferred for dress by blondes and brunettes, and the great fondness of the negro races for white and the primary colours, are probably *in part*, at least, related to differences in the colour of the

[1] I urge this the more that I learn from Wartmann's second Memoir, that the late Dr Cunier published cases "where several *dyschromatopsic* [colour-blind] females presented the greater number of the symptoms assigned by Beer and other oculists, to the absence of the black pigment of the choroid." These appear to have been colour-blind Albinos, but I cannot ascertain the particulars of their cases, which are recorded in "Annales d'Oculistique, t. 1, p. 490," a work of which only the later volumes are accessible in Edinburgh.

[2] A Contribution to Scottish Ethnology. By John Beddoe, B.A., M.D. 1853.

choroid, to which that of the hair and of the iris is a clue. The hair is probably the more important external index of the chromatic condition of the choroid, especially where the hair differs in shade from the iris; but this is not certain; and even if it were, it will often be impossible, in the living human subject, to look at both, so that in all cases each should be examined, and the result recorded. I regret that I was not alive to this at an earlier period of my researches, so as to have noted the hair and eyes of all my colour-blind acquaintances. Like most other writers, however, on the false vision of colours, I have not had my attention prominently attracted by the dark or the fair monopolizing colour-blindness. Such observations as those of Dr Beddoe can alone settle this question, and would, in addition, throw light on other important problems.

CEREBRO-RETINAL THEORY OF COLOUR-BLINDNESS.

The second of the leading physical theories of colour-blindness refers the false perception of colours to the peculiar organization of the nervous apparatus of the eye, including so much of the brain as is essential to vision. (*ante* p. 97.) It may be conveniently called the *cerebro-retinal* theory of colour-blindness.

Many more modifications occur in the various published versions of this theory than in the chromatic hypothesis which has been discussed. On the whole, however, they may be ranked in two divisions, and named the *cerebral* and *retinal* theories, according as the *brain* or the *retina* is held to be most concerned in the manifestation of abnormal vision. It is not intended, however, by such names to imply that the retina alone, or the brain alone, has been regarded as the sole seat of colour-blindness. On the other hand, in our ignorance of the full extent to which the brain is essential to vision in all its relations, it has been felt vain to speculate too nicely on the relative share which the nervous expansion within the eye, and those portions of the brain into which the optic nerve can be traced backwards, severally contribute to the vision of colour.

Of the cerebral theories of colour-vision, only one calls for special notice. According to phrenologists, the power of distinguishing colours depends upon a particular part of the brain lying over the roof of the orbit, and, when largely developed, rendering one part of the superciliary ridge specially prominent. In the words of Mr Combe, "Observation proves that those individuals in whom the portion of the brain which is immediately above the eye, beneath the eyebrow, is largely developed, possess in a high degree the faculty of distinguishing colours."

The question whether a prominence in one part of the eyebrow characterizes those who have the power of readily distinguishing colours, and, conversely, whether a flatness or depression is found in the same region in those whose sense of colour is imperfect, plainly admits of settlement without discussing any portion of the metaphysics of phrenology. The thesis maintained by Mr Combe and

other phrenologists is, that not only is a feeble normal perception of colours accompanied by a small development of the superciliary prominence which they term the organ of colour, but that this organ is still more decidedly depressed or absent in those who are colour-blind. In support of this conclusion, they have published accounts of several well-marked cases of colour-blindness, in whom the organ of colour was defective or absent. Among the cases also which I have recorded, Dr K. and Mr R. have reported that they have been pronounced by phrenologists to have the same defect. Mr D. also informed me that the organ of colour was wanting in him.

The general result, however, of more extended inquiry has not been favourable to the phrenological conclusions. I have made no cranial examination myself of any of the cases of colour-blindness known to me, not from indifference to the matter, but because, as I must frankly confess, I am unable to distinguish such differences between the development of one superciliary ridge and another, as I find other observers declare themselves able to do; and I therefore give no opinion, founded on personal observation, on this question. There has been no want, however, of inquirers; and the general result, so far as my knowledge extends, has been the following:— Wartmann, in his first memoir, observes, "I must say that among the Daltonians whom I have examined, the organ in question (*i.e.*, of colour) was in general but little evident, except in two of them, in whom it was remarkably prominent."[1]

The late Lord Jeffrey was pronounced by phrenologists to have a feebly developed organ of colour.[2] He, however, was not only free from any trace of colour-blindness, but, as he has recorded himself, received great delight from the spectacle of brilliantly coloured objects, and had a remarkable power of matching colours from memory.[3]

Szokalski, who has had great experience in cases of colour-blindness, and agrees with phrenologists in the belief that there is a cerebral organ of vision, and that this is diminished in volume in the colour-blind, adds, notwithstanding, "We know well that phrenologists place the organ of colour in the middle of the superciliary arch; we have, however, examined scrupulously and with great pains many persons who have presented very decided depressions of this arch; but despite of the best wishes in the world, we could never discover in them any trace of chromato-pseudopsy."[4]

Mr W. White Cooper observes on this passage, "neither does our own experience support the theory of the phrenologists; in two well-marked Daltonians examined by us, the whole superciliary region was remarkably well developed."[5]

[1] Taylor's Scientific Memoirs, 1846, p. 184.
[2] Combe's System of Phrenology, 3d Edition, p. 405.
[3] Op. cit., p. 406, and Edin. Rev., Nos. 88 and 89.
[4] Quoted by Mr W. White Cooper, Article *Vision*, Cyc. of Anat. and Physiol.
[5] Op. et loc. cit.

The whole of the observations referred to above were made on the head during life, and are open to the objection to which all such observations are liable, that they identify the internal development of the brain with the external configuration of the skull. This objection is specially pertinent in reference to the phrenological organs situated in the neighbourhood of the frontal sinus, and that it is not a speculative difficulty, can be shown by a very curious example which has not hitherto come before writers on this subject. At the *post-mortem* examination of Dalton's body, the following fact was observed, as stated to Dr Henry by Mr Joseph A. Ransome, Dalton's medical attendant, who made the examination; "Mr Bally (formerly assistant to the late Dr Spurzheim), was present, and took casts of several parts of the brain and the cavity of the skull. He pointed out a remarkable prominence on the frontal portion of the orbitar plates (which represents the phrenological site of the '*organ of colour*'), and the imperfect or deficient development of the convolution of the anterior lobes, which rested upon them. Of course, Mr Bally adopted this as the true explanation of the peculiarity of Dalton's vision; and we as witnesses (not only without faith in phrenology, but even opponents), are bound to record the fact."[1]

Here, then, according to the judgment of those present, there appeared a marked deficiency in that portion of the brain which phrenologists regard as the organ of colour, in the person of the most famous example of colour-blindness, and though he were not famous, his case would deserve record as the solitary one where the brain itself was examined. Something, as I have implied in a note, must be deducted from the value of this case on the ground of the effects which time and disease had produced upon an organ so liable to derangement as the brain. Let it be assumed, however, that no such deductions are called for, and then the truth I wish to dwell upon will be more fully appreciated, namely, that during life no phrenological sign of colour-blindness appeared on Dalton's forehead. Those who have seen him, or who are familiar with his busts or portraits, will be aware that his head, which remarkably resembled that of Sir Isaac Newton, was like it in exhibiting prominent eyebrows. The superciliary ridge in truth, projected very markedly so as

[1] Henry's Life of Dalton, p. 201. It seems proper to inform the reader, whether disposed to draw conclusions favourable or unfavourable to the phrenological explanation of colour-blindness, that Dalton died at the age of 78, seven years after an attack of paralysis, which permanently impaired the use of the right side, as well as the power of articulation, and the memory of words. Several slighter attacks succeeded in the intervening years, and the last proved fatal. Lesions of the brain, such as occur in cases of the kind, were found after death, the particulars of which will be found in the work quoted above. How far a diminution in volume, or apparent imperfect development of the anterior cerebral lobes, may have been connected with the advanced age, and repeated attacks of paralysis which Dalton had undergone, I will not pretend, even remotely, to determine, but the brain was certainly not in a normal condition at the time of the *post-mortem* examination.

to overhang the eyes. If any man, indeed, would have been selected by phrenological signs as the possessor of an exquisite sense of colour, Dalton would have been. Yet assuredly, if Newton and Dalton had been contemporaries, and had simultaneously published and compared their descriptions of the colours of the rainbow, they must have been hopelessly perplexed to account for statements, so strangely conflicting, regarding a common object of vision.[1]

The doctrine, accordingly, that colour-blindness depends upon the absence of that portion of the anterior lobes of the brain in which phrenologists place their organ of colour, has not been substantiated. It may be quite true, nevertheless, that there is a part of the brain specially connected with the vision of colour, as we are quite certain that a part, and only a part, of this organ is essential to the integrity of vision in general. But the researches of physiologists do not, as yet at least, justify the conclusion that any portion of the brain so situated as to impress its form upon those parts of the skull which can be examined during life, is peculiarly related to the sense of sight; and I entertain no expectation of further investigation lending more justification to the phrenological hypothesis. Colour-blindness is, in the great majority of cases, only partial, and although I differ from most previous writers on this subject in regarding it as affecting the perception of the lighter and darker shades of all colours; still it has been abundantly shown that the greater number of the colour-blind do not mistake yellow or blue when full and well illuminated; nor have they any difficulty with white and black. If, then, an organ of colour is essential to the perception of blue and yellow, *ex. gr.* there seems no reason why the majority of the colour-blind should exhibit a deficiency of more than about a third of the hypothetical organ of colour, unless it be contended that it is mainly serviceable in enabling us to perceive red. It appears vain, therefore, to expect to discover the existence of colour-blindness by so rude an external indication, as the prominence over a small area, of one of the plates of the skull. But it is assuredly desirable that observations should be multiplied on this point, and that the condition of the brain in the colour-blind should be carefully noted.

I do not discuss the *Retinal* theories of colour-blindness minutely, for the greater number of them are either mere descriptions of this affection of vision in the terms of a hypothesis which regards the

[1] I have not trusted to my own judgment in referring to Dalton's forehead, although I have seen him, and have examined a cast taken from his face after death, as well as many portraits of him. Dr Henry, to whom I referred the question, Had Dalton a prominent superciliary ridge? informed me in reply, that, unwilling to trust his memory on the matter, he had consulted a copy made by machinery from Chantrey's faithful bust, and found the eyebrows strongly marked in it as he remembered them to have been on the living head.

retina as its seat, or they are speculations which connect such an assumption with views regarding the organization of the membrane in question, which neither admit of being confirmed or refuted. Many of these views are exceedingly ingenious, and well worth the attention of students of the theory of vision. They will be found fully and impartially discussed in Wartmann's Memoirs. But they lie too far beyond the scope of my present inquiry, which mainly deals with what is physically demonstrable in the phenomena of colour-blindness to justify their discussion here, especially as they have little bearing on the practical questions connected with this affection of vision. I shall content myself, accordingly, with explaining the theory published by Wartmann in his second memoir, and with publishing a very interesting speculation on the same subject, for which I am indebted to my friend Professor Kelland.

"I admit," says Wartmann, "with Harvey, Young, Jüngken, Müller, and others, that its seat is in the retina, and I think it is produced by an abnormal state of this nervous expansion of such a sort that it reacts similarly under two or more different coloured radiations. If the vibration (*ébranlement*) caused by a red ray is identical with that which a green ray produces, there will be confusion of these colours. This theory is independent of every system destined to explain light."[1]

Wartmann then proceeds to illustrate his view more fully by the analogy of sonorous bodies. If, as he observes, a series of elastic surfaces unalterable by atmospheric variations, and each of which gives out a different musical tone when placed in communication with a sounding body, is arranged together close to a musical instrument, frequently in use, at the end of some days it will be found that the note which at first threw into vibration only one of the elastic surfaces, will by and by produce the same effect upon a second, and thereafter upon several more. "Thus," continues he, "the elements of this system will have become modified in their facility of vibration, and by also resounding easily under the influence of different tones, they will resemble the retina of a Daltonian who confounds different colours." An experiment is then described, in which the panes of a closed window which at first vibrated only under the influence of certain notes or tones of a piano in the window-recess, were by prolonged daily use of the instrument thrown into vibration by several additional tones to which at first they did not respond; and conversely acquired their original indifference to these when the piano was left unused for six months. According to this view, the retina, to use Wartmann's own words, "is in a state *analogous* to that of an elastic system which two or more musical tones throw into resonance with equal ease."[2]

[1] Deuxième Mémoire, p. 46.

[2] Op. cit., p. 48. Wartmann's Deuxième Mémoire, from which these views are taken, was read to the Physical Society of Geneva, on 9th Oct. 1848; but

The theory of Professor Kelland, which I subjoin, was first explained orally to the Royal Scottish Society of Arts, April 1853, on the occasion of my reading a communication on Colour-Blindness, and was shortly afterwards reduced to writing at my request.[1]

"AN ATTEMPT TO EXPLAIN CHROMATOPSEUDOPSIS. By the Rev. PHILIP KELLAND, Professor of Mathematics, University of Edinburgh.

"On the undulatory theory, light is due to vibrations, and difference of colour, like difference of pitch in music depends on nothing else than the period of pulsation, or the time occupied by a vibrating particle in performing its oscillation. The whole scale of colours is comprehended in something like the same limits as the octave in music—the extreme vibrations being performed in times which are very nearly in the proportion of two to one. The sensation of a given colour is therefore due to the reception by the retina of pulsations of a given period. In what way the retina performs its functions we do not precisely know. As the instrument in conveying pulsations, it may be presumed itself to vibrate agreeably to their period. We shall, accordingly, regard it in its normal state, as a vibrating system capable of responding to pulsations of every period, within certain limits, and of conveying to the mind the sensation which we term colour appropriate to each period. But in other states of the eye, this appears incapable of being effected. The retina, for instance, vibrates more readily in accordance with one period of pulsation than with another. Thus one eye may be very well disposed to respond to vibrations of the period of the yellow, so as to be powerfully affected by it; another may be almost insensible to the red, so as to be scarcely able to discern faint objects of that colour. This constitutes one form of chromatopseudopsis. But a more remarkable form is that in which the powerful complementary colours produce the same impression on the mind. This would appear to be owing to the facility possessed by the retina for starting into vibrations of every period, when excited by those of one only, just as a musical pipe or string gives all the notes of the chord at the same time, or as the different strings of a harp vibrate when only one has been struck. The retina, in this case, may be compared to a piano, or rather to one octave of a piano without dampers; the sound of one note causes

from a reference in that memoir to a second edition of his *first* paper on colour-blindness, contained in la Bibliothèque Universelle, the publication of the theory referred to goes back to June or July 1845. (Deuxième Mémoire, pp. 2 and 45.)

[1] As a certain resemblance occurs between the theories of Wartmann and Kelland, it is proper to mention that the latter was not aware of the former's opinions till the autumn of 1854, when, on first becoming myself acquainted with Wartmann's "Deuxième Mémoire," I showed it to Professor Kelland.

all the others to vibrate at the same time, but not with equal intensity; the centre note between the note struck and its octave being most powerfully affected. Assuming this analogy to be correct, we should expect to find that, in a retina so constituted, every colour would be mixed more or less with its complement. In the case of green or red, the complementary red or green respectively is of equal intensity with the original, and consequently the same impression may be produced on the mind whether the direct red raises up its complementary green or *vice versa*. On the other hand, the powerful yellow having as its complement the weak purple, we should not expect that it would be matter of indifference which is the direct agent and which the subjective impression. Accordingly, we find many eyes unable to distinguish green from red, but few apt to confound yellow with purple.

"To eyes constituted as I have supposed, those combinations of colour which by contrast produce the most powerful and pleasing impressions on others ought to be rather unpleasing, since contrast of colour in the one is replaced by difference of tone in the other. We have reason to believe that a similar phenomenon occurs relative to sound: we know, at least, that many persons are totally insensible to the *pleasurable* sensation of melody, but whether or not such persons experience a different impression of pitch from that which affects ordinary ears, has not probably been ascertained. Persons have been known destitute of the power of distinguishing one air from another, provided the rhythm of both were the same, but whether this has arisen from mere dulness of discrimination of tones, or from a positive similarity of impression from tones which, to ordinary ears are very different, has not, perhaps, been well investigated. It is not impossible that the latter hypothesis is correct, at least it involves interesting matter for further inquiry."

The ingenious theories of the professors of Geneva and Edinburgh will be acknowledged by all to assist us in forming a clear and vivid conception of what may be the condition of the colour-blind retina, and they very probably furnish not only a picture of its condition, but to some extent a fac-simile of its mode of abnormal action. Kelland's theory is the more complete of the two, inasmuch as it both provides an explanation of that simple insensibility to a colour, such as red, which we do no more than name when we call it colour-blindness; and also of that confusion of colours with each other, which is likewise simply named when it is called, Chromatopseudopsis: whereas Wartmann's theory only accounts for the latter. In the preceding sections, however, of this paper, it has been shown that there is a positive insensibility of the retina in many persons to red light, which excites neither the sensation of luminosity nor of colour; and further, that the dark shades of all colours, primary, secondary, or mixed, are not distinguished by the colour-blind from black, and that the very light shades are not distinguished from white. Apart thus from the tendency to confound

colours with each other, there is, on the part of the colour-blind, a positive inability to perceive certain of these. Their vision of such colours is not false or confused; they have no vision of colour at all. The condition of a retina thus insensitive to colour appears more like that of a vibrating sonorous body which cannot be made to sound by strokes, or impulses of a certain intensity, than one which passes into vibration if any other elastic substance in its neighbourhood begins to vibrate.

An addition, therefore, must be made to Wartmann's hypothesis, and whether we adopt Kelland's theory, or not, we seem to be fully entitled to affirm that the cerebro-retinal apparatus of vision in the colour-blind is, either through congenital defect, or subsequent morbid change, unendowed with that sensitiveness to colorific impressions which it possesses in those whose vision is normal. It is probably the retina that is the chief seat of this diminished sensibility to colour, and the simpler form of colour-blindness might fitly enough be called *chromatic amaurosis*.

ARTICLE III.—*On the Weight and Dimensions of the Heart in Health and Disease.* By THOMAS B. PEACOCK, M.D., Assistant Physician to St Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest.

(*Continued from* p. 323.)

M. BOUILLAUD[1] concludes his observations with the remark that, in cases of hypertrophy, the weight of the heart may amount to nearly three times that of the organ of usual dimensions, and five times that of the most atrophied heart. It will, however, be seen, from the data published in the former part of this paper, that this estimate is too low. As in males the average weight of the heart is 9 oz. 8 dr. (or, if the cases in which the weight of the organ was 12 oz. be included, 9 oz. 11 dr.), and the extreme weights of the organs examined were 5 oz. and 40 oz. 12 dr., the weight in the most hypertrophied heart was upwards of four times that of the average organ, and eight times that of the lightest heart weighed. In females, the range of weight in the heart, though sufficiently remarkable, is less than in males. The mean weight of the heart in females is 8 oz. 13 dr. (or 8 oz. 15 dr.), and the extreme weights are 5 oz. 8 dr. and 23 oz., so that the most hypertrophied heart was nearly three times the weight of the average organ, and four times that of the lightest heart. The greatest weight recorded was in a case of hypertrophy without material valvular disease; and the lightest hearts were found in cases of cancer of the pylorus and disease of the liver—an observation which accords with

[1] Traité Clinique des Maladies du Cœur 2me Ed. 1841, Tome i. p. 68.

TABLE X.—Showing the dimensions of the healthy heart *in males*, specifying in separate columns, the cases of phthisis, bronchitis, and of other diseases, unconnected with pulmonary disease, together with the dimensions of the heart in persons dying of chronic diseases *only*, unconnected with disease in the lungs. The table includes the mean of all the observations, together with the dimensions of the lightest and heaviest hearts examined.

	Phthisis.			Bronchitis.			Other.			Chronic cases only.		
	Mean.	Lightest	Heaviest	Mean.	Lightest	Heaviest	Mean.	Lightest	Heaviest	Mean.	Lightest	Heaviest
	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
Circumference of the heart, . .	94·5	92	97	118·6	116	130	103·1	98	87	104·4	98	120
Girth of right ventricle, . . .	49·5	50	49	67·3	66	76	55·6	54	48	57·2	54	60
„ left ventricle, . . .	45·	42	48	51·3	50	54	47·5	44	39	47·2	44	60
Length of the cavity of right ventricle,	43·3	36	48	50	51	51	42·5	42	51	40	42	34
„ „ left ventricle,	37·3	36	39	40·6	45	36	37·	36	40	34·4	36	32
Thickness of walls of right ventricle base,	2	2	2	2·5	2	3	1.81	2	2	1·8	2	2
„ „ midpoint,	2	2	2	3	2·5	4·5	1·93	2.25	2	1·95	2.25	2
„ „ apex, .	1·58	1·25	1.5	1·3	1·5	1·5	1·37	2	1	1·5	2.	1.5
„ „ of left ventricle base,	5·3	6	4	5·5	5	6	5·1	5	6	4·7	5	5
„ „ midpoint,	6·3	5	7	5.8	5·5	6	5·95	6	8	5·8	6	6
„ „ apex, .	2·3	2·5	2	2·5	2	2·5	2·5	3	2	2·8	3	3
„ of septum, . . .	5·8	5	6	5·08	5	4	5·77	6	6	5·6	6	5
Circumf. of right auriculo-ventric. aper.,	49·3	51	43	58·6	62	60	54·4	57	57	57·7	57	60
„ of pulmonic, . . .	38·6	39	29	46	45	45	40	39	42	40·8	39	42
„ of left auriculo ventricular,	45·3	48	37	54	54	54	44·3	42	45	47·2	42	48
„ of aortic, ...	34·6	36·	26	39	39	36	35.5	33	39	36·4	33	39

The observations in cases of phthisis are three; the ages 20, 31, and 37, and the weights of the heart, 6 oz. 4½ dr., 9 oz. 4 dr., and 11 oz.
The number of cases of bronchitis, etc., is also three, the ages 38, 40, 72, and the weights of the heart 11 oz., 12 oz. 8 dr., and 14 oz.
The number of other cases is twelve, the ages ranging from 23 to 66, and the mean age 32·8; the weights of the heart range from 6 oz., 9⅓ dr. to 12 oz., and the mean weight is 9 oz. 4·7 dr.
The number of cases of chronic disease only are five, the ages 23 to 35, the weights of the heart from 6 oz. 9½ dr. to 9 oz. 1·5 dr.

TABLE XI.—Dimensions of the healthy heart *in females*, specifying separately the dimensions in cases of phthisis, bronchitis, and of all other diseases, connected with pulmonary disease.

	Phthisis.			Bronchitis.			Other.		
	Mean.	Lightest.	Heaviest.	Mean.	Lightest.	Heaviest.	Mean.	Lightest.	Heaviest.
	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
Circumference of the heart, . .	102·6	90·	108·	93·	94·	92·	107·8	108.	120·
Girth of the right ventricle, . .	56·	48·	60·	53·	54·	52·	59·	60·	72·
„ left ventricle, . .	46·6	42·	48·	40·	40·	40·	48·8	48·	48·
Length of the cavity of the right ventricle,	45·3	42·	48·	42·6	44·	42·	43·7	42·	48·
„ „ left ventricle,	36·6	27·	42·	34·6	32·	36·	37·5	36·	39·
Thickness of walls of right ventricle, base,	1·6	1·5	1·5	3·	4·	2·	1·81	1·	2·
„ „ midpoint, .	2·3	1·5	3·	2·87	4·	2·	1·87	1·5	1·5
„ „ apex, .	1·3	1·5	1·5	1·87	2·	1·	1·18	·5	1·
„ „ of left ventricle, base,	4·8	6·	3·5	5·2	4·	4·	4·9	4·	5·
„ „ midpoint, .	5·6	7·	5·	6·	4·	7·	5·8	5·	6·
„ „ apex, .	3·16	2·	2·	2·25	2·	2·	2·7	2·5	2·5
„ of septum, ...	3·5						4·7	4·	6·
Circumf. of right auriculo-ventricular apert.	54·	51·	60.	47·5	51·	40·	52·1	54·	51·
„ of pulmonic, ...	42·	42·	45·	37·7	39·	34·	39·	39·	42·
„ of left auriculo-ventricular, .	45·	48·	48·	44·2	45·	42.	45·5	45·	45·
„ of aortic, ...	34·	33·	36·	34·5	39·	39.	34·	33·	36·

The cases of phthisis in the table are three in number, the ages 20, 24, and 66 ; the weights of the heart 7 oz., 9 oz. 5 dr., and 10 oz. 11 dr.

The cases of bronchitis are four, the ages 25 to 60, mean 47, and the weights of the heart 9 oz. to 11 oz., mean 10 oz. 2·7 dr.

The other cases, unconnected with pulmonary disease, are eight in number ; the ages 20 to 64, the mean age 35·3, and the weights of the heart 7 oz. to 9 oz. 10 dr., the mean weight 8 oz. 4 dr.

The cases of chronic disease only have not been separated in this table, as they are only two in number.

the results of M. Louis[1] as to the small size of the heart in cases of this description.

M. Bouillaud, in referring to the statement of Lobstein, that a heart examined by him weighed 2 lbs. (or 34 oz. avoird.), suggests that the organ had most probably been weighed before being deprived of its coagulum and blood; but this weight is so far from being extreme, that there is no reason to doubt the correctness of M. Lobstein's report. Dr Hope[2] mentions having examined, at St George's Hospital, a heart which weighed $2\frac{1}{2}$ lbs., which, if, as is probable, the weight employed was avoirdupois, equals the heaviest organs weighed by Dr Clendinning and myself.

PART III.—*Dimensions of the Healthy Heart.*

INFERENCES.

1. In males, the average circumference of the healthy heart, in persons of adult age, dying of diseases unconnected with any pulmonary affection, measured externally, was 103·1 lines, and the extreme circumference in different cases was from 87 to 120 lines.

The girth of the right ventricle averaged 55·6 lines, that of the left ventricle 47·5 lines; but these dimensions varied greatly in different cases, the girth of the right ventricle having a range of from 48 to 60 lines, that of the left ventricle of from 39 to 60 lines.

The mean length of the cavity of the right ventricle was 42·5 lines, that of the left ventricle 37 lines. These measurements also varied—the length of the right ventricle ranging from 34 to 51 lines, that of the left ventricle from 32 to 44 lines.

The parieties of the right ventricle had an average thickness at about the middle of its anterior wall of 1·93 lines; and decreased slightly in width towards the pulmonic orifice, and about half a line towards the apex, having thus a thickness of 1·81 lines in the former situation, and of 1·37 at the apex.

The walls of the left ventricle had a medium width at about the mid-point of 5·95 lines, at the base of 5·1 lines, and at the apex of 2·5 lines. The thickness of the parieties of the ventricles exceeded or fell short of these dimensions, without passing the limit of health, in the right ventricle by half a line, and in the left by about a line and a half. The thickness of the right ventricle ranged from 1·5 to 2·5 lines; that of the left ventricle from 5 to 8 lines.

The septum of the ventricles had an average thickness at the middle of 5·77 lines, and its width ranged from 3·75 to 7 lines, generally corresponding in thickness with the parieties of the left ventricle.

The right auriculo-ventricular aperture had a mean circumference of 54·4 lines; the pulmonic orifice of 40 lines; the left auriculo-

[1] Sydenham's Society's Translation, p. 52.
[2] Diseases of the Heart, 3d Ed., 1839, p. 238.

ventricular aperture of 44·3 lines, and the aortic orifice of 35·5 lines; but the dimensions of the orifices also varied greatly in different cases, the right auriculo-ventricular aperture having a range of from 45 to 60 lines, the pulmonic of from 34 to 45; the left auriculo-ventricular aperture of from 38 to 51, and the aortic orifice of from 28 to 48.

2. In females the dimensions of the heart are ordinarily less than in males, but the difference is rather apparent in the diminished thickness of the walls of the ventricles and in the circumference of the orifices, than in the capacity of the cavities.

In adult females the circumference of the heart in persons dying of various affections, unconnected with disease in the lungs, had a mean of 107·8 lines, and in different cases it attained extremes of 87 to 120 lines.

The girth of the right ventricle averaged 59 lines, and varied from 49 to 72 lines in different cases. The girth of the left ventricle averaged 48·8 lines, and ranged from 38 to 60 lines.

The medium length of the cavity of the right ventricle was 43·7 lines, and it ranged from 34 to 48 lines. The left ventricle had a mean length of 37·5 lines, and extremes of 32 and 44 lines.

The thickness of the walls of the right ventricle was near the base 1·81 lines, at the midpoint 1·87 lines, and near the apex 1·18 lines; and the greatest thickness varied in different cases from 1·5 to 2·5 lines. The parieties of the left ventricle had a medium width near the base of 4·9 lines, at the midpoint of 5·8 lines, and at the apex of 2·7 lines, the greatest width ranging in different cases from 5 to 8 lines.

The septum averaged 4·7 lines in thickness, and its width in various cases was from 4 to 6 lines.

The right auriculo-ventricular aperture had an average circumference of 52·1 lines, the pulmonic orifice of 39 lines, the left auriculo-ventricular aperture of 45·5 lines, and the aortic orifice of 34 lines; and the variations in the capacity of the different orifices, was, in the right auriculo-ventricular aperture from 42 to 57 lines, in the pulmonic from 33 to 42, in the left auriculo-ventricular from 37 to 54, and in the aortic orifice from 29 to 36. It will thus be seen that while in the two sexes the dimensions of the whole heart, and the size of the cavities did not differ materially, in females the walls were somewhat thinner, and the orifices less capacious than in males. The comparison is not, however, a just one, for, while a larger proportion of the males the dimensions of whose hearts are given, died of chronic diseases,—the females, with two exceptions, all died after short periods of illness. Had the respective proportions of cases of acute and chronic disease in the two sexes been similar, the dimensions of the heart would most probably have shown a greater difference. It would, however, appear that in the female heart the cavities are somewhat larger, relative to the thickness of the walls, than in males.

3. From these observations it thus appears that the girth of the right ventricle, measured externally, exceeded that of the left in

males by 1-6th, and in females by 1-5th. The length of the cavity of the right ventricle exceeded that of the left ventricle, in males by about 1-7th, and in females by about 1-6th. In both sexes the thickness of the walls of the right ventricle is about 1-3d that of the parieties of the left ventricle. The thickness of the septum is intermediate between that of the external walls of the right and left ventricles.

In males the pulmonic aperture was about 1-8th more in circumference than the aortic; the left auriculo-ventricular orifice about 1-4th more than the aortic; and the right auriculo-ventricular opening one-half larger. In females, the difference between the circumference of the aortic and of the other orifices, is somewhat greater.

4. In the two sexes, the length of the heart measured from the base to the apex was greater than its breadth measured across the broadest part. These dimensions were taken in comparatively few cases: in these the mean length of the organ was 49 lines, and the mean breadth 43 lines.

The walls of the right auricle measured at the middle of the sinus had a medium thickness of half a line to one line, but from the arrangement of the *musculi pectinati*, it is not easy to estimate their width. The walls of the left auricle had an average thickness of 1 to $1\frac{1}{2}$ lines.

The right coronary artery in the cases in which it was measured had a circumference of from 5 to 8 lines; ordinarily this artery gives off the anterior branch immediately after its origin, but not unfrequently that vessel arises separately from the right sinus of Valsalva. The left coronary artery is generally larger than the right. In one case I have seen the two coronary arteries arise by a common trunk, and in another both arteries arose from the same sinus. The orifice of the coronary vein has generally a circumference of about 10 lines.

The fossa of the foramen ovale has a mean size of 8·6 lines in its longest, and 6·8 lines in its shortest diameter, but the dimensions vary considerably. The fold on the left side ordinarily overlaps the isthmus by 2 or 3 lines, and, when a valvular opening remains, it is usually 2 to 3 lines wide.

5. The heart is ordinarily greater in persons who have died of bronchitis and other pulmonary diseases, phthisis excepted, than in persons who have died of affections in which there was no obstruction in the lungs. The enlargement generally consists in hypertrophy and dilatation of the right ventricle and dilatation of the left ventricle;—the cavity of the right ventricle being enlarged, and its walls increased in thickness, while the left ventricle, though increased in size, retains the usual thickness of its walls.

The circumference of the heart in cases of bronchitis in males, averaged 118·6 lines, and ranged from 110 to 130 lines.

The girth of the right ventricle averaged 67·3 lines, and ranged from 60 to 76 lines; that of the left ventricle averaged 51·3 lines, and ranged from 50 to 54 lines.

The mean length of the cavity of the right ventricle was 50 lines, and it ranged from 48 to 51 lines; that of the left ventricle had a medium of 40·6 lines, and extremes of 36 and 45.

The walls of the right ventricle were on the average 3 lines in width, and ranged from 2 to 4·5 lines, and in one, case not included in the tables, of chronic bronchitis,—with curvation of the spine,—in a young man 17 years old, the parieties of the ventricle were fully 5 lines thick. The walls of the left ventricle had an average thickness of 5·8 lines, and ranged from 5·5 to 6 lines at the broadest part. The septum had a medium thickness of 5·08 lines, and a range of from 4 to 6 lines.

The right auriculo-ventricular aperture had a medium circumference of 58·6 lines, and extremes of 54 to 62 lines. The pulmonic orifice a medium of 46 lines, and extremes of 45 and 48. The left auriculo-ventricular aperture, in the three cases measured, was 54 lines, and the aortic had a medium of 39 lines, and extremes of 36 and 42 lines. It will thus be seen, that the girth of the right ventricle was $\frac{1}{3 \cdot 5}$ greater than that of the left, and the length of the cavity of the right ventricle 1-4th greater than that of the left. The capacity of all the orifices was above the average, and the pulmonic exceeded the aortic aperture by 1-6th, and the mitral exceeded the aortic by 1-3d, while the tricuspid orifice was as usual one-half larger than the aortic. A comparison of the dimensions of the heart in females in the cases of bronchitis, with those of the healthy organ, show similar results, but in only one case did the walls of the right ventricle attain a thickness of 4 lines, and in this instance there was chronic bronchitis, with curvature of the spine, and the subject was 25 years of age.

The fossa of the foramen ovale in cases of chronic bronchitis, becomes considerably enlarged, and the valve is not unfrequently dilated and protruded into the left auricle. In one case, of which I have the preparation, a large sac is thus produced. When the fossa is dilated, the portion of the fold which ordinarily overlaps the isthmus is frequently drawn down, so that it may be found scarcely to reach above the edge of the opening; and when the valve has not become adherent the aperture might so be reopened; but, though I have seen several cases of very enlarged fossa with an unadherent valve, I have never met with one in which the fœtal aperture was restored.

Though the disproportion between the size of the right and left ventricles of the heart, and especially the greater size of the pulmonic than of the aortic aperture exists at all ages, except in very early life, or before birth; and the experiments of Legallois have shown that is not dependent on the mode in which death takes place, it is certainly much aggravated in all cases in which there exists any obstruction to the transmission of the blood through the lungs. This is most marked in cases in which the obstruction has been of long duration,

but I have seen the orifice of the pulmonary artery very considerably expanded in persons who have died of acute bronchitis and pneumonia, after short periods of illness.

6. In persons who have died of phthisis, the heart is usually smaller than in those who have sank from other diseases; but the decrease in size is less than obtains in some other forms of disease, and is chiefly due to the diminution in the size of the cavities and in the capacity of the orifices, while both the right and left ventricles are thicker than in other chronic cases where there is no pulmonary disease.

The average circumference of the heart in males who died of phthisis was 94·5 lines, and the extremes 92 and 97.

The girth of the right ventricle averaged 49·5 lines, and its extremes were 49 and 50; that of the left ventricle had a mean of 45 lines, and extremes of 42 and 48.

The length of the right ventricle had an average of 43·3 lines, and extremes of 36 and 48; that of the left ventricle averaged 37·3, and its extremes were 36 and 39.

The walls of the right ventricle had a mean thickness of 2 lines; those of the left ventricle, a mean of 6·3 lines, and extremes of 5 and 7 lines.

The right auriculo-ventricular aperture had a medium circumference of 49·3 lines, and extremes of 43 and 51. The pulmonic orifice a mean of 38·6 lines, and extremes of 29 and 48. The left auriculo-ventricular aperture a medium of 45·3, and extremes of 37 and 51; and the aortic aperture a mean of 34·6, and extremes of 26 and 42. It will thus be seen, that while the ventricular walls retain their usual thickness, or exceed it, the length of the cavities, and the capacity of the orifices are much less than usual, and the difference is the more apparent when the dimensions of the heart in phthisis are compared with those of the organ in other chronic affections, unconnected with disease in the lungs. It will also be observed that the proportion between the cavities and orifices on the two sides of the heart is not materially different from that which obtains in the healthy organ; the right side being, however, somewhat less than natural, relatively, to the left. As some of the results here deduced differ from those of M. Bizot, who states the heart in phthisis to be smaller in all its dimensions, it is right to state that the data here analysed are much fewer than those collected by that observer.

7. The observations are too few to warrant definite conclusions as to the different dimensions of the heart in persons who have died of acute and chronic diseases, but they would appear to indicate that the chief difference consists in the diminished thickness of the walls of the left ventricle.

It will be observed, from the annexed table, that though the series of observations collected by M. Bizot, Dr Reid, Dr Ranking, and myself, correspond to a considerable extent, there are differences between them. M. Bizot's measurements of the width of the walls are

TABLE XII.—For the purpose of comparison I have prepared the following table, which exhibits the mean dimensions of the *healthy heart* as deduced by M. Bizot, Dr Reid, and Dr Ranking. I have not thought it necessary to include M. Bouillaud's observations, for they are few in number, include persons of different ages and of both sexes, and are very imperfect as to the measurements in each case.

	Bizot.[1]		Dr Reid.[2]		Dr Ranking.[3]		Own Cases.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
Circumference of Heart,					113·	99·5	102·7	104·9
Length of cavity of right ventricle,	37·2	34·					43·3	44·
„ „ left „	34·39	31·03					37·6	37·1
Thickness of walls of right „ base,	1·9	1·6					1·85	1·86
„ „ „ „ midpoint,	1·4	1·29	2	1·75	1·8	1·5	1·98	2·0
„ „ „ „ apex,	1·03	·93					1·42	1·3
„ „ left „ base,	4·5	4·3					5·15	4·9
„ „ „ „ midpoint,	5·15	4·8	5·6	4·65	6·25	5·5	6·	6·
„ „ „ „ apex,	3·7	3·4					2·4	2·5
Thickness of septum, „	4·9	4·4	5·6	4·25	5·2	4·5	5·73	4·7
Circum. of right auriculo-ventricular aperture,	54·19	48·25	59·7	55·	52·75	47·	53·9	51·
„ pulmonic,	32·34	30·1	42·	39·3	30·3	28·1	40·	39·3
„ left auriculo-ventricular aperture,	45·5	41·33	52·	47·3	44·25	38·75	45·2	45·
„ aortic,	31·24	28·8	36·	34·	29·5	27·5	35·6	34·

The measurements of M. Bizot are calculated from the examination of the hearts of 60 males and females, from 16 to 79 years of age. The thickness of the septum refers only to persons between 30 and 50 years of age.—The observations of Dr Reid are based upon the measurement of the hearts of 16 males and 9 females, adults, weighing from 7 to 13½ oz.—Dr Ranking's conclusions are drawn from the weight of the heart in 15 males from 26 to 65 years of age, and 17 females from 18 to 62 years of age.—My own observations include the hearts of 16 males and 13 females, dying of various different forms of disease in persons from 20 to 66 years of age, in whom the hearts weighed from 6 oz. 9½ dr. to 12 oz.—Dr Reid's and Dr Ranking's observations are given in the original papers in fractional parts of an English inch ; I have here reduced them to French lines for facility of comparison.

[1] Mem. de la Soc. Med. d'Observat., t. i., p. 262, Paris, 1837. Anatomical, and Pathological Researches, Ed. 1848, p. 373.

[2] L. & E. Monthly Journal, 1842 ; and Physiological,

[3] London Medical Gazette, vol. xxix. ; vol. i. 1841–42, p. 903.

TABLE XIII.—Showing the dimensions of the heart in males in cases of hypertrophy, unconnected with marked obstruction, in cases of obstruction at the aortic orifice or in the aorta, and in cases of incompetency of the aortic valves.

	Hypertrophy.			Obstruction at Aortic Orifice or in Aorta.			Incompetency of Aortic Valves.		
	Mean.	Lightest.	Heaviest.	Mean.	Lightest.	Heaviest.	Mean.	Lightest.	Heaviest.
	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
Circumference of heart, . . .	129·	104·	182	119·6	102·	138	124·3	104·	171
Girth of right ventricle, . . .	66·5	54	96	61·3	54	72	60·8	44	90
„ left ventricle, . . .	62·5	50	86	58·3	48	66	63·5	60	81
Length of cavity of right ventricle, .	51·2	48	62	43·	42	48	55·7	38	72
„ „ left ventricle, .	45·7	40	51	38·8	36	42	45·4	38	60
Thickness of walls of right ventricle base,	2·2	2	3	2·3	3	2	2·32	2	2·5
„ „ „ „ midpoint	2·0	1·5	2·5	2·7	3	3	3·25	3	5·
„ „ „ „ apex,	1·7	1·	2·	1·5	2	1·5	1·82	1	2·
„ „ left ventricle base,	5·7	5·	8	8·1	6	10	6·6	7	10·
„ „ „ „ midpoint	8·5	7·	11	8·06	8	11	6·7	7	5·5
„ „ „ „ apex,	2·8	2·	3·5	2·47	3	2·5	2·35	2·5	1
Thickness of septum, ...	8·2	7·	10	6·6		8	7·5	9	
Circumference of right auriculo-ventricular aperture, ...	55·7	42	63	57·7	60	60	57·4	48	60
Circumference of pulmonic aperture, .	45·	35	54	44·2	42	42	45·4	39	51
„ of left auriculo-ventricular aperture, ...	51·2	39	60	49·8	48	51	53·5	42	60
Circumference of aortic aperture, .	39·2	34	42	37·7	39	36	39·	36	45

The cases of hypertrophy analysed in this table are four, in persons from 44 to 65 years, and with a mean age of 51 years. The hearts in the cases, weighed from 12 oz. 12 dr. to 40 oz. 12 dr., the mean weight being 21 oz. 4 dr.

The cases of obstruction are eight in number, in persons from 24 to 78 years of age, and with an average of 53·7. The weights of the heart range from 10 oz. 12 dr. to 21 oz., the mean being 15 oz. 2 dr.

The cases of incompetency are seven, the ages of the subjects from 32 to 57, the weights of the heart from 11 oz. 8 dr. to 34 oz., and the mean 19 oz. $3\frac{3}{7}$ dr.

TABLE XIV.—Showing the dimensions of the heart in females, in cases of obstruction at the aortic orifice, of incompetency of the aortic valves, of mitral valvular disease, and of combined mitral and aortic valvular disease.

	Obstruction at Aortic Orifice.			Incompetency of Aortic Valves.	Mitral Valvular Disease.			Combined Mitral and Aortic Disease.		
	Mean.	Lightest.	Heaviest.		Mean.	Lightest.	Heaviest.	Mean.	Lightest.	Heaviest.
	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
Circumference of heart,	105·6	104·	120·		111·8	108·	127·	118·	126·	110·
Girth of right ventricle,	56·	60·	60·		62·8	60·	75·	63·5	72·	55·
„ left ventricle,	49·6	44·	60·		49·	48·	52·	54·5	54·	55·
Length of cavity of right ventricle,	42·3	39·	48·	52·	46·	42·	51·			43·
„ „ left ventricle,	37·	39·	42·	52·	39·	36·	42·			42·
Thick. of the walls of the right ventricle base,	2·16	25·	2·	2·	2·2	2·	2·	2·6	2·	2·
„ „ „ midpoint,	2·3	3·	2·	2·	2·2	2·	3·	2·6	2·	2·
„ „ „ apex,	2·	2·	2·	1·5	1·3	1·	1·5	2·1	1·	1·5
„ „ left ventricle base,	6·	5·	5·	6·	5·	4·	5·	7·3	6·	7·
„ „ „ midpoint,	7·6	8·	7·	7·	5·4	5·	5·	9·	6·	10·
„ „ „ apex,	2·8	3·	2·5	3·	2·2	2·	2·	2·6	3·	2·
Thickness of septum,	5·5	6·	5·		5·	4·	5·		5·	
Circum. of right auriculo-ventricular aperture,	57·	54·	60·		51·2	57·	54·	48·	42·	51·
„ of pulmonic aperture,	36·5	34·	39·	39·	34·6	39·	39·	35·6	36·	32·
„ of left auriculo-ventricular aperture,	43·5	42·	45·		23·	39·	12·	34·3	42·	48·
„ of aortic aperture,	32·5	32·	33·	29·	29·	30·	33·	30·6	29·	35·

The cases of aortic valvular obstruction are only three in number, the subjects were 75, 75, and 60 years of age, and the hearts weighed 10 oz. 2 dr., 10 oz. 12 dr., and 14 oz. respectively.

In the single case of incompetency of the aortic valves, the subject was 46 years of age, and the heart weighed 16 oz. The cases of mitral valvular disease are five in number, the ages of the subjects ranged from 23 to 68, and the mean age was 39·4, the hearts weighed from 8 oz., 8 dr. to 15 oz. 12 dr., the mean weight being 11 oz. $13\frac{3}{5}$ dr. The cases of combined mitral and aortic valvular disease are three in number, the ages from 18 to 63, the mean being 40 years, the weights of the heart ranged from 13 oz. 8 dr. to 22 oz., and the mean weight was 19 oz. $2\frac{2}{3}$ dr. The dimensions of two cases of combined disease in females, 11 and 12 years of age, included in the general table, are excluded from this table.

generally the least, those of Dr Reid the largest. In the dimensions of the orifices, Dr Ranking's observations show the smallest size, and Dr Reid's the largest. The seat at which M. Bizot took the dimensions of the aortic and pulmonic orifices differed from that selected by Dr Ranking, the former having measured the opening at the level of the free border of the valves, while the latter took the dimensions on the line of their insertions; the precise point measured by Dr Reid is not mentioned. All three observers estimated the circumference of the orifices after they had been laid open, whereas my own measurements were made while the apertures were entire by graduated balls passed through them, and therefore indicate their absolute capacity. After division the fibrous ring of the arterial orifices, especially that of the pulmonary artery, contracts, and it becomes extremely difficult, or impossible, to judge of the size, which should be assigned to the opening. The mode in which the length of the cavity of the right ventricle is estimated by M. Bizot and myself, also differs; his measurements refer to the length of a line from the base to the apex; mine, to that of a line following the course of the ventricle from the orifice of the pulmonary artery to the apex. It will be seen that the relative size of the heart in males and females in my own table differs but little, much less than is the case in the other observations. This is explained, as before mentioned, by the female hearts measured being, almost all of them, those of persons who had died of acute diseases, while the majority of the male hearts were from cases of chronic disease.

PART IV.—*Dimensions of the Diseased Heart.*

INFERENCES.

1st. The heart attained the greatest increase of size in the cases in which the hypertrophy and dilatation were unconnected with any marked disease of the valves or aorta, and the enlargement in these instances was not confined to the left cavities, but involved, though to a less degree, those of the right side also.

2d. In the cases of obstructive disease of the aortic valves, or of the aorta, the heart was also very greatly enlarged, and the enlargement involved both the left and right cavities of the organ, but was not so great as in the cases of hypertrophy unconnected with valvular or aortic disease. It was also by no means proportionate to the amount of impediment to the circulation, being in some cases very great where the obstruction was only trivial, and in others slight where very great obstruction existed.

3d. In the cases of incompetency of the aortic valves, the capacity of the left ventricle was greater than either in those of simple hypertrophy or obstructive disease, but its walls were usually less increased in thickness than in those diseases. The hypertrophy involved both sides of the heart, but the right ventricle attained a greater thickness in this than in either of the other forms of disease.

It will be seen from the table of dimensions of the diseased heart in males, that the circumference of the organ averaged in the cases of hypertrophy, obstructive disease, and incompetency, respectively, 129, 119, and 124 lines, and attained the extremes of 182 lines, or 15 French inches, or 16 English; 138 lines, or $11\frac{1}{2}$ French inches, or about 12 inches English; and 171 lines, or $14\frac{1}{4}$ French inches, or about 15 English. The girth of the right ventricle measured externally averaged, in cases of hypertrophy, 66 lines, of obstructive disease 61 lines, and of incompetency 60 lines, and attained the extremes of 96, 72 and 90 lines. The girth of the left ventricle averaged in three several forms of disease 62, 58 and 63 lines, and attained the extremes of 86, 66 and 81 lines.

The length of the cavity of the right ventricle averaged in the three several forms of disease 51, 43 and 55 lines, and attained extremes of 62, 48 and 72 lines; the length of the left ventricle averaged 45, 38 and 45 lines, and the extreme lengths were 51, 42 and 60 lines. The walls of the right ventricle, measured at the thickest part, were 2·2 lines in width, in cases of hypertrophy, 2·7 lines in cases of obstructive disease, and 3·25 in cases of incompetency; and their minimum and maximum thickness in these several forms of disease were 2 and 3 lines, 3 lines, and 3 and 5 lines. The walls of the left ventricle averaged in width in cases of hypertrophy 8·7 lines, in obstructive disease 8·1 lines, and in incompetency 6·7 lines, and the extremes were respectively 7 and 11 lines, 6 and 11 lines, and 6 and 10 lines.

The septum had an average thickness in the cases of hypertrophy of 8 lines, and its extremes were 5 and 10 lines: in the cases of obstruction of 6·6 lines, and its extremes 6 and 8; and in the cases of incompetency, in the only two cases in which it was measured, it was 6 and 9 lines thick.

The capacity of the orifices in these diseases varied according to the form of the affection. In the cases of hypertrophy they were all above the healthy standard. The aortic orifice averaged 39 lines in circumference, and its extreme dimensions were 34 and 42 lines. The left auriculo-ventricular aperture averaged 51 lines, and its extremes were 39 and 60 lines. The pulmonic aperture averaged 45 lines, and ranged from 35 to 54 lines, and the right auriculo-ventricular aperture averaged 55 lines, and ranged from 42 to 63 lines.

In the instances of obstructive disease the aortic orifice was in some cases greater, in others, less in circumference than natural. Its average capacity was 37 lines, and its extreme dimensions 33 and 42 lines, and all the other apertures exceeded the natural size. The mitral orifice averaged 49, and ranged from 48 to 54 lines, the pulmonic averaged 44 lines, and ranged from 42 to 48, and the tricuspid averaged 57, and ranged from 54 to 60 lines.

In cases of incompetency of the valves the capacity of the aortic orifice also varied with the nature of the disease, in some instances

the valves being healthy, but incapable of closing the aperture from the dilatation which it had undergone; while in others the capacity of the aperture was not greater than natural, but the valves being diseased were incapable of closing it. The mean circumference of the orifice in these cases was 39 lines, and it ranged from 30 to 45 lines; but in the case in which the capacity of the orifice was only 30 lines, the incompetency was the result of laceration of the valves, from violent muscular exertion, and as there was no reason to suspect the existence of any disease of the heart before the occurrence of the accident, it is probable that the aperture had contracted so as to afford some compensation for the imperfection of the valves. In these cases the capacity of all the other orifices was above the healthy standard, the mitral had a mean circumference of 53 lines, and its extreme dimensions were 42 and 60 lines, the pulmonic a mean of 45, and extremes of 59 and 51 lines, and the tricuspid a mean of 57, and extremes of 48 and 60 lines.

The auricles were greatly enlarged in all three forms of disease, and generally increased in thickness; the right ventricle being from $1\frac{1}{2}$ and to 2 lines thick, the left from $1\frac{1}{2}$ to 2 or $2\frac{1}{2}$ lines. The coronary arteries, when measured, were also found above their natural size.

Though the walls of the left ventricle did not exceed 11 lines or about one English inch in thickness, in any of the cases included in the table; in another instance, in a man 74 years, in which there was some valvular thickening with dilatation of the aorta, the organ attained the weight of 19 oz. 8 dr., and the left ventricle near the base measured 14 lines, or nearly an inch and a quarter English. The cases included in the table, also, do not afford evidences of the extent of which the aortic office may be contracted; in one case, that of a female 75 years of age, which has before been several times alluded to, the aortic aperture was a mere slit 10 lines long, and the thickened and ossified valves did not admit of being separated for more than 3 or 4 lines; yet, though the division probably originated in a congenital malformation of the valves, and had therefore very slowly advanced, the parieties of the left ventricle were only 8 lines in thickness, and those of the right 2 lines. In a case of obstructive and regurgitant disease of the aortic valves, not included in the tables, the septum had a width of 8·7 lines or $\frac{3}{4}$ of an English inch.

The shape of the heart varies considerably in these different forms of disease, according to the seat and extent of the hypertrophy and dilatation. In cases of obstructive disease, whether the impediment be seated in the aortic valves or in the ascending portion of the aorta, the cavity of the left ventricle becomes peculiarly elongated, and the walls are ordinarily thicker near the base than elsewhere, and hence the whole organ has an acutely triangular form, and is much longer in its longitudinal than in its transverse diameter. On the other hand, in cases of regurgitation through the aortic valves, while the left ventricle is elongated, the thickening of the parieties is more

equably distributed throughout the cavity, and the apex, instead of being pointed, is obtuse, and thus the whole organ assumes a more oblong form, of which, however, the longest diameter is the longitudinal. In cases of regurgitation through the mitral aperture, and where there is considerable disease both of the mitral and aortic valves, the left ventricle, instead of being elongated, is expanded laterally, and especially at the apex, so that the organ has a more obtusely triangular form, and is generally broader from side to side than longitudinally. In these forms of disease, the right ventricle though implicated to a greater or less degree, is not so much so as to affect the shape of the organ generally, but in cases of obstructive disease of the mitral valve, the left ventricle, as will be shown more particularly below, is little, if at all, altered in dimensions, while the right ventricle is hypertrophied and very greatly dilated; and hence the heart, though not very greatly larger than natural, is much broader, and is very obtuse at the apex. These differences of form are so striking and peculiar, that in many cases of diseased heart, it is easy to predicate before the organ is laid open, the nature of the affection which will be found.

4. In cases of contraction of the left auriculo-ventricular aperture, uncomplicated with disease of the other orifices, the heart does not attain so great an increase of size as in the forms of disease before mentioned, for the enlargement is chiefly limited to the right side of the heart. Indeed, in some cases, more especially in young subjects, the capacity of the left ventricle, the thickness of its walls, and the circumference of the aortic orifice, are found not at all to exceed the natural dimensions, or even to fall below the healthy standard.

It will be observed, that the comparison between the cases of mitral valvular disease and obstructive disease, in females, would lead to the conclusion, that the heart in the latter disease is smaller than in the former; but this is doubtless an erroneous inference, founded upon the smallness of the number of cases of obstructive disease in females. A more correct impression is most probably to be gained by comparing the dimensions in cases of mitral valvular disease in females, with those in the other forms of disease in males;—for though the heart does not in any disease attain so great a size in females as in males, the general effect of different forms of disease on its nutrition, may be inferred to be similar in the two sexes. The average circumference of the heart in the cases of mitral valvular disease, given in the table, will be seen to have been only 111 lines, and in no case did the circumference exceed 127 lines or $10\frac{1}{2}$ French, or upwards of 11 English inches. The girth of the right ventricle averaged 62 lines, and in one case amounted to 75 lines. The girth of the left ventricle was very much less than that of the right, or only 49 lines on the average, and in no case more than 52 lines. The length of the right ventricle was on the average 46 lines, and its greatest length was 51 lines.

The average length of the left ventricle was 39, and the extreme length 42 lines. The walls of the right ventricle averaged 2·2 lines in thickness, and ranged from 2 to 3 lines; those of the left ventricle had an average thickness of 5 lines, and ranged from 4 to 6 lines.

The mitral aperture varied considerably in the amount of contraction. In two cases, one of which is not included in the tables, it only admitted a cylinder 11 lines in circumference; in a third instance its circumference was only 18 lines, and in three others it was 22, 24, and 39 lines, the mean being 23 lines. The aortic orifice was also below the natural size, or on the average only 29 lines in circumference, and its dimensions ranged from 25 to 33 lines.

The orifices on the right side of the heart were, on the contrary, ordinarily larger in comparison. The pulmonic aperture averaged $34\frac{1}{2}$ lines in circumference, and ranged from 29 to 39 lines; the tricuspid averaged 51 lines, and ranged from 40 to 57 lines. The left auricle was in all the cases greatly dilated, and its walls varied in thickness from $1\frac{1}{2}$ to 2 lines; the right auricle was generally still more enlarged, and its walls had a width of from 1 to $1\frac{1}{2}$ lines.

It will thus be seen that, while the left ventricle and aortic orifice were below the healthy standard, the dimensions of the other parts of the organ exceeded the usual size, so that it may be inferred that the operation of this form of disease is either to cause atrophy of the left ventricle, or, when the disease commences in early life, to prevent the full development of that cavity. It will also be observed that the hypertrophy of the right ventricle did not in any cases of mitral valvular disease equal that which was found in the two cases of chronic bronchitis with curvature of the spine.

5. The tables contain only one case of regurgitation through the left auriculo-ventricular aperture, from dilatation of the orifice without disease of the valves; and in this the subject was a child eight years of age, so that it cannot be compared with the other cases; allowance, however, being made for the age of the patient, the heart was much more enlarged than in the cases of mitral valvular contraction, and the left ventricle also was increased in size.

6. In the cases in which both the aortic and mitral valves were diseased, the dimensions of the heart were intermediate between those in cases of uncomplicated mitral and aortic disease. The enlargement, instead of being chiefly limited to the right ventricle, involved the left also; and the length of the cavities, and the thickness of the walls, exceeded those in cases of mitral disease alone, but were less than in cases of aortic disease.

The girth of the right ventricle in two of the cases in the table was 72 and 55 lines; of the left ventricle 54 and 55 lines. The mean thickness of the parieties of the right ventricle was 2·6 lines, and the extremes 2 lines and 4 lines; the mean width of the walls of the left ventricle was 9 lines, and the extremes 6 and 11 lines. In the only case in which the width of the septum was measured, it

was 5 lines. In these instances the subjects were females, but I have notes of other cases of combined aortic and mitral disease in males. The subject of one of these, 36 years of age, and the heart weighed 17 oz. 8 dr., the dimensions are not given in the tables. The aortic valves were incompetent, and the mitral aperture so contracted as only to admit the handle of a scalpel, indicating a circumference of about 12 lines. The cavity of the right ventricle was 42 lines long, the left 37 lines. The walls of the right ventricle had a width of from $4\frac{1}{2}$ to $5\frac{3}{4}$ lines, and the left ventricle of $5\frac{3}{4}$ at the base and midpoint, and $3\frac{1}{2}$ near the apex. The longitudinal diameter of the heart was 42 lines, the transverse 54 lines. The walls of the right auricle averaged $1\frac{1}{2}$ lines in thickness, those of the left auricle 2 lines. In a second case the subject was a boy 18 years of age, and the disease was the sequence of rheumatism four years before. The organ weighed 16 oz. The pericardium was adherent, and both the aortic and mitral valves were thickened and incompetent. The circumference of the heart measured 122 lines. The cavity of the right ventricle was 27 lines in length, the left 48 lines. The walls of the right ventricle measured 3 lines in thickness, those of the left 8 lines. The aortic orifice had a capacity of 42 lines, the mitral of 45, the pulmonic of 42, and the tricuspid of 54. The longitudinal diameter of the heart was 42 lines, the transverse 56 lines.

7. Only one case of combined obstruction of the right and left auriculo-ventricular apertures is included in the tables; and in this instance the most remarkable feature was the great degree of contraction which existed in both openings, the left auriculo-ventricular aperture having a circumference of only 18 lines, and the right of only 21 lines; yet with this very great amount of obstruction, which had probably commenced before birth, the walls of the right ventricle was only 1·5 lines in width, and those of the left ventricle 4 lines. The right auricle was very greatly dilated, and its walls were from $\frac{1}{2}$ a line to 2 lines in thickness; the right ventricle was somewhat dilated. The left auricle was also dilated, and the left ventricle, though to a less degree. The aortic orifice measured 30 lines in circumference, the pulmonic 35 lines. The aortic valves were also thickened and adherent. The subject of the case was a female 37 years of age.

8. The dimensions of the heart in cases of malformation vary with the kind of the deviation from the natural structure. The most frequent form of irregular development is that in which the pulmonary orifice is contracted, and the septum ventriculorum imperfect, or the foramen ovale open. Three cases of this description are included in the tables. The subjects were males, 15 and 20 years of age, and a female, 19 years old. In the first case the pulmonary artery was so contracted, as only to give passage to a ball 13 lines in circumference, in the second of 12 lines, and in the last of 8 lines. In the first and third cases the aorta arose from both ventricles, in the second the foramen ovale was open. In each of

these cases the right ventricle was very greatly hypertrophied, measuring 54, 66, and 84 lines in circumference externally, while the left ventricle had only a girth of 42, 42, and 48 lines. The walls of the right ventricle measured $5\frac{1}{2}$, 7, and 4 lines, those of the left ventricle $4\frac{1}{2}$, 6, and 6 lines. It will thus be seen that, though the enlargement was chiefly on the right side, allowance being made for the age and sex of the subjects, the left ventricle must also have partaken of the hypertrophy; and this is still more obvious in cases of malformation in younger persons. The auricles were also greatly dilated and hypertrophied, measuring from $1\frac{1}{2}$ to 2 or $2\frac{1}{2}$ lines.

In another instance in which malformation of a similar description was found in a child $6\frac{1}{2}$ years old, the pulmonary orifice only admitted a cylinder $6\frac{1}{2}$ lines in circumference; and in a fifth, in an infant 2 years and 5 months old, it was a mere slit 2 lines long. In these cases the walls of the right ventricle were much hypertrophied, measuring in each case, at the thickest point, 4 lines; while the left ventricle measured in the former case 6 lines, in the latter 3; and the density and resistance of the walls of the right ventricle, as in all the other cases of this kind of malformation, contrasted remarkably with the flaccidity of those of the left ventricle.

The septum is not, in these cases, hypertrophied in proportion to the walls of the right ventricle; indeed, that portion of the heart is more implicated in diseases of the left ventricle than in those of the right. Thus, while in cases of hypertrophy without valvular disease and in others of obstructive disease it had a thickness nearly equal to that of the outer walls of the left ventricle, or of from 9 to 10 and 11 lines;—in the cases of malformation in which the parieties of the right ventricle were from 6 to 7 lines thick, or fully three times the natural width, the septum measured only 6 lines in thickness, or was not materially wider than usual.

Different opinions have prevailed as to the occasion of the hypertrophy of the right ventricle in cases of malformation. It has been supposed to be due to the entrance of aerated blood into the right cavities of the heart; but this cannot be the true explanation, for, in the most remarkable cases of hypertrophy, the course of the blood must necessarily be from the right ventricle or auricle into the left. The more correct theory is, doubtless, that which ascribes the hypertrophy to the effort to overcome the obstacle to the flow of blood from the ventricle, through the contracted pulmonary orifice. Another cause, which would have a similar effect, obtains in many cases, and appears to have been overlooked. I allude to the increased action of the right ventricle consequent upon the aorta arising in part from that cavity, and the share which it consequently takes in the maintenance of the systemic circulation. In one of the cases of malformation which I have mentioned, while the parieties of the infundibular portion of the right ventricle, upon which the stress from the contracted pulmonary orifice would chiefly fall, had only an extreme width of 4 lines; those of the sinus, which were chiefly con-

cerned in propelling the blood from the right ventricle into the aorta, were 7 lines in thickness. It is true that the walls of the sinus are ordinarily thicker than those of the infundibular portion, but the difference is ordinarily inconsiderable.

It has been thought that the condition of the right ventricle, in some cases of malformation, affords an exception to the general rule of the non-occurrence of true concentric hypertrophy; and specimens, when first removed from the body, and preparations preserved in museums, not unfrequently present the appearance of increased thickening of the walls of the right ventricle, with diminution of its cavity. These appearances are, however, I believe, deceptive, and depend upon specimens being examined, or the preparations having been immersed in spirit, before the tonic contraction of the muscular fibres has subsided. I have seen cases of malformation in which, when the right ventricle was first laid open, there seemed to be absolutely no cavity, but in which, after maceration, the ventricle proved to be unusually large.

In cases in which the maintainence of the circulation is thrown upon the left ventricle, as when the right auriculo-ventricular aperture is obliterated, or when that opening or the pulmonary orifice is greatly contracted after the complete development of the septum of the ventricles, the left ventricle becomes much hypertrophied and dilated, while the right ventricle undergoes a proportionate decrease in size. In some of these cases, indeed, the right ventricle becomes reduced to a small hollow, about the size of a pea, which is surrounded by thick ventricular walls; but this condition is clearly one of atrophy, not of hypertrophy, and the defective nutrition of the muscular substance of the right ventricle from disuse, is shown by its unusual paleness and flaccidity, and by the looseness of its texture.

General Remarks on the Dimensions of the Diseased Heart.—The dimensions of the heart in disease have attracted the attention of most systematic writers, and especially of Laennec, Bertin, Lobstein, Cruveilhier, and Hope; and the series of observations published by Bouillaud, and the incidental allusions to the measurements of the organ in cases of disease, contained in Dr Ranking's valuable paper, throw much light on the changes which it undergoes. It may not be without interest to compare the observations of these writers with the results obtained from the analysis of the cases now published.

The circumference of the largest heart measured by M. Bouillaud was 12 French inches, and of that mentioned by Dr Ranking $12\frac{3}{4}$ English inches—dimensions which very nearly correspond. In my own tables the dimensions are given of the heart of a male in a case of hypertrophy without valvular disease, which measured 15 inches and 2 lines French, or somewhat above 16 English inches in circumference, and one of incompetency of the aortic valves, in which the circumference of the heart was 14 inches and 3 lines, or

about 15 English inches. In females the extreme circumference of the heart was 10 inches and 7 lines, or about 11 English inches, in a case of mitral valvular disease, and 10 inches and 6 lines in one of combined aortic and mitral disease, and 10·0 inches in one of obstructive disease at the aortic orifice.[1]

The thickness of the walls of the right ventricle is stated by Laennec[2] rarely to exceed 4 or 5 lines; and in the observations of M. Bouillaud, no case is given in which it measured more than $4\frac{1}{2}$ lines. Bertin,[3] however, describes a case in which there existed a congenital contraction of the orifice of the pulmonary artery from adhesion of the valves, with patency of the foramen ovale, in a female 57 years of age; and the cavity of the right ventricle was less than natural, and its walls from 16 to 11 lines thick. M. Burnet[4] relates a somewhat similar case of congenital disease, in which, however, the foramen ovale was closed, in a girl 7 years of age, in whom the walls of the right ventricle were nearly an inch in width, and the cavity almost obliterated; and Louis[5] has described a case of contraction of the pulmonary orifice with imperfection of the septum ventricular curve, in a man 25 years of age, in whom the parieties of the right ventricle had a width of 8 to 10 lines. Hope, in cases of congenital obstruction of the pulmonic aperture with an open foramen ovale, in a girl 8 years of age, and of aneurism of the aorta with regurgitation through the aortic valves, in a man 25 years of age, found the walls of the right ventricle $\frac{1}{2}$ an English inch, or $5\frac{3}{4}$ lines thick. Dr Ranking refers to a case in which the pulmonary artery was contracted, and the foramen ovale unclosed; and to another, in which the aorta arose from both ventricles, with pulmonic valvular obstruction, in which the parieties of the right ventricle measured 17-48ths (about 4 lines), and 44-48ths (10 lines) of an English inch in thickness. In none of the observations now published were the walls of the right ventricle so extremely hypertrophied as in the cases last named. The greatest thickness of the parieties being 5 lines in a case of chronic bronchitis, with deformed spine, 5 lines in cases of obstructive disease, and of incompetency of the aortic valves, $5\frac{3}{4}$ lines in a case of combined mitral and aortic valvular disease, and 7 lines in cases of congenital obstruction at the pulmonic orifice, with a patent foramen ovale in one, and deficiency of the septum ventriculorum in the other. In the latter case the subject was a female, while in all the others they were males; with this exception, the walls of the right ventricle did not exceed 4 lines in thickness in

[1] M. Bouillaud, for purposes of comparison, gives the weight and dimensions of the heart of an ox; the weight was 66 oz. 7 dr. avoird., and the circumference 18 French inches.

[2] Diseases of the Chest, 4th Ed. Forbes' Translation, 1834, p. 547.

[3] Maladies du Cœur. Paris, 1824, p. 326. Obs. 87. See also Bouillaud, Traité Clinique, 2me Ed., 1841, t. ii., p. 273. Obs. 126.

[4] Bouillaud, p. 281. Obs. 128; and Journal Hebd. de Med., 1831.

[5] Mém. ou Rechérches Anatomico-Pathologiques. Paris 1826, p. 313. Obs. 10. See also Arch. Gen. de Med., 2mo serie, t. iii., 1823.

females, and the case in which they attained this width was one of combined aortic and mitral valvular disease.

The parieties of the left ventricle are stated by Laennec to have been seen by him an inch, or even 18 lines thick, or double or triple the size in the sound state. This statement is repeated by Elliotson,[1] and Hope mentions, that the walls of the left ventricle may attain a thickness of one, one and a half, or, according to some, of two inches; and he mentions a case in which they were $1\frac{1}{2}$ inch thick in a case of regurgitation through the aortic orifice. The extreme thickness of the parieties of the left ventricle in the cases mentioned by Bouillaud, is of 13 lines, and of the hearts examined by Ranking, in one the walls of the left ventricle attained a width of one inch, or about $11\frac{1}{4}$ lines, and Bertin mentions a case in which they were upwards of one inch in thickness.

The greatest width of the parieties of the left ventricle in the cases given in the present memoir, was in males 11 lines, in cases of hypertrophy without valvular disease, and of aortic disease, and of dilatation of the aorta with chronic bronchitis; but another case is referred to, in which there was slight valvular disease with dilatation of the aorta, and the parieties of the left ventricle measured 14 lines in width. In females the greatest thickness of the walls of the left ventricle was 10 and 11 lines, in cases of combined aortic and mitral valvular disease.

The septum of the ventricles is mentioned by M. Bertin to have been found by him one inch in thickness; the greatest width in M. Bouillaud's observations is 10 lines, and Dr Ranking found it 38-48ths of an English inch (8·75 lines) in one case. In the present observations the greatest width in males is 10 and 11 lines in cases of hypertrophy without valvular disease, and 9 lines in cases of incompetency. In females the greatest width is 6 lines in a case of mitral valvular disease. I regret, that in the observations the dimensions of the septum were less frequently obtained than would have been desirable.

The parieties of the right auricle were found by M. Bouillaud, in two cases, to measure 3 and $3\frac{1}{2}$ lines in width, but his measurements are taken near the appendix, where ordinarily the walls are thicker, than across the middle of the sinus. Bertin also examined a heart in which the walls of the right ventricle had a width of 3 lines; and Dr Hope speaks of a thickness of $\frac{1}{4}$ of an inch as occasionally seen. In my own observations, the walls of the right auricle attained a width of 3 lines in only one case, that of the female, 19 years of age, in whom congenital contraction of the pulmonary aperture and deficiency of the septum of the ventricles, was combined with some disease of the tricuspid valves. The parieties of the right auricle measured $2\frac{1}{2}$ lines in a case of combined mitral and aortic disease, in a boy of 18 years of age, and attained the same thickness in

[1] Lumluyan Lectures.

a case of aneurism of the apex of the left ventricle, with open foramen ovale. They were also 2 lines in the case of great contraction of the mitral and tricuspid valves, and also in one of combined aortic and mitral valvular disease.

The maximum thickness of the parieties of the left auricle, mentioned by M. Bouillaud, is $2\frac{1}{2}$ lines. In my own observations its greatest width was two lines in cases of regurgitation through the left auriculo-ventricular aperture, obstructive and regurgitant disease of the mitral valves, combined aortic and mitral valvular disease, and regurgitation through the aortic aperture.

In the data collected by M. Bouillaud, the aortic aperture had a maximum capacity of 41 lines, and a minimum of 11 lines. Dr Hope mentions a case in which it was contracted to the size of a small pea. Its circumference in one case, measured by Dr Ranking, in a boy 18 years of age, was only one inch and 3-48ths (12 lines). In my own observations, its largest size in males is 45 lines, and its least 30 lines in cases of regurgitation through the aperture. In females its greatest circumference was in a case of combined aortic and mitral valvular disease, in which it measured 35 lines, while in another case before referred to, it was reduced to a mere slit 10 lines in length, and the thickened and ossified valves did not admit of being separated for more than 3 or 4 lines.

The pulmonic aperture in the hearts measured by M. Bouillaud, had a maximum circumference of 42 lines, and a minimum of 34 lines. The extreme size of the orifice in his observations is not mentioned by Dr Ranking; but he gives two instances in which the aperture was contracted, as is almost always the case, from congenital malformation, and in these the aperture measured one inch, and one inch and 40-48ths (rather more than 11 lines and 20·5 lines) in circumference. In the cases related by Bertin, Louis, and Burnet, the aperture had a diameter of only $2\frac{1}{2}$, $2\frac{1}{2}$, and $1\frac{1}{2}$ lines. In the case quoted by Hope, the pulmonary orifice would only admit a goose quill, and in one related by Dr Hunter, the pulmonary artery would only admit a small probe, though the patient was 13 years old. In the observations published in the paper, the largest size of the pulmonic orifice in males is 54 lines in a case of hypertrophy, unconnected with valvular disease; and in females 39 lines in a case of mitral valvular disease. In the cases of malformation before mentioned, in males 15 and 20 years of age, and in the female 19 years of age, the aperture had a circumference of only 13, 12, and 8 lines; and in children $6\frac{1}{2}$ years, and 2 years and 5 months old, it was only $6\frac{1}{2}$ lines, and 5 lines in circumference.

The left auriculo-venticular orifice in the cases of M. Bouillaud had an extreme capacity of 51 lines, and of 24 lines; but he mentions another case in which the aperture was so contracted as only to be 6 or 7 lines long and 5 lines wide, and two in which it was only 3 lines in its largest diameter, or 8 or 9 lines in circumference.[1] Dr Ranking

[1] Observations 112, 116, and 117.

measured two hearts, in which the mitral orifice had a circumference of $5\frac{1}{2}$ inches ($61\frac{3}{4}$ lines), and 5 inches and 2-48ths (54 lines), and he found none in which it was less than 2 inches and 19-48ths (27 lines). In the present series of measurements, the extreme capacity of the left auriculo-ventricular aperture is in males 60 lines in cases of hypertrophy, without valvular disease and incompetency of the aortic valves; and in females 45 lines in a case of obstruction at the aortic orifice. Its smallest size is in males, in a case of combined disease of the aortic and mitral valves, in which it would only admit the handle of a small scalpel, and had a circumference of not more than 12 lines. In females its smallest circumference was 12 and 18 lines, in cases of uncomplicated mitral valvular disease.

The right auriculo-ventricular aperture in the observations of M. Bouillaud had an extreme capacity of 69 lines, and the only case of contraction of this aperture to which he refers, is that of General Whiple,[1] in which there was an opening one inch long, and one line wide. Laennec does not refer to any case of extreme contraction of the tricuspid aperture; but Dr Forbes, in a note to his translation,[2] mentions one in which the aperture would only admit the thumb, and in which, like one I have before referred to, the mitral and the aortic apertures were also contracted. In the case related by M. Louis, the tricuspid aperture was also, as frequently happens, implicated in the disease of the pulmonic orifice. Dr Ranking measured a heart in which the right auriculo-ventricular aperture had a circumference of $6\frac{1}{2}$ English inches, or somewhat more than 73 lines. He does not refer to any cases of contraction. In the present series, the largest circumference of the tricuspid aperture in males is 63 lines and 60 lines, in cases of hypertrophy without valvular disease, and 60 lines in cases of obstructive disease, and incompetency of the aortic valves. In females the greatest circumference is 60 lines in a case of obstructive disease, and 57 lines in one of uncomplicated mitral valvular disease. The most extreme degree of contraction of this aperture with which I have met, was in a case of combined aortic, mitral, and tricuspid disease, not mentioned in the tables, in a female 32 years of age, in whom the tricuspid valves were adherent, so that the aperture would only admit the point of the forefinger, indicating a circumference of about 16 lines. The case of a similar kind mentioned in the paper, the aperture was 21 lines in circumference. Elsewhere I have expressed the opinion, that the disease of the valves in these cases, like the similar fusion of the valves of the pulmonary artery, is due to intra-uterine disease, and that they are, therefore, to be regarded as malformations.[3]

[1] Journal de Médicine et de Chir. Par MM. Corvisant, Leroux, etc. Vol. xix, p. 468.

[2] Page 590, 4th Ed. 1834.

[3] See Lectures on Malformations.—London Medical Times and Gazette, 1854.

It was my intention, before concluding this memoir, to have alluded to some of the preparations which illustrate the size and form of the heart in disease, contained in the museums of the metropolis; but the length to which the paper has already extended precludes my doing so.

In reviewing the communication, it will be seen that the conclusions drawn from the data collected, correspond generally with those of other observers, so far as relates to the weight and dimensions of the healthy heart; but in reference to the diseased organ, they present several differences which are not without interest. They show that the heart may attain a weight and size much larger than was previously supposed, and that the greatest amount of enlargement may be found when there is no material valvular disease, or

APPENDIX TO

Sex.	Age.	Weight.	Girth of Right Ventricle.	Girth of Left Ventricle.	Length of Cavity of Right Ventricle.	Length of Cavity of Left Ventricle.	Thickness of Walls of Right Ventricle.			Thickness of Walls of Left Ventricle.		
							Base.	Mid Point.	Apex.	Base.	Mid Point.	Apex.
	Years.	oz. dr.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.	lines.
M.	14	4 10	41	37	33	34	$1\frac{1}{2}$	2	$\frac{3}{4}$	4	5	3
M.	23	6 $9\frac{1}{2}$	54	44	42	36	2	$2\frac{1}{4}$	2	5	6	$3\frac{1}{2}$
M.	25	11 0			50	44	2	2	2	5	6	3
M.	35	8 0	58	48	44	36	$1\frac{1}{2}$	2	$1\frac{1}{2}$	$4\frac{1}{2}$	5	3
M.	38	11 0	66	50	51	45	2	$2\frac{1}{2}$	1_2	5	$5\frac{1}{2}$	2
F.	19	8 10	54	48	52	42	2	$2\frac{1}{2}$	1	4	4	3
F.	21	9 8	60	60	48	44	$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	$4\frac{1}{2}$	6	3
F.	21	7 13	58	48	42	36	2	$1\frac{1}{2}$	1	6	5	3
F.	20	9 5	60	50	46	41	2	$2\frac{1}{2}$	1	5	5	$2\frac{1}{2}$
F.	22	9 10	72	48	48	39	2	$1\frac{1}{2}$	1	5	6	$2\frac{1}{2}$
F.	28	8 8	54	48	48	36	$1\frac{1}{2}$	$2\frac{1}{2}$	$1\frac{1}{2}$	5	6	3
F.	37	7 1	60	48	42	36	1	$1\frac{1}{2}$	$\frac{1}{2}$	4	5	$2\frac{1}{2}$
F.	50	9 4	60	52	48	41	$2\frac{1}{2}$	$2\frac{1}{2}$	1	4	6	3
F.	56	10 8					$2\frac{1}{2}$	3	$2\frac{1}{2}$	6	8	2
F.	64	7 0			34	36	2	2	2	5	5	3
M.	18	16 0	66	56	57	48	3	$2\frac{1}{2}$	$1\frac{1}{2}$	7	8	4
F.	3 mos.	1 0	27	24	17	18	$1\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$	$2\frac{1}{2}$	$3\frac{1}{2}$	$1\frac{1}{4}$
F.	19 yrs.	17 8	84	48	53	46	4	4	3	5	6	2
F.	75	10 2	60	44	39	39	$2\frac{1}{2}$	3	2	5	8	3

any other obvious source of obstruction to which it can be referred. They indicate also a general relation between the dimensions and weight of the heart, and the different forms of disease of which it may be the seat, or in which it may be indirectly complicated. This, so far as I am aware, has not previously been investigated, but it must be admitted to be not only of pathological interest, but of practical importance.

In the following table are included the weights and dimensions of hearts examined since the first and second parts of the paper were written. They are not, therefore, included in the calculations, as to the weight of the heart, but they enter into those of the dimensions of the organ, both healthy and diseased.

GENERAL TABLES.

Thickness of Septum.	Circumference of Orifices.				Cause of Death, etc.
	Tricuspid.	Pulmonic.	Mitral.	Aortic.	
lin s	lines.	lines.	lines.	lines.	
4	42	36	39	30	Fever.
6	57	39	42	33	Peritonitis; waxy liver; morbus renum; weight of body 7 st. 7 lbs.
6	54	45	45	39	Cholera.
		36		32	Hemiplegia; weight of body 88 lbs.
5	62	45	54	39	Secondary abscesses in lungs and liver.
$5\frac{1}{2}$	60	39	48	33	Fever; weight of body 9 st. 7 lbs.
	57	39	51	33	Cholera.
5	45	39	42	36	Chronic peritonitis; meningitis.
$3\frac{1}{2}$	51	39	39	33	Phthisis; weight of body 6 st. 11 lbs.
6	51	42	45	36	Cholera; weight of body 8 st. 10 lbs.
5	57	42	54	36	Cholera.
4	54	39	45	33	Cholera; weight of body 6 st. 2 lbs.
$5\frac{1}{2}$	57	36	45	33	Erysipelas.
	45	39	45	30	Chronic bronchitis.
	54	42	45	36	Morbus renum.
7	54	42	45	42	Aortic valves much thickened and incompetent; mitral valves thickened, adherent and permanently open; pericardium universally adherent, sequence of rheumatism 4 years before.
3	24	18	21	16	Atelektasis pulmonum.
6	51	8	45		Malformation; contraction of pulmonic aperture, aorta arising from both ventricles; posterior wall of right ventricle 7 lines thick.
6	54	34	42	32	Obstruction at aortic orifice, with dilatation of aorta; valves with vegetations, some ath. of mitral and in aorta; 5 valves to Pulmonary artery.

ARTICLE IV.—*Report of some Cases of Maxillary Tumour occurring in Mr Syme's Practice.* By JOSEPH LISTER, M.B. London, F.R.C.S., Resident Surgeon to the Clinical Wards of the Royal Infirmary.

CASE 1.—*Tumour of the Upper Jaw; Excision of the Superior Maxillary Bone; Cure.*

William Darling, æt 55, admitted March 15, 1854. A farm labourer, born near the border, on the English side. Has usually enjoyed good general health. Twenty years ago some slight swelling appeared on the right cheek, and gradually increased for three years, when it appears that his medical attendant made an opening into the antrum from the mouth, by an incision above the alveolar border opposite to the bicuspid teeth; and fluid, sometimes thick and yellow, at others thin and watery, has been ever since discharged from the aperture. He suffered little or no pain in the part till last Christmas, when the upper gums of that side became swollen and painful; they subsequently ulcerated, and some of the teeth became loose, while the pain has ever since been on the increase.

At the present time (March 24th) there is an unnatural fulness of the right cheek in the vicinity of the nostril; and this part of the face, which is pretty firm to the touch, is tender on pressure and the seat of almost constant pain of burning, and occasionally of shooting character. In the mouth a swelling, continuous with that on the cheek, extends down to the teeth, of which the first bicuspid is absent and the second bicuspid, canine, and second incisor are loose. On the palate opposite to these teeth is a red irregular ulcerated surface, elevated above the general level of the palate, pretty firm in consistence, and extending from the teeth to a little beyond the middle line. Through the opening above alluded to, a probe may be passed up apparently into the antrum. The man has a robust aspect, and his general health is good.

Mr Syme regarded the case as one of tumour of the superior maxilla requiring removal, and operated accordingly on the 25th March. Chloroform having been administered, he made an incision through the cheek from the angle of the mouth to the prominence of the malar bone, in a curved line with the convexity directed downwards and backwards, and dissected up the flap of soft parts from the bone, detaching the ala of the nose from its connection with the margin of the bony nostril, the facial and other arterial branches being tied as they were divided. Having severed with saw and cutting pliers the junction of the superior maxillary and malar bones, he divided the bone with the forceps from this point to the nostril in a horizontal line below the margin of the orbit, with the intention of preserving, if possible, the floor of that cavity. Having previously extracted the central incisor tooth of the affected side, he divided through its socket the alveolar border and hard palate, one blade of the bone-forceps being placed in the nostril and the other in the mouth, and then wrenched out the bone from its remaining connections. A single deeply seated artery alone required ligature at this stage. On examining the upper part of the cavity, Mr Syme saw that the disease extended to the floor of the orbit, which he therefore at once removed. Several pieces of folded lint were then placed in the cavity, and the edges of the incision united with interrupted sutures, and a hare-lip pin at the angle of the mouth.

On examination of the tumour, I found it of pretty firm consistence, and of dark-red colour; its section not homogeneous, but presenting in the mass a coarsely granular appearance, very like that of some cancers of the face. Under the microscope the characters of malignancy were pretty strongly marked in the constituent cells.

No hemorrhage whatever occurred after the operation. The patient com-

plained much for a few days of pain in the head, and suffered a good deal of constitutional disturbance. The incision, however, united perfectly by first intention; the pin and stitches were taken out on the fourth day, and the lint was removed as it became loosened by the suppurative process. In a few days all constitutional disturbance subsided, and on the 10th April, fifteen days after the operation, he was able to walk down stairs and show himself to the clinical class, having previously been shaved.

April 17th.—At the present time there is scarcely the slightest difference in appearance between the two sides of the face.

He was discharged cured on the 28th April.

CASE II.—*Tumour of the Upper Jaw; Excision of the Superior Maxillary Bone; Cure.*

Angus M'Taggart, æt. 52, admitted May 22, 1854. A native of Argyleshire: employed as a railway labourer near Glasgow. Has generally enjoyed good health. A year and a half ago he observed a lump of pretty firm consistence forming on the right side of the palate, close to the alveolar border, about opposite to the second bicuspid and first molar teeth. About half a year later he noticed a little swelling of the right cheek, immediately external to the nostril, and at this time the right nostril became stuffed, and has continued so ever since. The swelling on the cheek, and also that on the palate, continued to increase, growing rapidly in May and June of last year, then more slowly for nearly a year, and again rapidly during the present month (May.) The upper part of the cheek became more and more involved in the tumour, and, three weeks ago, he found it begin to press upon the eye, which consequently felt weak, with some lacrymation. He never felt any pain in the tumour.

At the present time, May 31st, the right cheek presents a tumour of somewhat rounded form, filling up the hollow between the nose and the malar eminence, most prominent opposite to the bridge of the nose, and projecting further forwards than it. The inner part of the right lower eyelid is pushed up by the tumour, which extends upwards to the level of the inner canthus, where the nasal and ascending process of the superior maxillary bone are felt to rise and become expanded over it: about three quarters of an inch below the canthus these bones are so much thinned as to yield readily to the pressure of the finger. The consistence of the tumour, except where bones are expanded over it, is soft and semifluctuating. There is no undue fulness between the eyes, nor any difference between the distances of the right and the left inner canthus from the middle line, and pressure made below the inner part of the right eyebrow causes no pain. The tumour does not reach farther outwards than the vertical level of the outer canthus, so that the malar bone is quite free. Inferiorly, it extends on the cheek to a little below the level of the nostril, the lip not being engaged in it. The lower part of the nose is somewhat pushed over to the left side, and the right nostril is seen to be entirely occupied by a part of the tumour, of soft consistence, which, by its pressure, has caused the septum nasi to bulge to the left side, so as almost, though not completely, to obstruct the left nostril also. The integument over the tumour is very slightly redder than that of the other cheek, but is freely moveable upon the growth beneath. The tumour also projects, in considerable mass, into the mouth; the upper teeth are absent on the right side, backwards from the second bicuspid tooth inclusive, and the alveolar border is there tumid and soft: the tumour extends thence on the palate, in the form of a somewhat irregular rounded mass, which projects about three quarters of an inch from the natural level of the palate, and reaches about half an inch to the left of the middle line, where the palate-plate of the other maxillary bone is felt expanded over it. It reaches a very little beyond the limit of the hard palate posteriorly, so that the soft palate is almost entirely unaffected. The mucous membrane is entire over the whole of the mass. He has a very robust aspect, his general health is excellent, and his complexion natural.

It appeared clear, from the patient's statement, that the disease had commenced in the lower part of the bone, and grown upwards, and hence there was no reason to suspect that it had roots in the upper parts of the head ; and therefore, as there was as yet no unnatural fulness between the eyes, Mr Syme considered it probable that the whole morbid growth would admit of removal by operation.

On the 31st May, the patient being under chloroform, Mr Syme excised the superior maxillary bone, in a manner very similar to that described in Case I. He contented himself with the single incision from the angle of the mouth to the zygoma, and this gave room enough for the complete separation of the integuments from the tumour, even to its extreme upper limit, at the inner canthus, although the softness, as well as the extent of the morbid growth, made this dissection unusually difficult. The case did not admit of any attempt to save the lower margin of the orbit. The extent of the disease to the left side, in the palate, made it necessary to divide the alveolar border through the socket of the second incisor tooth of the sound side, and therefore to take away the lower part of the septum narium. The softness of the tumour caused some difficulty in tearing away the bone from its remaining connections, and during this part of the operation the investing capsule burst at one part in front, and discharged a large quantity of brain-like matter over the pillow. Two vessels only required ligature after the removal of the bone. Five or six small pieces of folded lint were placed beneath the flap, and the edges of the incision were united with interrupted suture, and a hare-lip pin at the angle of the mouth.

On examining the tumour I found that it was surrounded by an investing capsule, consisting for the most part of fibrous membrane, but containing in some places pieces of thin bone, and this even in parts where the thinned bony wall of the antrum was present as a distinct layer outside the capsule. Mr Syme considered that this appearance was probably due to the growth having commenced in the hard palate or alveolar processes, which had become expanded over its surface as it grew up into the antrum. The capsule was entire throughout except at one point on its anterior aspect, where, as above stated, it had burst during the operation : hence the whole disease had been completely eradicated. The part of the tumour that projected from the palate was moderately firm in consistence, and its section presented, like Darling's tumour, a coarsely granular aspect, produced by the fact that the growth consisted of a multitude of minute lobules, pretty readily separable, and each made up of well marked cancer cells (so called), *i. e.*, large cells with one or two large nuclei containing one or two nucleoli ; some of these cells developed into more or less perfect fibres. The rest of the tumour within the capsule was for the most part very soft ; it had not, however, the appearance of cerebriform matter, but was coarsely granular like the firmer part, and was evidently of precisely the same nature but in a softer state.

No hemorrhage occurred after the operation ; the patient was pretty quiet during the rest of the day ; he took a dose of morphia at night, and slept pretty well. Next day he was pretty easy, and the pulse tranquil, and he afterwards lay very quiet both night and day without repetition of the morphia. He took milk on the day after the operation, and had porridge on the following day. The edges of the incision united perfectly by first intention ; a red blush came on along the line of union about the second day, and at the same time the cheek became somewhat swollen and red : part of the lint was removed on the second day, and fresh lint substituted : on the third the stitches were taken out and other pieces of lint removed, and on the fourth day the hare-lip pin was taken away from the angle of the mouth. On the sixth day Mr Syme gave the patient permission to shave : the redness about the line of incision was at this time almost gone, and that on the cheek much diminished. He was discharged on the 11th July, a little swelling and redness of the upper part of the cheek being almost the only remaining deformity.

Remarks.—This case, on account of the softness as well as the

extent of the tumour, presented perhaps the extreme of difficulty in the operation. Considering that the part removed extended to the second incisor tooth of the sound side, and upwards to the inner canthus, while the softness of the mass made it necessary to avoid cutting towards it in the dissection of the flap from its surface, the satisfactory execution of the operation in the present case by the single incision, sufficiently proves the needlessness of the additional one along the ala of the nose and middle line of the lip, which Mr Syme himself practised in his earlier cases, but which he has for a number of years rejected as unnecessary.

It will perhaps be questioned how far it was right to interfere with so extensive a tumour in this situation, when its consistence made it probable that it was malignant. The reasons which led Mr Syme to expect that the disease would admit of entire removal by operation, have been mentioned above, and we have also seen that the examination of the part removed justified that expectation. So far, therefore, the prognosis is favourable; and as bearing further upon it in a very interesting manner, I may mention that about a year ago, an elderly woman from Galashiels called at the infirmary to show herself to Mr Syme, bearing upon her face the scars of the two incisions which he practised when he excised her superior maxillary bone in 1829, for a tumour as soft in consistence as M'Taggart's. I looked into her mouth and saw it to be quite sound, with the exception of the loss of half the hard palate.

It is impossible to tell whether the essential nature of that woman's tumour was the same as that of the one before us, whose structure, above described, differs from anything which I find mentioned either by Dr Bennett, in his work on Cancerous and Cancroid Growths, or by Mr Paget, in his Surgical Pathology. I believe it to be more nearly allied to the so-called "epithelial cancer" of the face than to anything else, although no scaly epithelium was to be found in it. It closely resembled a large cancer of the cheek removed by Mr Syme in September of last year, which, though an inch in thickness, and two and a half inches in diameter, had throughout the same coarsely granular aspect to the naked eye, produced by its being made up of a multitude of minute lobules, partially separable from one another, and composed for the most part of cells, bearing a close resemblance to those of some scirrhous and encephaloid tumours, which I examined about the same time, while scaly epithelium was absent. And my own observation leads me to think that a similar structure is by no means rare in growths on the cheek and lip, which would ordinarily be unhesitatingly pronounced to be examples of epithelial cancer. Taking into consideration the history of the present case, it seems probable that it was in the first instance a cancroid or semi-malignant epulis.

It is curious that in Darling's case the tumour was evidently of precisely similar nature; it is true it was firmer in consistence, but the palatal portion of M'Taggart's was pretty firm; again, the latter

was painless, while the former was the seat of burning pain; but tumours similar in structure, whether simple or malignant, often differ remarkably and unaccountably in this respect. There also seems at first sight a great difference in the duration of the two tumours, but it appears likely from the history that in Darling's case the morbid growth had only existed for a few months, and that the jaw had previously been affected with abscess or cystic tumour of the antrum, which is known to give rise occasionally to solid tumours.

Case III.—*Multilocular Cystic Tumour of the Lower Jaw; Excision; Cure.* Janet Livingstone, æt 34, admitted August 26th, 1854; born at Oban, now in service in Glasgow. Has generally enjoyed good health. About seven years ago a swelling began on the left side of the lower jaw, rather tender to the touch. A medical attendant supposed that it was connected with the teeth, and consequently extracted two of them, but without benefit. In two years it had attained to about the same size which it at present has; and she applied here to Mr Syme, who opened the tumour inside the mouth, and evacuated a large quantity of thick yellow fluid, and stuffed the cavity with lint. She remained about five months in the hospital, during two months of which time it continued to discharge and diminished much in size. She thinks it had closed completely when she left, but soon after its closure swelling again commenced, and in two years had attained about half its former size. She applied again to Mr Syme, who repeated the former process, giving exit to fluid of a similar nature to that discharged on the former occasion. After remaining about four weeks in the Infirmary she left, with the tumour much reduced in size, with orders to call back in six weeks. She did so, and Mr Syme, finding the tumour again on the increase, opened it a third time, and again stuffed it. After this the tumour, which had never entirely disappeared, remained stationary for about a year and ten months, but a year ago she again noticed a slight increase in it, and it has ever since continued to grow. She observed that the swelling did not recur in exactly the same situation, but that it encroached further forwards at every recurrence. She has occasionally had some uneasiness in the anterior part of the tumour, generally after exposure to cold; but with this exception, it has been painless.

At the present time (Sept. 11th), the left inferior maxillary region presents a great degree of swelling, extending outwards about an inch from the normal level of the cheek, and downwards three quarters of an inch below the natural position of the base of the jaw. On manipulating the part, the left side of the inferior maxilla is found thickened opposite to the incisor teeth, and a crackling sensation is felt when pressure is made there with the finger, showing that the outer table of the bone is expanded and thinned; from this point backwards the tumour expands rapidly, and about the seat of greatest prominence outwards, the outer table of the bone is evidently absent, and fluctuation is extremely distinct between this part and a place near the angle of the jaw, and also the interior of the mouth above the sockets of the teeth, of which all the malar and the second bicuspid are absent on this side: the same crackling sensation is also to be felt below the jaw, where the tumour projects furthest downwards. On looking into the mouth while the affected side is directed towards the window, the light is seen clearly immediately above the level of the teeth, showing that the tumour is translucent where it is thickest. The expansion of the bone appears to extend to within about half an inch of the articulation.

Mr Syme, on seeing the case, remarked that it was a curious fact that while cystic tumours of the upper jaw are generally readily cured by the removal of a portion of their wall, and stuffing with lint, yet this is by no means the casa with similar tumours of the lower jaw, which are very apt to return. Having

repeatedly given trial to this method of treatment in the present case, he thought it best to advise removal of the affected portion of the bone.

The operation was performed on the 13th September. The patient being under chloroform, Mr Syme made an incision from the angle of the mouth, downwards and outwards for a short distance, then parallel to and a little above the base of the jaw, and, lastly, upwards over the ramus of the bone to the articulation; he then dissected the soft parts upwards and downwards from the surface of the tumour, tying the facial artery when it was cut across. After pausing for the administration of more chloroform, he divided the jaw with small saw and cutting-pliers through the socket of the left outer incisor tooth, divided the mucous membrane of the mouth, and being unable on account of the softness of the tumour, to use it as a lever in disarticulation, he divided the neck of the bone and the coronoid process with bone-forceps before removing the tumour; an expedient suggested by Mr Cusack for cases of similar difficulty. He afterwards seized the two bony prominences with artery-forceps, and dissected them out. Only four or five vessels were tied in the operation; a small branch internal to the articulation gave some trouble from its position before it could be secured. A hare-lip pin was introduced at the angle of the mouth, and the edges of the rest of the incision were united with interrupted suture, two small pieces of folded lint having been previously placed in the wound.

On examination of the tumour, I found it to consist of four chief cysts; two of these were opened during the operation, viz., that which extended to the symphisis, and the highest, which reached to within half an inch of the top of the ramus of the jaw. The larger of the other two contained one ounce of yellowish viscid fluid, rendered opaque by a multitude of minute scales of cholesterine; the microscope showed the fluid to contain also a few blood discs and cells, some of them nucleated, and granular matter. The walls of the main cysts were studded with multitudes of smaller cysts, varying in size from a millet seed to a filbert, or larger. The parieties of the cysts were formed partly of thinned bone, partly of mere membrane; their internal surface was extremely irregular, being partially subdivided by very imperfect partitions, sometimes osseous, sometimes membranous, and delicate bands were in some parts seen crossing the cavities, seeming to have once formed the extreme margins of partitions which had become thinned away and partially absorbed. The walls of the small cysts were formed of interlacing fibres, and lined in part at least by a delicate scaly epithelium.

No hemorrhage occurred after the operation; the patient had a good deal of pain in the head on the afternoon of the 13th, but was relieved by a dose of morphia. She suffered little constitutional disturbance, and by the 16th the pain in the head was gone, and her chief annoyance was from thirst, and the unpleasant taste of the discharge in the mouth. The stitches and hare-lip pin were removed on the 17th, when union was found to have taken place throughout by first intention; except at the angle of the mouth, where the force necessary for the removal of the pin caused the lips of the wound to separate for about an inch. A strip of plaister was consequently applied, and the union became consolidated in a day or two. About ten days after the operation, considerable ædematous redness came on in the flap and just below the ear; in the flap this subsided in a few days under water-dressing, but below the ear it went on to the formation of a small abscess, which, on being opened, discharged some pulpy masses of sloughy cellular tissue. On the 10th October she asked permission to get up, which she was allowed to do, and she may now (Oct. 18th) be said to be well, a little weakness being her only complaint, and some dragging of the features to the right side almost the only deformity, and the latter will no doubt be removed when the portio-dura divided in the operation recovers its functions. There has been no tendency to displacement of the remaining half of the jaw, the balance of the muscles being sufficient to retain it *in situ*; she masticates soft solid food, and can strike the teeth together with a snap. She has long been able to articulate distinctly.

Remarks.—The structure of the tumour furnishes ample explanation of its repeated recurrence, and points to the cause of the difference between cystic tumours of the upper and lower jaw in their tendency to return after stuffing with lint. This treatment can of course lead only to the obliteration of the cavity stuffed, and, while very effectual in curing the disease of the upper jaw, which is, in most cases at least, merely an accumulation of fluid in a single cavity (the antrum) can have little power over proliferous multilocular cysts such as existed in this jaw. I find no notice of this form of disease in the inferior maxilla, either in the ordinary text-books on Surgery, or in Mr Paget's Surgical Pathology. A knowledge of it must prove of importance in practice, by making us hesitate less to recommend excision of the morbid part in cases where the ordinary treatment has had a fair trial.

The excision of a hard tumour of the lower jaw is generally a pretty easy task, the safety of the operator lying in cutting always towards the tumour. But the extreme thinness of the walls of these cysts made this impossible in the present case, which therefore presented very unusual difficulty in the operation.

The incision from the angle of the mouth is that which Mr Syme always practises for excision of tumours of the lower jaw, having found that the space afforded by the mouth permits the operation to be performed with much greater comfort, while the additional deformity which may be caused by the scar of the incision running into the mouth, instead of near the base of the jaw, is so slight in comparison with the advantage gained, as hardly to deserve consideration.

It fell to my lot to administer the chloroform in these cases. No choking or other unpleasant symptom was caused by its use, while the patients were thereby relieved from no ordinary degree of mental and bodily suffering.

Part Second.

REVIEWS.

1. *Lettsomian Lectures on Insanity.* By FORBES WINSLOW, M.D., D.C.L. London, 1854. Pp. 160.
2. *Practical Observations on Mental and Nervous Disorders.* By A. B. MADDOCK, M.D. London, 1854. Pp. 236.

THE first of the treatises before us comprises three Lectures delivered before the "Medical Society of London," by Dr Forbes Winslow, as Lettsomian Professor of Medicine for the year 1853. Dr Winslow is known to the profession as the author of "The Anatomy of Suicide," as the editor of the Psychological Journal of Medicine, as the proprietor and physician of a first-class private

asylum in the neighbourhood of London, and as a physician of note in this department of medical practice.

Dr Winslow has selected for his lectures three subjects of vast extent and practical importance; 1. The psychological vocation of the physician; 2. The medical treatment of insanity; and, 3. Medico-legal evidence in cases of insanity. Each of those subjects has already been made the special object of several monograph works, more or less voluminous; and it may therefore be readily imagined that the review of them taken by Dr Winslow in the scope of one lecture devoted to each, can be only either a popular and general one, or one calculated to develop and illustrate some peculiar views of the writer in regard to the more important and debateable points at issue under those several texts.

The first lecture partakes of the former character, and the second and third merit the latter. The object of the first lecture is to impress upon us the importance and dignity of psychological and metaphysical studies,—to show their importance in practical medicine, by illustrating the influence of the mind over the body; and to claim for the cultivators of medical science more exalted and onerous functions than those usually assigned to them;—to consider them in their "*spiritual character*, as having at their command and under their control a *medicina mentis*, as well as a *medicina corporis*—agents of great power and magnitude—which have not been sufficiently recognised or appreciated."

It would, indeed, be difficult to overrate the great importance of those studies to the practical physician, called upon as he is to give counsel and advice in almost every conceivable emergency that may distract the judgment or influence the passions and emotions of those entrusted to his care, or led to seek his advice. To those who intend to make the subject of mental alienation a special study and department of practice, the importance of a profound knowledge of mental philosophy is so obvious, that to us it appears almost a work of supererogation to illustrate it. Yet it is notorious that it is not considered a necessary part of the preliminary education of any medical man that he should have devoted any part of his time to the study of logic and mental philosophy; and that without any study of the phenomena of the healthy manifestations of mind (other than he may pick up in his intercourse with the world) and without any special study of mental derangement at all, he is legally qualified when he receives his degree or diploma, and daily called upon it may be, to exercise the most onerous and responsible duties that can devolve upon one human being in reference to another—those of depriving a fellow-creature of his liberty, of determining his incapacity to manage his affairs, or dispose of his property, and of his responsibility or irresponsibility for acts affecting his life. Our licensing boards render attendance upon lectures on mechanical philosophy and natural history, and a knowledge of Latin and other branches, imperative upon their graduates—the profoundest knowledge of

which *might* in a half century's practice of medicine, never be of essential detriment to a medical practitioner, or his patient; and yet require no preliminary education or special study to prepare their graduates for performing the most responsible duties that can possibly devolve upon them. We do not underrate the importance of mechanical philosophy or any other branch of education, at present embraced in the curriculum of medical education; on the contrary we believe them to be all desirable; but we maintain that some additions might be made to the curriculum of the *preparatory* studies of those devoting themselves to medicine, and that an examination bearing on the subjects of logic and mental philosophy might with propriety be added to the present matriculation examination on Latin only. Add to this a special course of instruction on mental derangement, such as has been already very properly rendered imperative by the Honourable East India Company's Examining Board, and we believe a great boon would be conferred on the medical profession and on the public. We trust that such lectures as the one now before us may have the effect of leading our colleges and other licensing boards to take this subject under their serious consideration.

Under the head of the medical treatment of insanity, Dr Winslow insists very strongly on the curability of the disease, and upon its being a disease of the material organ of the mind. He states it to be the result of his experience, "that if brought within the sphere of medical treatment in the earlier stages, or even within a few months of the attack, insanity, unless the result of severe physical injury to the head, or connected with a peculiar conformation of chest and cranium, and an hereditary diathesis, *is as easily curable as any other form of bodily disease for the treatment of which we apply the resources of our art.*" This is certainly very cheering, but it contrasts very strongly with an opinion we have heard expressed by one not less eminent in this department of medicine than Dr Winslow,—that he doubted whether any attack of insanity was ever completely recovered from, and whether, even in the cases apparently most rapidly and completely cured, there did not remain some permanent impairment of the mental powers, more or less affecting the intellectual or moral character and activity of the individual in after life. We would gladly adopt the more favourable view of the question, and although we do not for a moment doubt that under early and proper treatment, a very large proportion of recoveries take place, we suspect that it is in a comparatively small number that the cure is as complete, or as little likely to be followed by a relapse, as in many of the bodily diseases we could name.

Dr Winslow's views of the pathology of insanity are brief and comprehensive. "I believe insanity," he says, "to be the result of *a specific morbid action of the hemispherical ganglia, ranging from irritation, passive and active congestion, up to positive and unmistakeable inflammatory action.* This state of the brain may be confined

to one or two of the six layers composing the hemispherical ganglia; but all the layers are generally more or less implicated in conjunction with the tubular fibres passing from the hemispheres through the vesicular neurine." Dr Winslow guards himself against the objection to which his view of the pathology of insanity is exposed by the fact, that according to the statements of the most careful observers there are no positive *physical* proofs of inflammatory action in the *substance of the brain*—none of the *products* of inflammation to be met with in the great majority of cases,—by using the term "*specific*" as distinguished from "*ordinary inflammation*." He guards his readers against treating cases of insanity on the ordinary therapeutical rules in cases of inflammation, by depletion and antiphlogistic measures, and begs them to remember that it is neither *ordinary* inflammation, nor "mere loss of nervous tone," but *specific* inflammation or morbid action.

With all respect for Dr Winslow's distinguished abilities as a writer, we must confess that this does appear to us as a darkening of counsel with words without knowledge. We admit that in a very large proportion of post-mortem examinations of those dying insane, morbid changes, indicative of inflammatory action, are found in the *membranes* of the brain, but we are yet without proof of any *constant* physical changes in the *substance* either of the grey or white matter of the brain, to enable us to affirm that there is evidence of a morbid change in its structure sufficient to justify the theory of "a specific morbid action" there. The words *irritation, passive and active congestion*, are certainly very indefinite; they remind us of Pharaoh's seven lean kine that swallowed up all the fat ones, and seem adapted to comprise every theory of insanity that would ascribe it to a morbid change in the nervous matter which is the immediate organ of thought; but until that morbid change is actually made out by the discovery of its anatomical characters, we think it premature to announce such a pathological creed regarding insanity. It has appeared to us that in many cases of protracted insanity no change in the structure of the cerebral substance itself could be detected, either by the eye or the microscope, to distinguish it from the nervous substance of persons dying in the possession of all their faculties. Dr Winslow takes no account of the theories which ascribe insanity to disease of the *blood*, and to that which refers it to "loss of nervous tone"—he objects that this loss is due to a "*prior morbid condition of the encephalon*," the sequel of specific inflammation of the hemispherical ganglia; but of this specific inflammation we contend there is no sufficient evidence, nor, indeed, any at all, in a great majority of cases. We cannot divest our minds of the impression that the analogy between insanity and the effects of certain poisons entering the blood—the suddenness of the invasion of the disease in many cases, and the suddenness of its remission in others, sometimes of long duration, tend to show that it may be a disease of the blood, or may depend upon a condition of the nervous organism which immediately

precedes thought or feeling, and which is not and possibly never can be cognisable to our senses in a post-mortem examination.

Dr Winslow's remarks on the treatment of insanity are judicious, valuable, and such as might be expected from one of his experience. We regret that they are necessarily so far condensed in the limits of this one lecture as to form rather hints and general rules, than to be available for the guidance of practitioners in the treatment of the specific varieties and numerous forms of mental derangement.

The largest and most valuable of the lectures in our opinion is the third, which treats of the medico-legal relations of insanity. It contains a number of valuable hints and advices for the guidance of medical men in giving evidence in courts of law in regard to cases of insanity. It comprises also a very lucid explanation of the law of England in regard to insanity, both as a disqualification for legal acts and as an exculpation from crime.

There is only one point in this lecture on which we are inclined to differ somewhat from Dr Winslow, and that relates to *moral insanity*. He contends that in all the so-called cases of moral insanity there is a disorder also of the reasoning, intellectual, reflective, or comparing powers; and he asks whether there is not also in such cases a derangement of ideas, and certain *latent delusions*. While we are ready to admit that, in moral insanity, there are important and essential derangements affecting the will, and the control over the thoughts, volitions, and acts of the individual, and influencing him therefore as a reflecting, reasoning, and responsible agent, we protest against the assertion that, in such cases, *actual delusions* must exist. This doctrine we conceive to be contrary to the most carefully collated facts regarding such cases, and most dangerous to the best interests of afflicted humanity, however opposed it may be to the *legal definitions* of *insanity;* and we do not think it would be serving the interests of truth, out of deference to the opinions of the twelve judges, or any legal distinctions, or precedents, to maintain that the loss of self-control, the *inability* to control the thoughts, feelings, or actions, is not the true and proper test of insanity rather than the existence of *delusion*, or the *knowledge* of right and wrong.

The precautions which Dr Winslow gives as to offering definitions of insanity in a court of law, and the tests which he lays down for the determination of the *fact* of insanity, are eminently wise and practical. He advises us to compare the man with his former self—not with other men or the world at large—and to ascertain whether his conduct, and ideas, and delusions, are so incompatible with his known natural character as to be referrible only to *disease*. The same rule appears to us to be applicable to the so-called cases of moral insanity;—if the excited emotions or passions, or perverted feelings, desires, and impulses are obviously such as are altogether contrary to the known character and natural disposition of the individual, they must be regarded as morbid, and as the evidence, therefore, of insanity;—and the definition which Dr Winslow applies to

insanity with *delusion* is equally applicable to insanity without delusion, the former being *delusions* which are unmistakeably the product of a *diseased* intellect, and the latter *impulses* and *desires* which are morbid, and the product of *diseased* feelings, affections, or passions.

Of Dr Maddock's work on Affections of the Nervous System, we would fain speak favourably, as one containing many judicious and practical observations on the treatment of insanity and allied nervous affections, the result, doubtless, of considerable experience. We cannot, however, avoid taking exception to the character of the work, and of all *quasi* scientific books of this description, which are neither confessedly popular, or confessedly professional. We charitably hope we may be wrong, but we cannot help a feeling, that a book which commences with a popular description of temperaments, poetically illustrated, followed by a description of the nervous system, in which we are considerately made acquainted with the fact, that the medulla oblongata is a small, but important appendage, "lying directly at the bottom or basis of the entire mass of the brain material," is intended rather for the general public than the profession; and that a work which consists in a great measure of "cases" considered "hopeless" and "incurable" at Bethlem and elsewhere, and which are speedily cured by the author, under protestation that this case will be remembered by Dr H. of ——, and the other is well known to Mr —— of ——, savours very strongly of the advertisements of Morrison's Pills and Holloway's Ointment, which figure in every provincial newspaper. We regret that a writer who can adorn his work with so many quotations from Horace, Shakespeare, and Milton, and so many references to medical men of high standing in the department of his profession, should appear to prostitute his talents and experience in the production of a treatise which looks to us very like a large advertisement for consulting practice.

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1. *On some Diseases of Women admitting of Surgical Treatment.* By I. B. Brown, F.R.C.S., etc. London: 1854. 8vo. Pp. 288.

2. *On Uterine Polypus: its Nature, Early Detection, and Treatment.* By R. Barnes, M.D. London: 1854. Small 8vo. Pp. 44.

In the volume before us we have an exposition of the surgical treatment applicable to many of the most common and serious affections of the female generative organs. It is by one of the surgeons of St Mary's Hospital, a lecturer on obstetrics in a young educational establishment connected with that hospital, and a gentleman well known to the profession as having taken an especial and active interest in this department of the medical art. The subjects discussed are among the most difficult in the whole field of surgery, rupture of the perineum, prolapsus of the uterus, vesico-vaginal and recto-vaginal fistula, polypus, stone, imperforate hymen, ovarian

dropsy, etc., etc. From the work is excluded all the minor surgery of diseases of the cervix uteri and vagina, subjects which the profession will willingly allow to rest for cool consideration after having been so recently and extensively discussed, not to say perplexed.

The most important part of the work is that devoted to the consideration of the restoration of the perineum, the recto-vaginal and vesico-vaginal septa. It is well known that the vesico-vaginal deficiency is by far the most important, and for two reasons; namely, first, that it causes more suffering and distress than the others, suffering equal to that produced by any other affection; and, secondly, that it is one of the most difficult to cure, the difficulty in practice often amounting almost, if not altogether, to an impossibility. This piece of plastic surgery offers full scope for ingenuity in device and dexterity in achievement to the best and ablest surgeons, and probably for a long time to come will offer. It would be vain, in our present position, to speak decidedly on the merits of the various proposals for closing up a vesico-vaginal fistula. The last author in the field has always a new plan, great confidence and moderate success. Before Mr Brown a very able and distinguished Parisian surgeon, M. Jobert, published a treatise on these fistulæ. His plan of treatment has been described briefly in former numbers of this Journal, and we have had the pleasure of seeing cases of his where they proved wonderfully successful. Every workman likes his own tools and ways. Mr Brown, of course, recommends his own, and we find he adopts the ingenious so-called clamp suture of Dr Marion Sims of Alabama, a gentleman, the supposed neglect of whom has very nearly turned the head of some contributor to our monthly contemporary in America.

For book-making purposes surgeons have displayed wonderful ingenuity in *describing* new operations—and nowhere is this peculiarity more prominent than in regard to this affection. It requires a very critical eye to discover the differences among the "new operations." They all come to this—you thoroughly pare the edges—you bring them together and keep them there by various sutures the best way you can—you take off all strain on these sutures as far as possible—and you keep all as quiet as possible not to disturb the parts and so stop the process of union. It is not to be supposed, however, that the small differences alluded to are always unimportant though they may often be so. Mr Brown's plans have the merit of simplicity, and well deserve the consideration of any one who purposes operating. He has had an average amount of success. The first case he records, failed; in the second, after treatment, no fistulous opening could be found; the third died about sixteen days after the operation: the fourth failed.

"These cases," says Mr Brown, "illustrate the various points of difficulty which are met with in the treatment of this distressing lesion; and although they do not exhibit a great amount of success, they may fairly be looked upon as valuable illustrations of our present knowledge and practice; and I still

look forward to a greater amount of success, by steady observation and persevering efforts, which the late improvements in surgical science certainly justify; especially as the difficulties are rather mechanical than pathological. It cannot be concealed, however, that it requires no ordinary amount of perseverance and determination, to bear up under the vexatious disappointments which are constantly occurring in the hands of the most painstaking operators."—P. 111.

Mr Brown enters at length into the subject of lacerated perineum. We cannot now follow him through this elaborate essay. He recommends the division of the sphincter ani on both sides to remove strain upon the sutures inserted; he approves of operating immediately after the injury; and advises the copious use of opiates to constipate the bowels as an important part of the after treatment. In other respects there is little novelty. Much is made of the mode of incising and preparing the surfaces to be united, but it all comes in the end to the simple making a *raw* and uniting the denuded parts. We cannot approve of the drawing (plate 1) of the proposed sections. It bears more resemblance to a field work or fortification than to any incisions ever made in restoring the integrity of the perineum.

The chapter in Mr Brown's work on polypus is meagre and incomplete. But the profession has in its place a little book on this subject from the pen of Dr Barnes, which contains an excellent account of this disease, and ably discusses the novel points in its pathology and treatment.

One of the best and soundest parts of Mr Brown's volume is the twelfth chapter, on diseases of the rectum resulting from certain conditions of the uterus.

"Displacement forwards or backwards, and enlargement of the uterus, from whatever cause,—whether pregnancy, hypertrophy, inflammatory engorgement, distension by fluid or by hydatids, polypi, or scirrhus, or any other disease—alike tend to injuriously affect the rectum.

"As displacement may occur without enlargement of the uterus, it may operate singly in inducing rectal disease; but more often the two conditions concur, and it is then chiefly that the mischief is so considerable. The evils, too, will be greater when, with retroversion, engorgement of the body of the uterus, and with anteversion, that of its neck, go together. On the other hand, enlargement, without deviation of the womb forwards or backwards, may, and oftener does, act singly in provoking disease of the rectum, than either of these displacements does without it.

"The conditions of the uterus under consideration act on the rectum injuriously in two ways: first, by mechanical pressure; and, second, by inducing vascular disturbance like that present in themselves. An enlarged uterus drags on its lateral ligaments, elongates them, subsides lower down in the pelvis, and so comes to press on the lower bowel, to interfere with its muscular action and the circulation through its bloodvessels, and to irritate its mucous lining. At the same time any hyperæmic state of the uterine vessels causes an increased fulness of the hæmorrhoidal, and a determination of blood to them. Thus, by reflecting on the anatomy of the parts, it will easily be understood why and how diseases of the rectum, such as hæmorrhoids, prolapsus, fissure, stricture, fistula, as well as disordered functions of the bowel, as constipation, dysenteric irritation, etc., do sometimes result directly, either from the mechanical pressure of an enlarged uterus, or simply from the derangement of the hæmorrhoidal circulation, resulting from uterine disease."—P. 148.

These views of our author find their natural counterpart in the marked success attendant on their being carried out in practice. None of the cases in this work are more interesting and instructive than those in this chapter. We shall quote one as an illustration of the rational and successful treatment of uterine displacement.

"CASE XLI.—L. C., aged 31, unmarried; had suffered for several years from three or four large piles, which, she stated, protruded in the act of walking, and also created great uneasiness in the sitting posture. Every three or four weeks she lost a considerable quantity of blood from them. Her bowels were seldom opened more than once in three or four days, and then only from taking some aperient. After each defæcation, she had always increased inconvenience and pain. She suffered much from indigestion, headaches, and general lassitude. I first inquired as to the menstrual function, and found that, although she was generally regular every four weeks, the quantity was very small, and the discharge seldom continued more than one day, and that, at this time, she generally lost a considerable quantity of blood from the hæmorrhoids. On making a digital examination per vaginam, I found the uterus enlarged and painful to touch, and lower in the vagina than normal. I directed that three leeches should be applied to the os uteri, and that the bleeding should be encouraged by a hip-bath immediately on their removal; that the bowels should be freely opened by a saline aperient; and that this treatment should be pursued once a week. She was further directed to take twice a day the sixteenth part of a grain of the bichloride of mercury, and her diet was ordered to be simple, nutritious, and unstimulating. The result of these measures was, that at the next menstrual epoch there was a more copious uterine discharge, and an absence of bleeding from the hæmorrhoidal vessels. The same treatment was continued for another month, and resulted in an improvement in menstruation, both in quantity, and in the duration of its flow; the uterus had much decreased, being nearly of its normal size, and not painful to touch. I now considered it advisable to remove the hæmorrhoids, which I did by passing a ligature of twine round the base of each. I then placed the patient in bed, giving her a grain of opium every four or six hours, for the purpose not only of affording relief from pain, but also of constipating the bowels till the ligatures should come away, which they did in five or six days. The case progressed satisfactorily and the patient returned to the country perfectly recovered.

"This case presents some striking practical facts, which will serve to illustrate my preliminary remarks.

"1. The bleeding from the hæmorrhoids at the menstrual epoch. It is a fact that hæmorrhoids are always more troublesome at the time of menstruation, because more blood is circulating through the bloodvessels of the rectum as well as of the uterus; and, if there be any obstruction in the uterine vessels to prevent the menstrual flux, then the hæmorrhoids often bleed freely.

"2. The enlarged and inflamed uterus, with the absence of the menstrual flux.

"3. The great relief afforded by the antiphlogistic treatment.

"4. In ligaturing the hæmorrhoids I prefer *twine*. Its advantages over silk are very marked:—first, in procuring a quicker separation of the tumour; and, secondly, in causing less pain to the patient. It will also be observed, that I preferred the ligature to the scissors in the removal of these tumours, acting upon the golden rule laid down by my old and esteemed friend, Mr Copeland, in all operations upon the rectum, '*to cut skin, and tie mucous membrane.*' I am also indebted to the same gentleman for the practical hint to prefer the use of the vegetable product—twine, to that of the animal product—silk.

"I think it is not too much to assert, that had I at first devoted my attention to the hæmorrhoids in this case, I should not have succeeded in restoring my patient to the condition of health."—Pp. 150, 151.

The chapter on ovarian dropsy is very well worthy of perusal,

although the great difficulty of the subject is avoided, namely, the question whether ovariotomy should or should not be performed. The discussion of this very important point would lead us beyond the limits assigned to the present notice.

We can recommend Mr Brown's volume to the profession as the best treatise in our language on the diseases of females admitting of surgical treatment.

Principles of Comparative Physiology. By WILLIAM B. CARPENTER, M.D., F.R.S., etc. Fourth Edition. Lond. 8vo. 1854. Pp. 770.

THE third edition of Dr Carpenter's Principles of Physiology, General and Comparative, is exhausted,—a circumstance which excites no surprise in our mind, as we have always considered it to be the best work he has produced. That book was exactly three inches thick; and in preparing a fourth edition it was found that there was still more to add, which would have swollen its already bulky proportions into literally an insupportable size. But instead of dividing the entire treatise into two volumes, as suggested to him by many of his friends, the author has preferred to divide its subjects, and to treat of them separately, though connectedly. The present volume, therefore, consists of the "Comparative Physiology" of the last edition, extended from 530 pages to 744, and with 300 illustrations instead of 130.

On the merits of the work we need not dwell, as every anatomist, zoologist, and physiologist will readily admit that it is the best exposition of the present state of comparative physiology in the English language.

Histoire Naturelle des Végétaux Parasites qui croissent sur l'Homme et sur les Animaux Vivants. Par CHARLES ROBIN, etc.

Natural History of the Parasitic Vegetations which grow on Man and Living Animals. By CHARLES ROBIN, M.D., Professor of General Anatomy, etc. etc. With an Atlas of Fifteen Plates, partly coloured. Paris. 8vo. 1853. Pp. 702.

PRESS of matter has obliged us to delay our analysis of this important work so long, that we fear another edition will appear before we shall be in a condition to do so. We therefore now make our acknowledgments to M. Robin for his valuable book. It was a long time before our elder pathologists could be made to believe that scald head, the diptherite of infants, and other, to them, well known diseases, were in truth owing to the growth of fungi on the living tissues. But so it is; and the idea once admitted, the reader has only to look into M. Robin's volume, and he will be surprised at the enormous development which it has already undergone. At present this work is justly considered as the standard authority on the subject, containing as it does the most accurate description and figures of every known parasitic plant growing on living animals.

MEDICAL SCHOOLS

WINTER

	Anatomy, Systematic and Practical with Demonstrations.*	Institutes of Medicine, or Physiology †	Chemistry, and Practical Chemistry	Materia Medica and Therapeutics.	Practice of Physic.
University of Edinburgh.	Professor Goodsir.	Dr Bennett.	Dr Gregory.	Dr Christison.	Dr Alison.
Ex. Academical Lecturers, Edinburgh.	Dr Struthers.		Dr Wilson.	Dr Maclagan.	Dr Wood.‡ Dr Gairdner.‡
University of Glasgow.	Dr A. Thomson.	Dr A. Buchanan.	Dr T. Anderson.	Dr Couper.	Dr M'Farlane.
Anderson's Inst. Glasgow.	Dr M. Buchanan.	Dr E. Watson.	Dr Penny.	Dr Easton.	Dr A. Anderson.
Marischal Coll., Aberdeen.	Prof. Lizars.	Dr Ogilvie.	Dr Clark.	Dr Henderson.	Dr Macrobin.
King's College, Aberdeen.	Dr Redfern.	Dr Christie.	Dr Fyfe.	Dr Templeton.	Dr Williamson.
University of St Andrews.		Dr Day.	Professor Connell.		

SUMMER

	Practical Anatomy and Demonstrations.	Comparative Anatomy.	Histology.	Practical and Analytical Chemistry.	Midwifery.
University of Edinburgh.	Professor Goodsir.	Professor Goodsir.	Dr Bennett.	Dr Gregory.	
Ex. Academical Lecturers, Edinburgh.	Dr Struthers.			Dr Wilson.	Dr Keiller.‡ Dr Duncan.‡
University of Glasgow.	Dr A. Thomson.		Dr A. Thomson.	Dr T. Anderson.	
Anderson's U., Glasgow.	Dr M. Buchanan.			Dr Penny.	Dr Paterson.
Marischal Coll., Aberdeen.	Professor Lizars.		Dr Beveridge.	Dr Clark.	
King's College, Aberdeen.	Dr Redfern.		Dr Redfern.	Dr Fyfe.	

* Demonstrators are advertised in conjunction with the Anatomical Teachers, but in Scotrised to sign certificates in conjunction with the Lecturer on Anatomy to qualify for attend.

† This course is equivalent to that given under the name of general anatomy and physiology demonstrations, and one course of the institutes in Scotland, as equivalant to attendance on schedules are issued by the London Boards for Scotch students in consequence, which should

‡ These are not conjoint courses, but separate ones by the gentlemen named.

THE OBJECT AND ORDER OF MEDICAL STUDIES.

In the above table we have given medical students a correct list of the various teachers in the schools of Edinburgh, Glasgow, Aberdeen, and St Andrews. In now addressing to them a few words of advice, there are many subjects to which we might direct their attention, such as the necessity of those habits of diligence and regularity which are so necessary for the perfect attainment of any art or science; the importance of the profession of medicine in its

OF SCOTLAND, 1854–5.

SESSION.

Surgery.	Midwifery.	Natural Philosophy.	Natural History.	General Pathology.	Clinical Medicine.	Clinical Surgery.
Prof. Miller.	Dr Simpson.	Professor Forbes.	Professor E. Forbes.	Dr Henderson.	Drs Alison, Christison, & Bennett.‖	Professor Syme.
Mr Spence.		Dr Lees.			Dr Robertson.	Dr Dunsmure.
Dr Lawrie.	Dr Pagan.	Prof. W. Thomson.	Dr Wm. Couper.		Dr Easton.‡	Dr Hunter.‡
Dr Hunter.	Dr Paterson.	Dr Taylor.			Dr Bell.‡	Dr Fleming.‡
Prof. Pirrie.	Dr Dyce.	Professor Gray.	Professor Nicol.		Dr Kilgour.	Dr Keith.
Dr Kerr.	Dr Rainy.	Professor Thomson.				
		Professor Fischer.	Dr M'Donald.			

SESSION.

Botany.	Medical Jurisprudence.	Natural Philosophy.	Natural History.	Clinical Medicine.	Clinical Surgery.
Dr Balfour.	Dr Traill.		Professor E. Forbes.	Drs Alison and Bennett.‖	Prof. Syme.
	Dr Haldane.	Dr Lees.		Dr Robertson.	Dr Dunsmure.
Dr W. Arnott.	Dr Rainy.			Dr Easton.‡	Dr Hunter.‡
Dr Bell.	Dr Crawford.			Dr Bell.‡	Dr Fleming.‡
Prof. Nicol.	Dr Ogston.			Dr Kilgour.	Dr Keith.
Rev. T. C. Brown.	Dr Reid.		Rev. Mr Longmuir.		

land their functions are only to assist in the practical anatomy classes, and they are not authoance on anatomy and physiology.
in the English schools. The London Boards, however, consider two courses of anatomy with three courses each of anatomy and of general anatomy and physiology, in England. Special always be inquired for.

‖ This is a joint course.

practical, social, or political bearings; the advantage of keeping alive that enthusiasm, and those high aspirations which constitute in youth the chief stimulus to exertion; the consideration that our days of studentship constitute the most important epoch in our lives; and the caution required by the conviction that, for the most part, it is then our future character as men of honour, truthfulness, and probity is formed. Hoping that all these topics will receive mature consideration, we shall content ourselves with pointing out what we believe to be the true object and order of medical studies.

We are induced to do so in consequence of having observed too many instances where an ill regulated education, a confusion in the plan of proceeding, and an unacquaintance with what is essential and what is non-essential, has tended to retard and even to ruin the career of several young men who were otherwise distinguished for considerable aptitude and ability.

In the outset, then, we would especially endeavour to impress upon those we now address, one important fact, namely, that they are MEDICAL students, and that as such their ultimate object is to acquire an art, or, in other words, a knowledge of all those means which are directed to the prolongation of life and cure of diseases. It is not for them to become natural or moral philosophers, chemists, histologists, zoologists, or botanists, although these subjects, if wisely studied, will, by inducing habits of exact observation and sound reasoning, be of the most essential service to them as medical practitioners. Neither is it for them to attempt the acquirement of all the knowledge that the different professors they attend, teach in their particular departments. But what is expected of them, and what they must endeavour to obtain from the whole series of studies, is such a sum of learning and such an available kind of information, that they may undertake the serious duties of a medical practitioner with credit to themselves and advantage to the public. This is the great object which should never be lost sight of, from the beginning to the end of their medical studies.

No general observations are applicable to every individual, and he who proposes spending a portion of his student life in the Scottish schools, will find no difficulty in following any of the classes he may find necessary. In Scotland, a young man generally commences his medical education at the age of 17 or 18 years, and spends four years in the acquirement of professional knowledge. It is to him, and in reference to the proper employment of these four years—a time short enough for the object in view—that the following remarks are more particularly addressed.

The first duty which every medical student should perform, is to draw out a well considered plan of the order in which he is to study the various subjects contained in the prescribed curriculum—the hours necessary for attendance on classes and the time at his disposal for other pursuits. This general plan, if a good one, ought to be carefully followed, although it may receive a few modifications, additions, or subtractions as time advances, with the view of meeting particular wants or requirements in individual cases. What is especially necessary to be understood is that a thorough knowledge of some things is essential, whilst only a general knowledge of others is expected. The first essential subject to be acquired is Anatomy. This truth, which is sure to force itself upon the student sooner or later, is unfortunately one which often fails to impress itself upon him at that time when most likely to be useful. Anatomy, indeed, is, as it were, the alphabet of his subject; it is the *sine qua non*, without

which all other kinds of study are perfectly useless. Medical, like most other forms of education, consists of a series of steps, each of which must be mounted before it is possible to reach the other. There can be no leaping over in this case. Hence it is necessary to be convinced that the more earnestly Anatomy is cultivated during the first year by means of lectures, demonstrations, reading, but above all by dissections, the more thoroughly will the student be prepared for, and the more easy will become his subsequent studies.

Another important subject is chemistry, which teaches a knowledge of the elementary composition of bodies, and of the method in which combination takes place between them. Indeed as Medicine progresses, the more it becomes evident that Chemistry, particularly organic chemistry, must play a chief part in causing its advancement. Those who may feel ambitious of assisting in this great work, should pay marked attention to a science, on the development of which physiology and scientific medicine are greatly dependent.

Both Anatomy and Chemistry should be studied during the first winter session, and it will be well for the student to attend with them a course of Natural Philosophy. Of the three subjects, Anatomy should be considered as the chief one, and every thing else should give way to it. There will be ample time, however, for the study of chemistry, while by attentively following the lectures on Natural Philosophy, with only occasional reference to books, a sufficiency of that subject may be acquired.

The summer should be principally devoted to the furtherance of Anatomical knowledge by means of dissections, and by studious attention to the demonstrations and the course of comparative anatomy. Practical Chemistry should be followed in the laboratory. Either Natural History or Botany should also be attended, care being taken that neither of these agreeable studies interferes with that all-important subject, Anatomy, which, during these three months, should be strenuously and energetically pursued, in order that preparation be made for the next great step in medical education.

There seems to be a principle in the human mind which seeks for a rational causation, or an explanation of the phenomena it observes; and hitherto we have supposed the student to have been busied with anatomical and chemical facts. He has next to ascertain the theory or generalisations which are derivable from them. This constitutes the course of the Institutes, which comprises a systematic consideration of histology, physiology, and some parts of pathology. We need not dwell upon the necessity of theory, for the mind, and especially the young one, is only too prone to rush into it. It is not so much theory itself, as *correct* theory—that is, one which embraces all known facts—that is so much to be desired. Indeed nothing is more necessary than early to acquire the habit of checking rash speculation, and that proneness to crude and imperfect hypothesis, which alike characterises the infancy of knowledge in the

early history of the world, as well as in the early history of each individual. One of the most certain modes of accomplishing this is rigidly to separate fact from theory, to do which accurately in the ever advancing state of knowledge is much more difficult than the tyro may at first sight suppose. A fact may be defined, as that which is obvious to the well cultivated senses of the observer. We say well cultivated, because the senses themselves require to be educated before they can receive proper impressions. Here indeed lies the difficulty, for what is obvious to the sight of an experienced observer is overlooked by the student; the sound which is heard by the one is inaudible to the other; what the first feels distinctly is not tangible to the second. The acquirement of facts therefore must be commensurate with a proper cultivation of the senses, whilst a correct theory will depend upon combining this with just reasoning power and strength of mind. Both of these essential parts of education require constant practice, and hence it is that personal investigation (as in dissection) is necessary for the one, whilst severe thought, revolving mentally reasons for and against, and, above all, discussion with others are so essential for the other. Those who take every thing upon trust—who do not look, hear, or feel for themselves can never be good observers, and their range of facts is limited—while such as do not think or reason, however acute they may be in observation, can never make any real progress in scientific study.

With regard to improving the mental powers and that critical acumen so necessary for separating truth from falsehood, there is one method which we have no hesitation in strongly recommending to the student. We allude to the benefits to be derived from constant attendance on a well conducted debating society. Of all such institutions the Royal Medical Society of Edinburgh may be considered as a model of excellence, and it is one of which the students of that University have justly been proud, for the long period of 120 years. Similar societies, we believe, exist in Glasgow and Aberdeen. These institutions conjoin the advantages of an excellent library and reading room; an intercourse with the most industrious and intelligent students; an introduction to the mode of conducting public meetings and discussions; the means of obtaining fluency of speech and perspicuity of language; an opportunity of concentrating one's knowledge on those points which are the subject of discussion; of bringing forward in the form of a communication the results of individual reading, observation, or research, and above all, they animate to that legitimate and honourable endeavour to obtain distinction, which is itself a strong proof of existing excellence, and the best stimulus to future exertion.

But to return to our plan of education. Whilst during the second year every effort should be made to arrive at powers of generalisation, the student must hold fast and extend his knowledge of facts, and hence during this period he again attends the class of anatomy, and continues his dissections. Now also his

knowledge of facts will commence to take a new direction, as he must attend lectures on Materia Medica and Therapeutics, whereby a knowledge of the action of drugs, and other remedies, is to be acquired. He must further attend systematic surgery, and so obtain a general notion of external diseases. In doing so he will observe how necessarily operations on the body are connected with a knowledge of anatomy, and this study having been hitherto general ought now to be directed by a surgical aim. Thus every thing depends upon the manner the first year has been spent, for if anatomy be wanting, surgical details cannot be understood, and the result will be that instead of the attention being concentrated chiefly on the study of physiology, and obtaining just views of function, the mind will be perplexed, an attempt will be made now to learn the facts that ought to have been already mastered, the ideas of theory will be vague and confused, and at the end of the session, instead of being ready to take advantage of the summer three months, it will be necessary to pause, or even to retrograde, in the prescribed course.

During the second summer Botany or Natural History should be attended; and, in doing so, the student should resolve on making himself master, at this time, of as much of either of these sciences as he can.[1] Here, again, if he has made himself tolerably proficient with the general laws of physiology, in the course of the previous session, he will readily perceive how either subject bears upon his general course of study. Whilst attending botany or natural history he should assiduously extract from the lectures, all the facts and sound theory he can, and add them to his store of knowledge, placing them, with natural philosophy, amongst the useful but non-essential parts of study. During this summer, also, he should commence, if he have not previously done so, attendance on the hospital, enter to a course of practical pharmacy, once more pursue practical or analytical chemistry in the laboratory; and, if he can, occupy some hours in dissection, of which it is impossible to have too much, as thereby he will keep up his knowledge of anatomy.

We now arrive at the third year in Medical Education, and at what may be called the third essential step in the student's onward progress. It consists in directing his chief attention to hospital practice. Many Scotch students, indeed, first enter an hospital at this time, but we cannot too urgently advise them to take out a perpetual ticket for hospital attendance at the commencement of their studies. Not that during the first two years the tyro should make it a principal subject of attention, but that he may have the right of entering the wards, of habituating himself to the sight of disease, of seeing important operations, and so gradually and insensibly prepar-

[1] It is a matter of no importance whether botany or natural history be attended first, but it is of great consequence to the comfort and general advantage of the student that they be not studied at the same time.

ing himself for the future. It is at the commencement of the third year, however, that hospital study should assume the chief place in his system of education, and he will be wise never to let it afterwards hold an inferior position. His object in attending an hospital should be, while perfecting himself in observation, while extending his knowledge of facts, and connecting them with the principles he has acquired, to obtain what is called experience and practical tact, a kind of knowledge that cannot be obtained from books or lectures, and is only to be acquired at the bed-side. Its value cannot be too highly estimated, because, as the theory or science of medicine is imperfect, its shortcomings and redundancies can only be remedied by continual observation and experiment, such as the wards of an hospital present us with. Now, therefore, the student should commence the serious study of disease, at first in the surgical wards, where it is external, and directly presented to the sight, or the other senses; and, secondly, in the medical wards, where it is internal, and more a matter of inference and correct reasoning. This latter is generally done in the fourth year. But whilst engaged in acquiring experience, the knowledge of facts and theory should be kept up and extended. Many will further be obliged to follow a second course of systematic surgery, and this should constitute now the chief subject of study, especially as clinical surgery, and carefully watching surgical cases in the hospital are also essential. In addition, a systematic course of the Practice of Physic should now be attended, without, however, at this time, doing more than deriving from it a general knowledge of internal diseases. In short, during this third year, the student should take care and add to his previous knowledge a good acquaintance with Surgery and the Materia Medica.

During the third summer session, the student should pay attention to Dispensary practice, continue his attendance on the hospital wards, surgical or medical, according to the nature of his requirements, and attend one or more of those special classes which are delivered in every great school of medicine.

The fourth and last year should be entirely devoted to practical studies, the chief being Practice of Physic, Clinical Medicine, and Midwifery, with careful observation of cases in the medical wards. In summer, the course of Medical Jurisprudence must be followed. The student should also again attend the Dispensary, treat cases for himself, under the eye of a good practitioner, and pay attention to Practical Midwifery. During all this time, while extending his powers of observation and his experience, theory should be advanced to its farthest limits by the acquirement of a true pathology. At this time, one of the most important subjects to attend to is morbid anatomy, as studied at post-mortem examinations. For, if descriptive anatomy be useful as surgeons, morbid anatomy and general pathology are infinitely more useful as physicians. Indeed, considering how few operations any one student may have to perform while every practitioner, whatever be the sphere of his practice, has

to treat alterations in the structure of various tissues and organs, the importance of morbid anatomy cannot be too strongly urged. Moreover, there are few medical men, who are not occasionally called upon to open a dead body, and report upon the morbid appearances observed in reference to some legal investigation. His power of accomplishing this duty well, will entirely depend upon his previous experience in morbid anatomy; on having seen many well conducted *post-mortem* examinations, and on having carefully handled and examined the diseased parts. Not only may his reputation among his fellow-citizens be gravely affected by his conduct on such an occasion, but the vindication of innocence or the detection of guilt, the lives of the accused, and the satisfactory accomplishment of numerous judicial inquiries, may hang upon his correct judgment. How culpable, therefore, the man who, knowing his future responsibilities in these things, habitually neglects the *post-mortem* examinations of a large hospital!

In addition to those branches of instruction which are considered imperative, some students will further avail themselves of special classes, according as their time and means allow, and in pursuance of the particular direction in which they intend to practise. Thus, in most of the large schools of medicine throughout Europe, there will be found extra means of becoming practically acquainted with comparative anatomy, histology, operative surgery, analytical chemistry, ophthalmology, stethoscopy, pathological anatomy, insanity in a well-conducted asylum, etc., etc.; and such special courses are calculated to give a greater impulse to individual study, and render it more perfect than may at first sight be imagined.

In the sketch of medical education now given, it has been our endeavour to point out, that there are three essential parts,—1st, facts; 2d, theory; and, 3d, experience. It is by the combination of these three divisions of knowledge—by an appreciation of the advantages to be derived from each—by correcting the faults inherent in one by proper application of the others, that the student may be expected at length to obtain just views of the science and art of medicine. He will, we trust, also see the importance of studying these three divisions in their proper order. Indeed, on this point there can be no difference of opinion, as every body must learn in this way, and no other. For if any one attempt to master theory without an acquaintance with facts, or endeavour to profit by experience without any general notions of health or disease, we need scarcely say, that very gross ignorance must be the result. Indeed, it is to the imperfect education, arising from undue attention to one or more of these subjects, from a limited acquaintance with facts, or from a too theoretical disposition uncontrolled by experience, that we may ascribe the ridiculous doctrines which have originated among ignorant practitioners, and been spread amongst a still more ignorant and credulous public. To the student, order and method at the

commencement will save him time and labour. However hard and dry the toil at first, in the end he is sure to be rewarded by the consciousness of time well spent and labour properly directed. How often have we seen a student, in his fourth and last year, instead of zealously deriving benefit from hospital attendance, and feeling interested in the post-mortem examinations, deserting the wards and pathological theatre, in order to follow some elementary course of anatomy, or of chemistry. He feels convinced too late, that what *must* be done, had better have been accomplished long before, and that, in order to acquire elementary knowledge, he is under the necessity of sacrificing that precious experience, which he may never again have the opportunity of obtaining. We trust the observations now made, will at least guard our young friends from this danger, and impress upon them the importance of acquiring, in their proper time and order, elementary facts, correct theory, and personal experience. We would say to them—determine from the first to employ your faculties honestly in the acquirement of sound instruction. Let your aim be directed to a thorough knowledge of your responsible profession, and you will have nothing to fear from examinations. Above all, avoid those attempts to burden the memory, and those artificial systems of acquiring a parrot-like information, into which the base and interested endeavour to entrap the unwary.

A mind so educated and regulated will not be likely to go astray, and its possessor, while alive to the necessity of following the progress of the science and art of medicine, will in this manner best learn, amidst the multitude of suggestions, the number of theories and the opposing statements that may perplex him, to reject what is worthless, and only adopt what is truly useful. He will despise the miserable vanity of announcing what is new, without a scrupulous regard to its being correct. He will, while retaining the right of thinking boldly for himself, not forget that observation is difficult, theory imperfect, and experience fallacious. He will not, therefore, rashly substitute his own authority for that of those whose knowledge is more extensive, or commit himself to the ephemeral doctrines of the day, by which a few otherwise respectable men have lost their professional reputation. He will remember, that the conclusions of youth are almost always modified by the experience of age, and that the wisest and most eminent men of science have given the best proofs of a solid understanding, by the readiness with which they have acknowledged their own ignorance.

In conclusion we have again to repeat, that the order of medical study we have recommended, seems to us the best applicable for the student in Scotland, who dedicates four years to the acquirement of sound professional knowledge. But where the time allowed is necessarily shorter, or when it is important to pass the examination of a special board which has issued imperative regulations as to the order, as well as the subjects of study, care must be taken to modify it accordingly.

COURSE OF STUDY REQUIRED BY THE VARIOUS BOARDS OF THE UNITED KINGDOM.

	Age.	Anatomy.	Physiology, or Institutes of Medicine.	Demonstrations.	Dissections.	Surgery.	Practice of Physic.	Chemistry.	Practical Chemistry.	Materia Medica.	Medical Jurisprudence.	Midwifery.	Natural Philosophy.	Botany.	Natural History.	Practical Pharmacy.	Pathology, or Morbid Anatomy.	Surgical Hospital.	Clinical Surgery.	Medical Hospital.	Clinical Medicine.	Midwifery Hospital.
	Years.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.
Edinburgh University, M.D., - -	21	6	6	6	6	6	6	6		6	3	6		3	3	6	6		3	12	6	
University of Glasgow, - - -	21	6	6	6	6	6	6	6	3	6	3	6		3				24	24	24	24	
University of Aberdeen, - - -	21	12	6	6	6	6	6	6	3	6	3	6		3		3		24	3	24	3	
University of St Andrews, - -	21	12	6	12	12	6	6	6	3	3		3						24	6	24	6	
London University M.B., 1st, -	19	6	6	9	9	6	6	6	1 crs	1 crs				6		1 crs.						
" " 2d, - -	21				6						1 crs	1 crs						12	12	12	12	
Dublin University M.B., - -		6	6	6	6	6	6	6	3 ms	6	3	6		3							9	
" " Surgical Diploma,		18	18	18	18	18	6	6	3	6	3	6		3				27	27	27	27	
The Queen's Univer. of Ireland, 1st,		6	6	6	6			6	3	6				6				6	6	6	6	
" " " 2d,		6	6	6	6	6	6				3	6				3 mos.		18	18	18		3
Royal Col. of Physicians, Edinburgh, Graduates of British Universities admitted by ballot; if of Foreign Universities they are examined.																						
Royal Col. of Physicians, London, -	26	6	6			6	6			3	3	6		3			6			36	36	
King & Queen's Col. of Phys., Ireland,		6	6	6	6	6	6	6	3	6	3	6		3				24	24	6	6	6
Royal Col. of Surgeons, Edinburgh,		12	6	12	12	12	6	6	3	6	3	3				6		21	6	21	6	
Faculty of Phys., and Sur. Glasgow,	21	12	6	12	12	12	6	6	3	6	6	6		3		6		21	6	21	6	
Royal Col. of Surgeons, London, -	21	18	18	18	18	12	6	6		3		3				6	{	3 win. 2 sum.		1 win. 1 sum.		To be attnd.
Royal Col. of Surgeons, Dublin, -		18	18	18	18	18	6	6	3	3	3	3						27	27	27	27	
Apothecaries' Hall, England, - -	21	12	12	12	12		6	6	3	3	3	6		3		Appr.				18		To be attnd.
" " Ireland, - -		6	6	6	6	6	6	6	3	6	3	6		3		Appr.		18	18	18	18	30 cas.
Army Medical Board, - - -	21-25	24		12	12	12	12	12	6	3		6	6	3	3	3 mos.		18	8	18	8	
Navy Medical Board, - - -	20-26	12		12	12	18	18	3	3	6		6		3		6		18	6	18	6	Certif. of cas.
E. I. Company Medical Service, -	22-28						12					1 crs									6	6 cas.

Information respecting exceptions to these regulations under various circumstances, and other details as to the order in which, according to some Licensing Bodies, the courses should be taken out, etc., must be obtained by consulting the published Charts of the Colleges, etc.

To render this number truly useful to the student, we give three other tables. 1st. The variable kinds of study which, to the great injury of medical education in Great Britain, are put forth by different licensing boards. 2d. A maximum plan of medical education for a student who dedicates four years to the study, has ample means, and desires to obtain first-class honours. 3d. A minimum plan of medical education for a student who can only dedicate three years to the study, and who only wishes to pass the College of Surgeons. Both these plans of study are mere general guides, admitting of considerable deductions, additions, and modifications.

PLAN OF MAXIMUM COURSE OF STUDY, OCCUPYING FOUR YEARS.

FIRST YEAR.

Winter.	Summer.
Chemistry.	Botany, or Natural History.
Natural Philosophy.	Practical Chemistry.
Anatomy.	Dissections.
Dissections.	Comparative Anatomy.
Anatomical Demonstrations.	Anatomical Demonstrations.

SECOND YEAR.

Materia Medica.	Natural History, or Botany.
Surgery.	Dissections.
Dissections.	Anatomical Demonstrations.
Anatomy.	Practical Chemistry.
Institutes.	Practical Pharmacy.
Anatomical Demonstrations.	Hospital Attendance.
Hospital Attendance.	

THIRD YEAR.

Surgery.	
Materia Medica (if necessary).	Histology, Opthalmology, or other special classes.
Institutes (if necessary).	Hospital Attendance.
Practice of Physic.	Clinical Surgery.
Dissections.	Dispensary Practice.
Hospital Attendance.	
Clinical Surgery.	

FOURTH YEAR.

Midwifery.	
General Pathology for M.D., Edin.	Medical Jurisprudence.
Practice of Physic (if necessary).	Dispensary Practice.
Hospital Attendance.	Practical Midwifery.
Clinical Medicine.	Stethoscopy, Morbid Anatomy, or other special classes.
Post-mortem Examinations in the Pathological Theatre.	Hospital Attendance.
Occasional attendance on an Asylum for the Insane.	Clinical Medicine.

PLAN OF MINIMUM COURSE OF STUDY, OCCUPYING THREE YEARS.

FIRST YEAR.

Winter.	Summer.
Chemistry.	Botany.
Anatomy.	Dissections.
Dissections.	Anatomical Demonstrations.
Anatomical Demonstrations.	Practical Chemistry.
Hospital Attendance.	Hospital Attendance.

SECOND YEAR.

Winter.	Summer.
Surgery.	
Anatomy.	
Institutes.	Practical Pharmacy.
Dissections.	Dispensary Practice.
Anatomical Demonstrations.	Hospital Attendance.
Hospital Attendance.	Clinical Surgery.
Clinical Surgery.	

THIRD YEAR.

Materia Medica.	
Surgery.	Medical Jurisprudence.
Practice of Physic.	Midwifery.
Hospital Attendance.	Hospital Attendance.
Clinical Medicine.	Dispensary Practice.
Post-mortem Examinations in the Pathological Theatre.	

Part Third.

PERISCOPE.

PHYSIOLOGY.

M. CLAUDE BERNARD ON ABSORPTION.

Influence of Cerebro-spinal Nervous System; of the Sympathetic System; connection between Secretion and Absorption; Influence of Abstraction of Blood, of Atmospheric Pressure, of Electricity and Galvanism, on Absorption.

The nervous system does not act as the conductor of poisonous substances. Strychnia, when placed in the cellular tissue is not absorbed by the nerves, and when placed on an exposed nerve it has no poisonous effect. If strychnia is placed on the nervous centres, or in the centre of the cerebral substance, it is quite inert unless when it is absorbed by the vessels. Yet this substance, in producing convulsions, acts *upon* the nervous system. The veins alone absorb poisons; the nerves and arteries are not employed in their transmission. All this has been proved by Magendie.

The nervous system does not appear to have any direct influence on absorption. Paralysed members absorb, otherwise they would die. But although the cerebro-spinal nervous system has no effect on absorption, the great sympathetic exerts an indirect influence on this function. We know that on dividing the nervous filaments of the sympathetic the temperature is raised, and the circulation increased in the parts to which they are distributed. The phenomena of absorption are increased at the same time, for the conditions produced are those which are favourable to their development. We find that these phenomena occur in deep-seated organs, especially in those which are secretory in their functions. Thus, on dividing the sympathetic branches which supply the liver, the kidneys, or the salivary glands, their circulation becomes accelerated, and their secretions are increased. The phenomena of circulation and secretion are intimately connected with each other; and the latter are increased in exact proportion to the augmentation of the former.

There exists a relation between *absorption* and *secretion*, just as between endosmose and exosmose; the *sympathetic* and the *intermediate*. In the liver there is an external as well as an internal secretion; the former is that of bile,

which is carried away by the bowels; the latter is that of sugar, which becomes diffused in the blood. Probably there exists an internal secretion within the spleen, with which we are as yet unacquainted. The internal secretion of the liver is intimately *en rapport* with active absorption; and *a want of harmony between these processes produces diabeties.* When we irritate the cerebral peduncles we augment the circulation within the liver, and by so doing excite the increased production of sugar. The sugar thus secreted is not destroyed, because absorption is not proportionably excited.

It is of importance to know the influence which *abstraction of blood* has upon absorption. M. Magendie's experiments show that by abstracting blood, and thereby diminishing its lateral pressure, the power of absorption is increased; but that this process is impeded by distension of the vessels with blood. Hence a less quantity of a poison is required to destroy an animal having little blood, than another which is full blooded.

Absorption is influenced by certain physical and chemical causes. It is influenced by *atmospheric pressure.* A dense compressed atmosphere drives the fluids of the body inwards. Thus, undue pressure on the peripheral capillaries may occasion internal congestions. A rarefied atmosphere, on the other hand, by diminishing the pressure, causes the blood to be sent to the surface, and produces epistaxis. Absorption, therefore, which takes place from without inwards, is diminished by a rarefied, but increased by a heavy compressed atmosphere. A practical application of this fact has been made in medicine. It has been proposed, in the case of wounds made by poisoned instruments or venomous animals, to place a cupping-glass over the place, to prevent absorption of the virus, and to favour the withdrawal of what had been already absorbed. Barry made many interesting experiments which proved that the application of cupping-glasses to poisoned wounds, retarded, for a long while, the process of absorption.

M. Bernard has repeated the experiments of M. Fodéra, and has found that *electricity* and *galvanism* greatly augment the process of absorption.—*L'Union Médicale.*

ANALYSIS OF THE EXCREMENTS OF MAN AND ANIMALS IN THE HEALTHY CONDITION. BY WILLIAM MARCET, M.D.

The author describes a new method of extracting the immediate chemical constituents of the excrements of man and animals, and gives an account of the substances obtained by its employment. Healthy human fœces are boiled to exhaustion in alcohol. The residue is insoluble in œther, and yields to boiling water nothing but ammoniaco-magnesian phosphate. The strained alcoholic solution deposits, on standing, a sediment, from which it is decanted, and then mixed with milk of lime. The subsiding lime is of a yellow brown colour; it is dried on filtering paper, and treated with œther, cold or hot; and the solution thus obtained, yields, on spontaneous evaporation, beautiful silky crystals, which are purified by solution in a mixture of alcohol and œther, repeated filtration through animal charcoal, and recrystallization; they then appear in circular groups, have the form of acicular four-sided prisms, and polarise light very readily. This crystalline body the author proposes to call Excretine, the properties and various compounds of which he minutely describes. From this investigation it appears that healthy human excrements contain: —

1. A new organic substance, possessing an alkaline re-action, which the author names Excretine.
2. A fatty acid, having the properties of margaric acid, but not constantly present.
3. A colouring matter, similar to that of blood and urine.
4. A light granular substance, whose properties have not yet been sufficiently examined to admit of its being considered a pure substance.
5. An acid olive-coloured substance of a fatty nature, named Excretoic acid.
6. No butyric acid and no lactic acid.

The fœces of various animals were submitted to the same process of analysis, with the following results :—

1. The excrements of carnivorous mammalia—viz., the tiger, leopard, and dog (fed on meat), contain a substance allied in its nature to excretine, but not identical with it. They contain no excretine; they yield butyric acid, which is not present in human excrements.

2. The excrements of the crocodile contain cholesterine and no uric acid; while those of the boa yield uric acid and no cholesterine.

3. The fœces of herbivorous animals—viz., the horse, sheep, dog (fed on bread), wild boar, elephant, deer, and monkey, contain no excretine, no butyric acid, and no cholesterine.—*Proceedings of R. Society and Med. Times and Gaz.*

DONDERS ON THE RELATION OF THE COLOURLESS TO THE COLOURED BLOOD CORPUSCLES.

The relation of the white to the red corpuscles, estimated heretofore as 1 : 8 or 1 : 10, is, according to more recent observations of Donders and Moleschott, much below this; they estimate it at 1 : 373 ; their results are based on the examination of blood from seven individuals at different periods of life. They found the proportion to be, in the ages between $2\frac{1}{2}$ and 12 years, 1 : 226 : 30 to 50 years, 1 : 346 ; old men, 60 to 80, 1 : 381. In females, after menstruation, the numbers were as 1 : 247 ; females not menstruated, 1 : 485 ; in pregnant women, 1 : 281. The colourless corpuscles increase after food, and are diminished by fasting. Food rich in albumen increases the quantity of colourless cells.—*Schmidt's Jahrbucher and Brit. and For. Med. Chir. Review.*

PRACTICE OF PHYSIC.

PROFESSOR OPPOLZER'S CASE OF HEMORRHAGE FROM HEPATIC ABSCESS.

The following interesting case was related by the above author to the Vienna Society of Physicians :—

The patient was a young woman, æt. 31, who first menstruated at the age of 25. In her 30th year she became pregnant, and her gestation was quite normal, with the exception of intercurrent redness and pain of the ankle-joint. Her labour occurred on the 11th January, and required the use of forceps, and forcible detachment of the adherent placenta. The perineum was ruptured; there was considerable uterine hemorrhage for five days after labour; and there was also pain of the inner and back part of the right thigh, and anæsthesia of the right buttock. On the 1st February (20 days after her confinement), she was very pale and emaciated, and felt lancinating pains in the left hypochondrium, which extended into the epigastrium, and thence towards the umbilicus, rendering respiration difficult. At this period she observed, for the first time, a swelling in the epigastrium, but she admitted that for several years back she had not been able to lace her stays without occasioning dyspnœa, and pain in this region. On the 16th February she entered Professor Oppolzer's clinique. On admission she had no cerebral symptoms, nor jaundice ; she was very anæmic, and had a *bruit de diable* in the neck ; the conjunctivæ had a slightly yellowish tinge ; there was paleness of the mucous membranes, and the skin was waxy coloured. Pulse was 128, and very small. Resonance on percussion terminated on the right side, below the 5th, and on the left side below the 3d rib. A large tumour extended from the false ribs on one side to those on the other, and from the ziphoid cartilage to the umbilicus, which was most prominent in the epigastric region. This tumour was hemispherical in shape ; it rose during expiration, and fell during inspiration ; and on auscultation a friction murmur was heard in it. The skin covering it was foul coloured and

wrinkled. The tumour was painful to the touch; and it had for the greater part a smooth firmly resistant feeling; it was softer in consistence at its convex prominence, and in that place it gave a sense of obscure fluctuation. It was most easily felt, as a hard solid mass, at the external border of the right rectus muscle. The other parts of the abdomen were moderately distended, soft and elastic to the touch. The patient complained of lassitude, vertigo, tinnitus aurium, and great thirst.

Professor Oppolzer diagnosed the presence of two distinct morbid processes in an enlarged liver. The hard, resistant, smooth, and elastic portion of the tumour he considered to consist of fatty liver; while the tumour, of the size of the fist, which extended from the sternum towards the umbilicus was regarded by him as a mobile tumour, unadherent to the abdominal parietes. This, he thought, could only be an abscess, or an inflamed hydatiginous cyst, inasmuch as the age and constitution of the patient excluded the presumption of an encephaloid cancer. The nature of the tumour, the pain felt in it, the weakness of the patient, and the fever, all seemed to indicate the presence of an abscess; while, on the other hand, the absence of etiological explanations, as injuries or metastasis, militated against the conjecture. Its non-adherence to the abdominal walls was explicable by the presumed presence of fluid (the product of puerperal peritonitis), within the peritoneal cavity.

After admission the pain diminished, but fluctuation in the soft portion of the tumour soon became more distinct; and diarrhœa supervened, with ædema of the feet and labia. The labia eventually became gangrenous, and the patient died on the 21st March.

Post-mortem.—Numerous adhesions were visible on the peritoneum, and its cavity contained 15 lbs. of turbid serum. In the left lobe of the liver there was found a large round sac, the size of the head, the walls of which were irregular in conformation, and averaged from 1 line to 1½ line in thickness. Here and there were small purulent collections in the walls of the sac. The surrounding substance of the liver was pale and compressed. The cavity of the cyst was filled with a thick fluid of a dark reddish-brown colour, partly adherent to its walls, and also with *recently formed blood clots.* The posterior wall of the sac had penetrated through the liver to its surface, but the cyst had nowhere contracted adhesions. It was covered by soft greyish granulations of the size of poppy seeds. A mass of brownish concretions, like fibrous uterine polypi, were attached to the right ovary: and there was a communication between the vagina and the abdominal cavity through which these could be felt.—*Zeitsch. der Gesells. der Aertze zu Wien*, June 1854.

ON THE NATURE AND CAUSES OF GREEN VOMITING. BY DR FRAZER, DUBLIN.

The ejection of green-coloured fluids from the stomach is noticed in the course of many abdominal diseases; thus we meet it during attacks of peritonitis, puerperal fevers, and continued fevers with gastric complications, in strangulation, in intus-susception of the bowels, in that rare affection, acute gastritis, and in various enteric inflammations. Its appearance has been attributed by many to the presence of bile in the secretions evacuated during protracted vomiting, and we often find it described as consisting of *green bilious matter.* For various reasons, this explanation did not appear at all satisfactory to me.

I consider this green matter is composed of the blood in an altered and modified condition, the discharge being altogether of a hemorrhagic character. Repeated microscopic observations, in those instances in which I have noticed its occurrence for some time past, has confirmed my ideas regarding its nature, and examination shows that the little fragments of which it consists are various sized clots, containing abundance of blood globules; this alone might decide the inquiry, however I am desirous of placing it on other grounds, in addition to such as are afforded by the microscope. I may, therefore, bring forward the following arguments which further support the view I have taken. 1*st*, The

form of vomiting alluded to occurs in diseases where there is much congestion, and often inflammatory engorgement of the vessels of the stomach or intestines, as in gastritis, and fevers with gastric complications; and after death we find patches of softening in the mucous membrane, or decided marks of excessive vascularity; at least such has been the case in those fatal instances where I have had the opportunity of inspecting the stomach. *2ndly*, The coincidence of vomiting of ordinary blood, in addition to the green matter, which sometimes occurs (and as was noticed, for example, in the case of the woman with enlarged spleen already alluded to); besides, I have remarked, that in many cases of fever which were attended with this peculiar green discharge, hemorrhagic purpuric eruptions were frequently seen. *3rdly*, We have the physical properties of this substance; it forms small green clots in the fluid, in addition to tinging it with its peculiar hue, as we might expect blood to do. *4thly*, We must consider that a smilar green colour occurs in various parts of the body, and under very dissimilar circumstances. This is, perhaps, the most interesting branch of our subject, and one on which medical records afford little information; as instances of this change we have, among other examples,—The peculiar green hue of contusions that are of some standing, and the green colour of the ecchymoses that occur on the extremities in land scurvy, many cases of which were noticed in the hospitals in the late years of famine. The colour of the great intestine in various cases of fatal dysentery, and of the mucous tissue of the stomach in inflammatory gastric affections. The grass-green appearance of the interior of the bladder, and of the kidneys after death, in many acute or sub-acute diseases. The well-known greenish tinge often seen in moist gangrenous affections. The colour of the body as it begins to decay after death. The green hue of coagulable lymph, and of purulent exudations in bad constitutions, and many other similar facts in pathology that might be enumerated, which are as yet unexplained.

If we reject all extraneous considerations from the foregoing class of morbid changes, we have, I conceive, one uniform fact in them all, namely, the presence of blood undergoing some of its mysterious changes; what these are will require additional investigation; but I think we may conclude, that the death of the blood is essential—that having lost that vital attraction for oxygen which distinguishes it, and acted on by extraneous sources (probably deoxodizing ones during decay), it becomes changed in its hue.

With regard to its practical bearings, there is much importance in the view now brought forward; it cannot be regarded as a mere theoretic matter, and, therefore, of secondary value, for those green discharges being recognised as hemorrhagic, will go far to account for the collapse, cold extremities, failing circulation, and other alarming symptoms that attend its presence when ejected in any quantity; and with reference to *treatment*, which is the ultimate aim of medical knowledge, we have a rational explanation of the benefit that creasote, ice, and other well-known styptics exert in checking it.—*Dublin Hosp. Gaz.*

COD-LIVER OIL IN CONSUMPTION. BY PROF. WOOD OF PHILADELPHIA.

Professor Wood remarked to the College of Physicians of Philadelphia, that he had looked to the obituary tables accompanying, from year to year, the reports on meteorology and epidemics, with deep interest, in reference to an important therapeutical question—the efficacy of cod liver oil in the treatment of pulmonary consumption. The oil has been almost universally employed in this disease; and, during the first years after its introduction, a most striking effect was observed—the number of deaths from consumption diminishing surprisingly. Now, there appeared to be no other cause to which this diminution of deaths could be attributed, excepting the use of cod-liver oil. Still, Dr Wood had been fearful of attributing too much to the influence of this agent, inasmuch as it was known to have the effect of postponing the fatal event—of prolonging without eradicating the disease—and hence, might cause

the mortality from consumption to be thrown into future years. Dr Wood had looked with some solicitude to the report for the past year, for a solution of this question ; and he was happy to find the augmentation in the deaths from consumption, in 1853, no greater than is indicated by the report. This speaks very favourably for the remedial powers of cod-liver oil. There was to be anticipated an increase in the mortality from consumption during the past year, as the postponed mortality of the disease in former years would be thrown upon this. Hence, from a decrease in the proportion of deaths from consumption, since the period when it used to be between a sixth and a seventh of the whole mortality, we have a right to infer that we have gained something from the use of the oil in that disease ; probably that we have cured by it one in every eight cases, with the anticipation of a still larger proportion hereafter.—*Summary of Trans. Coll. Phys. Philad.* vol. ii. *N. S.;* No. 4.

ELASTIC AND MEDICATED COLLODION.

Simple collodion is apt to become too stiff, and to desquamate ; hence various expedients have been resorted to in order to obviate this, and to render it pliable and elastic for medical purposes. M. Robert Latour has proposed the following formula for the manufacture of elastic collodion :—

Collodion,	30 grammes,	
Venice Turpentine,	1·5 décigrammes,	
Castor Oil,	5 grammes.	*Misce.*

This fluid is to be applied by means of a hair-pencil, taking care to make the layer sufficiently thick to be resistant. A simpler elastic collodion, less solid than the foregoing, may be made thus :—

Collodion,	40 grammes,	
Castor Oil,	5 do.	*Misce.*

Glycerine may be substituted for castor oil.—*Arch. Gen. de Méd.*, July 1854.

It has been proposed to combine various medicines with collodion, to render it a suitable application to various cutaneous inflammations. M. Aran has proposed mixing collodion with an equal quantity of the tincture of the perchloride of iron. Dr Hannon, of Brussels, recommends very strongly a combination of collodion and acetate of lead, made by adding 30 grammes of collodion, drop by drop, to a warm concentrated solution of the said salt. The collodion thus formed is said to be very flexible and elastic.—*Presse Médicale*, 1850.

LACTIC ACID IN DYSPEPSIA.

Dr C. Handfield Jones, advises the use of lactic acid in dyspepsia. He has chiefly given it in cases of irritative dyspepsia, where the digestion was painful and imperfect, and had been so for some time ; he does not advise its use at the commencement of the treatment in a severe case, but only after irritation and vascular erethysm is somewhat reduced. It should be employed in doses of fifteen to twenty minims, in a half ounce of water, and taken *at* meal times ; he states that it seems to mingle with the food, and to supply one of the constituents of healthy gastric juice, which is probably imperfectly produced. Its use need not be confined to cases of dyspepsia, but may be extended to all cases where it is desirable to improve the tone and power of the stomach. It is pleasant, occupies but little space, and the only objection to its use is its present high price ; but if much employed it could probably be obtained cheaply.—*Association Med. Journal.*

[We have long been in the habit of giving muriatic acid to meet the same indications.]

SURGERY.

ON THE CURABILITY OF CANCER, AND ITS DIAGNOSIS BY MEANS OF THE MICROSCOPE.

At a meeting of the *Academy de Médicine*, held on the 26th September, M. Jobert presented a report on a case of sarcocele in a young infant, in which M. Pamard of Avignon operated, and *cured* the little patient. The alleged *cure* in this case gave rise to an interesting discussion, which is not yet concluded, on the curability of cancer, and its diagnosis by means of the microscope.

M. Robert doubted if encephaloid cancers were ever cured. The little patient had, doubtless, recovered from the disease, but it was not exempt from its recurrence.

M. Velpeau moved that the question of cancer should be brought before the Academy at an early meeting. All surgeons were not of M. Robert's opinion as to the incurability of cancer. He expressed doubts as to the value of the microscope in the diagnosis of cancer, and undertook to prove its fallibility in the diagnosis and prognosis of cancerous tumours.

October 3d.—On the President announcing that this was the day appointed for the discussion of cancer, much amusement was created by M. Velpeau declining to speak before M. Robert, and M. Robert refusing to speak before M. Velpeau.

M. Leblanc opened the debate on the curability or incurability of cancer, by briefly going over the researches which he had made on the subject with M. Trousseau, and those he had lately made microscopically, conjointly with MM. Lebert and Follin. He considered the microscope of great service for the diagnosis of the cancers so common among animals. He did not believe in the incurability of cancer; and considered that an *early* operation diminished the chance of recurrence. His speech contained much valuable matter, but no new facts.

M. Barth followed, making, on this occasion, his *début* in the Academy. His address was a very excellent one. As to *diagnosis*, he considered that, in a very great number of cases, this may be made with the naked eye, but that sometimes the characters were better seen by means of the microscope. In these first cases the microscope afforded additional certainty of diagnosis. In a certain number of cases the physical characters are doubtful, and in these the microscope furnished an essential element of diagnosis; but it did not always remove doubts, and might sometimes even occasion mistakes. As to the *curability* of cancer, he thought that absolute incurability was disproved by facts, and unsupported by analogy. We should take into account all the characters of the tumour—those inherent in it and accessory to it—in order to determine the possibility of chances of recurrence.

M. Gerdy made some very interesting and novel observations on the anatomico-pathological characters of the sanguineous cancerous tumours. It was singular that, although these tumours were known in science, there was nothing said about them in books. How were they to be distinguished? Chiefly by their anatomical characters. They were rich in blood-vessels, and in *splenoid* tissue analogous to the placenta. Surgeons know that they recur after extirpation. Some of these tumours were situated in or near the bones, and were vascular and pulsatile, analogous to the spleen or placenta. Breschet, Dupuytren, and Lallemand, called them simple tumours, although they have found colloid matter in them; but they are not such. He considered the microscope useful in many cases, but in others as likely to confuse.

October 10th.—M. Robert—I have said that the cure of cancer—especially of the encephaloid variety, is an exceedingly rare fact, if it can be well proved. It seems that M. Velpeau is not of this opinion. On consulting his work on diseases of the breast, I find that he has operated in 250 cases, in which the patients have survived the operations. He lost sight of 100 of these patients after one

year, 150 after two years, and 200 after five years. It is matter of regret that, in these cases, the final result was not ascertained. Of the 50 remaining, 20 have been free from the disease for five, ten, fifteen, twenty, and even twenty-five years; and these cases M. Velpeau regards as radical cures. This enormous proportion of cures seems astonishing, when we call to mind the results recorded by other eminent surgeons. Munro says, that 4 out of 60 cases in which he operated for cancer were free from the disease for two years; but that three of them eventually had occult cancer of the mamma, and the fourth an ulcerated cancer of the lip. *Ergo, 60 operations and 60 relapses.* Mayo says, that carcinoma of the mamma, even when operated on under the most favourable circumstances, and removed early, recurs in 95 *cases out of every* 100. Macfarlane operated in 32 cases of cancer, and collected the particulars of 86 other cases of his colleagues; and out of these 118 cases *there was not one in which the malady did not recur.* Boyer says, that out of more than 100 cases in which he has known individuals operated upon for cancerous tumours of the breast, or of other parts, he has only known *four or five cases* which were thereby radically cured. M. Broca, in his prize work on cancer, says, that of 19 cases in which tumours were removed by Blandin, which were demonstrated microscopically to be cancerous, *not one* remained exempt from a recurrence of the disease for two years. Lebert says, that of 34 cases operated on for cancer, 6 died after the operation, 21 had a recurrence of the disease within a period varying from three months to two years; and the remaining 7 were lost sight of. I have said enough to show how unanimous are the opinions of the greatest authors as to the curability of cancer.

I cannot help thinking that the marvellous discrepancy in M. Velpeau's statistics has been due to errors of diagnosis, and to his having mistaken benign for malignant tumours. The surprising cases which he says remained cured for fifteen, twenty, or twenty-five years, must have been seen at a time when an error in diagnosis would have been quite excusable even in M. Velpeau. But M. Velpeau says he has never been deceived; and that in more than a hundred cases his diagnosis has been confirmed by the microscope. This, I would remark *en passant*, is not very flattering testimony in favour of the microscope. Let me recall to your recollection the discussion that occurred in 1844, in the Academy, on fibrous tumours of the mamma. On that occasion M. Cruveilhier described very excellently the anatomical and clinical characters of those non-malignant tumours of the breast, which were often confounded with cancer, and shewed how he had often dispensed with operations by recognising their presence. His opinions, and above all, the name of "fibrous bodies," which he gave these tumours, were combated by MM. Bérard, Blandin, Roux, Amussat, etc., who admitted the existence of such benign tumours, but held that it was impossible to distinguish them from cancer. M. Velpeau, on that occasion defended the opinion of M. Cruveilhier. That he has studied these non-malignant mammary tumours, is evident from a paragraph in his article MAMELLES in the *Dict. de Med.* in 30 vols. He affirms that diagnosis was possible.

But he had not *then* the experience which enabled him *now* to rectify error in the matter. Hear, therefore, how he then expressed himself before the Academy:—"M. Cruveilhier has gone too far in affirming that fibrous tumours of the breast may always be easily distinguished from those which are cancerous. In some cases this distinction is very difficult and even impossible." And he further added—"The defiance which MM. Bérard, Blandin, and Roux have given M. Cruveilhier to recognize on the living woman a benign tremour, I would not be afraid to accept myself; and I do not hesitate to say, that if *I only committed four errors out of ten cases, I should feel quite satisfied.*" Gentlemen, may we not with justice conclude, after this avowal, that among M. Velpeau's cases of cancer cured before the year 1844 there must have been several patients who were operated on for merely non-malignant tumours? And observe that, by a singular coincidence, the proportion of four errors of diagnosis out of ten is precisly that of the successful cases in his statistics—viz. 20 in 50.

Since the year 1844 the study of benign tumours of the breast has greatly advanced. Velpeau erred at first in calling them fibrinous ; and it was only after the researches of Lebert that he gave them the name they still retain, *adenoid* tumours. These researches have greatly elucidated the pathology of tumours, and thus advanced clinical medicine ; while the microscope, in revealing their minute structure, has made us acquainted with physical characters never before observed. I cannot admit the infallibility which M. Velpeau claims for himself, for I believe that cases are still met with which may deceive the most able practitioner. It must therefore be believed that M. Velpeau's results are not rigorously exact, or capable of proving that the cure of cancer is less rare than is supposed.

Hitherto we have only been discussing the two kinds of mammary tumours which are liable to be confounded with each other ; but if we turn our attention to the tumours of other organs, we will find the causes of the errors of diagnosis to be more numerous, and hence we can understand thereby the diversity of opinion which exists among surgeons as to the curability of cancer. In these organs, besides cancer and hypertrophic tumours analogous to those of the breast, we find also epithelial and fibro-plastic growths. The latter, however, present some affinity to true cancer, and their external characters do not permit of their always being clinically recognized ; consequently they may grow again after the operation. Now, persevering microscopical researches, made by eminently qualified and conscientious men, have demonstrated, on the one hand, that the minute structure of such tumours differs essentially from that of cancer, and that often the naked eye may distinguish them from it by incontestible characters. Clinical observation, on the other hand, has shown that if these growths reappear after extirpation, that they do so less frequently than cancer ; that the relapses are generally local, and admit of fresh operations being performed with a better chance of definite success ; and, finally, that their becoming generalised, or causing death, only occurs in rare cases, and is exceptional.

If we would put an end to the confusion which exists at present on all that relates to cancer, and above all to its curability, we should sedulously exclude from the question all those epithelial and fibro-plastic growths, which have only special analogies to it, but which differ essentially from it in their intimate structure and evolution.

M. Velpeau.—M. Robert, in his speech, has only discussed one side of the question, viz., the curability of cancer ; and he has said nothing of the value of its microscopic characters, although he appears to be a partisan of the microscope. He complains that I have not given the definite results in a large number of my cases of operations. Among the 200 patients whom I unavoidably lost sight of, relapses occurred in a certain number of cases, but in the rest I never could ascertain the results. Out of the 50, whom I carefully watched, there are 20, all of whom I could find to-day, perfectly cured. M. Robert explains this considerable proportion of cures by premising errors of diagnosis. I do not pretend to be more infallible than him ; I have sometimes been deceived ; but there *are* cases in which one *cannot* be deceived. I have taken great care to call *cancer* only those cases which presented all the characters with which it was impossible to be deceived at the bedside. In these cases, the microscope has never shown my diagnosis to be fallacious ; and the same has occurred in every case in which I diagnosed an adenoid tumour. Far from undervaluing microscopical researches, I have always encouraged them, for I wish much to see science possessed of more certain marks than at present. But in granting a high place to the microscope for diagnosis, clinical observation must not be excluded. When M. Robert meets an ulcerated, fungous, softened tumour, he cannot be deceived ; nor can he, if he meets one which is hard, knobby, and uneven. For my own part, I confess that in such cases I would not be deceived ; and that the microscope has always confirmed my opinion, I would appeal to M. Lebert, who has for a long while attended my visits at *La Charité*.

M. Robert, in recalling the former Academy discussion of 1844, wishes to make me contradict myself, in making me say that I had then confessed having been deceived. I still frankly admit that there are cases in which I may be deceived. When I say that I have always diagnosed the tumour, I mean in the cases in which they presented their habitual characters.

I repeat I have been far from discouraging microscopic researches. For thirty years I have studied the subject of cancer with lively interest, and a sincere desire to arrive at the truth. In 1836, I stimulated M. Donné to make researches in this direction, and later still, I advised M. Lebert to follow out the inquiry. I said to them, "Search: for cancer ought to have a special character." I was especially anxious that something might be found in the blood of the cancerous, for if the characters of cancer could be found therein, nothing would be easier than, by examining a few drops of blood, to diagnose the disease. Microscopists found nothing peculiar in the blood; but at last they discovered a something, and, overjoyed at this, I was one of the first to applaud the achievement. But is that something sufficient? I believe not; and it is because I consider the microscopical characteristics insufficient that I have studied with the greatest care the clinical indications.

I tried at first to isolate benign from malignant tumours, and I commenced to separate from true cancers those tumours usually confounded with it. I have now come to the conclusion that in true encephaloid cancer, the variety which is most prone to return—a cure *may* sometimes take place. In support of this proposition, I shall cite some facts, and the Academy will judge whether they are good, or whether they deserve the severe judgment pronounced upon them by M. Robert.

In February 1836, a woman consulted me, who had on her breast a large tumour, the size of both fists, which was ulcerated and fungous, discharging an insupportably fœtid ichor, and which crumbled under the finger. There could be no more characteristic specimen of encephaloid cancer, and although all the microscopes in the world had been testifying against me, I should still have held this opinion. Well, I operated on this woman. This was nearly twenty years ago; and I saw the patient recently; she was perfectly cured, and has had no relapse.

Another lady had a tumour very similar to that which I have described, which was removed; the wound cicatrized perfectly, but eighteen months afterwards a tumour very like the original one, manifested itself in the axilla. This second tumour was removed in 1841, and since then she has enjoyed perfect health, and has had no relapse. I may state that M. Jobert was the surgeon who operated the second time. (Here M. Jobert made an affirmative sign.) Are there not other tumours, besides cancerous, which reappear thus, not at the original point, but at a distance? We cannot tell.

I know that to these facts an objection may be raised. It may be said,—"How do you know that the recurrence, which has not happened at the end of two, three, five or ten years, may not take place in twenty, twenty-five, or thirty years?" and in this manner it may be said, if no return of the disease has occurred, that the patient has died before there was time for its manifestation.

It may be objected that in the cases I have just reported, the microscope was not used; but since 1847, when the use of the instrument became general, every tumour that I have extirpated has been examined by means of it. To those who could say, with great justice, that I am not sufficiently qualified for microscopical researches, I would observe that the cancerous growths were examined for me by the learned young microscopists who attended my visits,—by M. Follin, M. Robin, M. Broca, and M. Lebert himself. Now, since this time (1847), all the cases in which I extirpated malignant growths, and in which no recurrence took place, were previously demonstrated by the microscope to be true cancerous tumours. It may be remarked, however, that sufficient time has not yet elapsed to enable us to say with certainty that these patients

are exempt from any chance of a relapse. But we know that when recurrence of the disease happens, it is generally within two years after the operation, and that after this period the chance of this becomes much lessened. Now, these patients have been free from disease for *seven* years. I think, therefore, that it is proven that cures have happened in cases where the existence of cancer had been microscopically examined. I maintain that errors of diagnosis were impossible in the cases I have cited.

M. Robert thinks that I confound different things together; as, for example, epithelial and fibro-plastic tumours. This objection brings me back to the microscope, on the use of which I wish to make a few remarks.

Laënnec divided tumours into two classes, which are merely those called now-a-days, homeomorphous and heteromorphous growths. Among the *heteromorphous* tumours, we class encephaloma, schirrus, phagedenic ulcers, etc. Among the *homeomorphous* we rank fibrous and fibroid tumours, cutaneous excrescences, condylomata, corns, fungosities, etc. Can it be said that there is the least analogy between a malignant fatal tumour, and a corn on the foot? The microscope reveals the same minute structure in a venereal vegetation and in an excrescence (*verrue.*) Are they then the same thing? M. Lebert having remarked that the tumours in question contain only epithelial matter, concludes that they are not cancer. He must have seen many cases, however, since the publication of his *Traité de Physiologie Pathologique*, to convince him that it is very difficult to reconcile clinical facts with the results of microscopical observations.

He says that the composition of these tumours explains their non-recurrence, but this is erroneous, for epithelial tumours return like others. Nay, more, I declare that they return *oftener* than other tumours. However, M. Lebert said to me:—"You do not distinguish, but confound together tumours which are different; those observations only are valuable which are confirmed by the microscope, and all preceding ones are worthless." I cannot entertain this objection. However, I commenced investigating anew; I submitted cancers of the lips to M. Lebert—he found them epithelial, and consequently pronounced them to be non-malignant ulcers. Yet, in the most of these cases, the disease returned after the operation. In the majority of cases, ulcers of the lips are epithelial tumours, and they return like cancers. M. Lebert has been obliged to acknowledge this; but he says that, unlike cancer, they grow on the same place, and produce no general infection. I would answer, that cancer also sometimes returns on the same place; and I would ask if it is true, that epithelial growths only return on the same site? I am inclined to dispute it, and Lebert and his disciples have attempted to get out of the difficulty by saying, that when epithelial cancer does not return at the same place, it recurs in the neighbouring lymphatics (*ganglions*), that is, in tissues directly continuous with those in which the malady primarily appeared. But does not this also happen in cancer?

Homeomorphous tumours, therefore, are exactly like cancer, for they recur both at the same place, and at a distance from it. The micrographers say that they never occur except in the neighbourhood, or in the lymphatics. I need not, in answer, adduce old cases of recurrence in the viscera, for in these the growths were not microscopically examined, but I may point to recent cases in which homeomorphous tumours recurred in different tissues from those primarily affected—as in the bones.

The treatment of the two classes of tumours is the same; we do not apply caustic, the cautery, or the knife, any less to epithelial than to cancerous growths. Wherein, then, do they differ clinically? It may be said that epithelial tumours do not cause death so speedily as cancerous, the latter often recurring in the viscera. But if both are fatal, what matters it where the recurrence happens? To recapitulate:—epithelial tumours recur after extirpation, not only on the original site, but—like cancer—at a distance from it;

they return alike in homologous and heterologous tissues, in the bones, and in the viscera. Call them epithelial cancers if you will, to distinguish them from cancer, properly so called, and from schirrus or encephaloma, but they will always be regarded as cancerous by me.

The so-called fibro-plastic tumours are said to be another variety of homeomorphous growths; but I cannot accept this distinction as a decided fact. It is said that they are simple hypertrophies of a normal tissue, or a production of a tissue analogous to normal tissues. They are evidently another thing. How can we compare, for instance, the enormous fibrous tumours sometimes found on the shoulder; and yet the microscope reveals the same texture in both. Keloid is also fibro-plastic tissue, but it does not resemble the growths of which I have spoken, and yet they are not distinguishable by the microscope. I have seen enchondroma return; I remember removing a tumour of this kind from the foot, which reappeared, after some years, on the thigh. You say perhaps it was cancer; we need more of such cases. There have been more similar to this, and confirmatory of it. I will quote one:—A man presented himself at *La Charitè*, having a tumour which had grown on the site of a similar one removed by M. Malgaigne; and some years before that a tumour of the same kind had been excised by M. Blandin, so that this was the third relapse. The new tumour had burrowed under the cicatrix of the last operation. In spite of the small chance afforded by another operation, I yielded to the earnest solicitations of the patient, and removed the growth, cutting round it very deeply, so as to make sure of leaving none of it in the wound. All went well, the patient seemed convalescent, when I observed, before the cicatrix had formed completely, a nodule commencing below it. We allowed the wound to cicatrise, and I sent him away without saying anything, firmly resolved to make no fresh attempt. Some time after, the tumour was again developed, and the patient, knowing my determination, went to the *Hôtel-Dieu*, to be under the care of M. Roux. Amputation of the thigh was performed, and he left the hospital. He entered the *medical* wards of the *Hôtel-Dieu* sometime afterwards, complaining of something in the chest. All at once a fresh growth had occurred on the leg, and simultaneously tumours of the same nature had developed themselves in the thoracic viscera. The patient died, and his body was examined by MM. Follin, Giraudet, and Verneuil, who found in those tumours nothing but purely fibro-plastic tissue.

I could cite many similar cases; but one or two must suffice. A most beautiful young woman had an enchondromatous tumour on her arm, which had increased to such an extent that her only chance of cure lay in disarticulation of the arm. The patient declined the operation, but remained in the hospital. The tumour grew as large as the head, and she died, and at the autopsy similar tumours were found in the lungs. M. Richard, a great partisan of the microscope, examined these growths, and found them to be composed of only fibro-plastic tissues. I remember another case of enchondroma of the foot which returned after removal; I amputated the leg, and it recurred on the thigh; amputation of the thigh was performed, but the tumours became developed in the lungs.

M. Lebert is aware of these facts, and he admits the recurrence of homeomorphous tumours on the original place, and even at a distance in the neighbouring lymphatics. But that is not enough; he could not deny the existence of cancerous cachexia in an individual having cancroid who had become emaciated, and had a sallow icteric tinge. *There are several species of cancer—cancers with and without cells.*

As regards the cancer-cell, what have we to prove that it is the pathognomonic character of cancer; that it is not, for example, a transitory condition as yet but imperfectly understood? At present there are two classes of microscopists, viz, the clinical and the scientific; the latter variety, who are principally in Germany and in England, naturally attach an absolute value to microscopical characters. The clinical micrographers are less absolute, and I

have more confidence in them. Their assertions, however, must be taken with some reserve. For example, Lebert says,—"There is nothing pathognomonic of cancer, except the cell." Now the cell is not pathognomonic, for we now know that it is the nucleus which is most important. But I am not more predisposed in favour of the nucleus than the cell. Very likely the nucleolus may, next year, be regarded as the all-important element. I sincerely trust that microscopists may arrive at something definite. If I have been somewhat severe upon them, it is because I think they have been too rash, and because I dreaded their going astray. They have affirmed that the cell is pathognomonic of cancer, and when experience demonstrated that their proposition was not always true, they actually tried to explain these pretended anomalies by an alteration of the cell, capable of making it momentarily disappear. If the cell is not *constantly* found in cancer, it cannot be said to be a pathognomonic mark of it.

The truth is, whatever be the explanation of it, *the cell does not always exist in true, well-developed cancer* (I speak of encephaloid); *and it is sometimes found in tumours which are evidently not cancerous.* I could quote many illustrative cases, and especially that of a woman with a non-malignant tumour in which the cells were found, who never had cancer, and who was not the least likely to have it. Many years ago I removed a sanguineous cyst from the lip of a young girl, in the matter contained in which, M. Lebert found cancer cells. The girl recovered perfectly; she has since been married and had several children, and is now in the best health. I removed a non-malignant tumour of the calcaneum, in which the microscopists found cancer cells. The following seems to me very conclusive:—A woman, about forty, had, on the right breast, an encephaloid lardaceous tumour, of that worst kind which returns so soon and always fatally. As the woman was young and fresh-looking, I removed the growth. The tumour was lardaceous schirrus, through which were scattered, here and there, portions of cerebriform tissue, not yet softened. I cut this tumour into five pieces, one of which I sent to each of five microscopists, asking their opinion of it. *None of them could find a cancer cell in it.* The poor woman appeared at first to convalesce, and to confirm the diagnosis of the micrographers. But after a few months, a small tumour appeared near the cicatrix, which was soon followed by general infection, and she died ten months after the operation. A few years ago I performed amputation of the thigh, on a lady, for a tumour containing cancer cells, and she is now in perfect health. Fifteen years ago I performed amputation for a disease of the external condyle of the tibia, containing cancer cells, and the patient is still alive and well.

I may remark that cancers containing *no* cells have occasioned the recurrence of cancers containing cells. We must therefore admit one of two things—either that the recurrence was not of the same nature as the primitive malady, or that the first tumour was the same as the second, but containing no cells. I recollect a case where a tumour of the jaw recurred. There were cells in the second tumour, but none in the first. Must we, therefore, conclude that the first tumour was epithelial and the second cancerous? Assuredly not. The nature of a tumour cannot be judged of merely by the presence or absence of cancer cells.

All these facts constrain me to declare that the microscope has not as yet aided much our diagnosis of cancer, and the question of its curability or incurability. I believe that there is in cancer some unknown and peculiar element which has hitherto escaped our observation. Else why should not the microscope be able to distinguish those fibrous tissues which recur from those which do not? or why should it not explain these recurrences? But on the other hand I am convinced that in doubtful cases the microscope may do us great service by confirming our diagnosis. The microscope is an additional eye which we possess; but we should never forget that this eye cannot enable us to see that which does not exist. It is difficult to avail ourselves of the

instrument, and this is demonstrated by the frequency with which we see the most expert microscopists at variance among themselves.

This is only a part of what I would say on the subject. The continuation of the discussion may probably furnish me with an opportunity for still further giving expression to my sentiments.—*L'Union Médicale.*

(*To be continued, with a Commentary by Professor Bennett.*)

Part Fourth.

MEDICAL NEWS.

PHYSIOLOGICAL SOCIETY OF EDINBURGH.

MEETING XII.—*April* 22, 1854. Dr BENNETT, President, in the Chair.

REPORT OF THE CANCER COMMITTEE.

Dr Bennett stated that the cancer committee had been dissolved without arriving at any report. In proposing the formation of that committee, Dr Bennett had conceived it possible for four or five members of the Society, all of whom were acquainted with the present state of our knowledge concerning cancerous and other gowths, to arrive at definite conclusions as to the structure, nature and definition of such formations. He regretted to say that the committee could not come to a unanimous opinion on any one point concerning them, from which might be inferred the excessive difficulty of framing satisfactory conclusions on the subject in the present state of science. He hoped, however, that on some future occasion, when our knowledge was more advanced, some other committee might prove more successful.

NEMATODE WORMS IN THE HEART.

Dr Bennett was enabled, by the kindness of Dr Paterson of Leith, to show the Society a preparation of a dog's heart, containing numerous nematode worms varying in length from two to four inches. They had been made the subject of research by Gruby and Delafond, who had described them in a series of papers communicated to the *Academie des Sciences*, and who had given to them the names of *filaria papillosa hœmatica canis domestici.*

Mr Barlow had observed the heart of a lion filled with similar filariæ; they had also been noticed in the eye of the horse.

WAXY SPLEEN.

Dr Sanders showed a specimen of the indurated waxy spleen of that kind in which the general structure of the organ, and particularly the pulp, is affected, and where the Malpighian bodies are indistinct and apparently obliterated. In this instance (as in one previously shown to the Society) the characteristic blue reaction of cellulose was very distinctly brought out by the application of iodine and sulphuric acid. Moreover, under these reagents, the pulp of the spleen showed under low powers of the microscope a marked reticulated structure, formed by narrow bands anastomising in a close plexus. These bands presented the blue reaction, while the areolæ which they enclosed were generally unaffected. When separated as well as possible from the corpuscles which adhered to them, these bands appeared composed of the elongated fusi-form or so-called muscular fibre-cells, which are usually described as forming the minute trabeculæ.

ON THE MITRAL VALVE.

Dr W. T. Gairdner explained what he considered to be the mechanism of the mitral valve, and illustrated his views by demonstrations and preparations. Dr G. directed attention to the occurrence of systolic murmurs at the apex of the heart in cases of simple dilatation without valvular disease or deformity, and accounted for the production of these murmurs by the impaired mechanism of the valves. (See Report of Medico-Chirurgical Society of Edinburgh, meeting of May 3, 1854, *Monthly Journal of Medical Science*).

Meeting XIII.—*June* 3, 1854. Dr Bennett, President, in the Chair.

SPORES OF THE LYCOPODIACEÆ.

Dr Bennett exhibited to the Society under the microscope the structure of the inflammable lycopodion powder, or so-called vegetable sulphur. It was composed of rounded cells varying in diameter from the $\frac{1}{1500}$ to the $\frac{1}{800}$ of an inch. They are apparently colourless, the slight amount of yellow they contain being invisible under a power of 250 diameters linear. They are granular on the surface, enlarging somewhat on the addition of water, and on being squeezed between glasses, give out clear globules of a transparent substance, which strongly refracts light, and resembles oil in appearance. This may be the cause of the inflammable character of this light powder, when some of it is thrown upon a flame.

AN ABORTIVE HUMAN OVUM.

Dr Matthews Duncan showed the Society an entire abortion which equalled in size that of the eighth week of pregnancy. On raising the decidua vera and opening into the sac of the chorion through the decidua reflexa, he found it to contain nothing but a limpid fluid. On the external surface of the chorion at the site of its insertion the villi were seen naturally developed. Dr D. had seen several such ova without embryos, but they were generally not above half the size of the one exhibited. He thought they should be distinguished from the moles without embryo, which had to a certain extent a similar history, but in which the membranes representing the ovum were in a manifest state of disease. In the present case they were all apparently healthy.

Meeting XIV.—*June* 17, 1854. Dr Bennett, President, in the Chair.

SPINA VENTOSA IN LEG OF RAT.

Dr Bennett exhibited a hollow osseous tumour formed by expansion of the tibia of a rat. The healthy bone was about one-eighth of an inch in thickness, whilst the tumour was an inch in diameter. It closely resembled a similar tumour of the human femur in the University Museum, which Mr Liston had removed by amputation.

DISEASED PLACENTA.

Dr Matthews Duncan exhibited to the Society a placenta with a coloured drawing and sections of the same. Nearly one-half of the placenta was atrophied, and was separated by an almost straight line from the remainder, which was quite healthy. The morbid portion presented on its uterine aspect a rich light yellow tint, and it was nearly smooth or level in surface. It was of great hardness, not friable, elastic, and its section varying from a fourth to half an inch in thickness, was of the same colour but duller than its surface; it presented numerous large flattened out vascular openings. On examination by the microscope, Dr D. could detect nothing but the appearance of shrivelled and collapsed fœtal tufts with rare and irregular fatty granules, a state of parts which might be expected from the compressed and indurated state of the tissue. This was one of the lesions described under various names as atrophied,

scirrhous, tubercular, and perhaps fatty. Its occurrence was frequently attributed to separation of the atrophied portion in the course of pregnancy, as in many of the cases where it was observed a hemorrhage had occurred some considerable time before delivery. In the present instance there had been no such occurrence. Dr D. remarked that a state of placenta resembling this was sometimes observed in the placenta of a twin which had died, and remained in utero with its co-twin. He thought the most probable explanation of its occurrence was an obstruction of some of the large divisions of the umbilical vessels from disease in them or from some other cause.

Dr Bennett had carefully examined the structure of this placenta, and he demonstrated to the Society that its villi were unusually pale and transparent for the most part, free from accumulations of fatty granules, and closely resembling in appearance the change so well known to the members of the Society, as occurring in the liver, kidneys, and spleen, under the name of waxy.

He took this opportunity of saying that the waxy as well as the so-called fatty degeneration of the placenta had been well known to him for the last ten years. Long before Dr Barnes had described the latter lesion, he had investigated and figured it. (See *Edinburgh Medical and Surgical Journal*, 1843, *and separate work on Inflammation*, 1844, plate, fig. 10.) At that time he considered the fatty degeneration, as he did still, to be a result of exudation of the liquor sanguinis from the blood-vessels, strictly analogous to the softening which occurs in the brain, both being the terminations of a true inflammation of texture. He had always been opposed to the idea advanced by Mr Paget that this change was caused by fatty degeneration of the coats of the minute vessels. Everything he had seen had convinced him that it depended on a change occurring in blood exuded or extravasated from the blood-vessels. Looking at the structure of the placenta he could have little doubt that its texture was subject to exudation (inflammation) or extravasation (hemorrhage) in common with all other highly vascular organs, and that the fatty degeneration was a change occurring in the fibrinous matters so separated from the blood. The nature of the waxy degeneration was not yet fully understood, but he had long separated it from fatty degeneration, believing it at first to be a chronic state of that lesion, where the fatty granules had been absorbed, although he now considered it might sometimes be primary in its character. He had figured these waxy cells in the liver in the eighth No. of his Clinical Medicine, fig. 121.

Dr W. T. Gairdner thought that the indurated state of the atrophied parts in this placenta, and the traces of fatty granules and of yellow pigment pointed out the origin of the morbid condition as being in all probability hemorrhagic extravasation among the villi. He differed entirely from Dr Bennett as to the pathological relations of the lesion. The paleness of the villi under the microscope was the result of their compressed and collapsed condition; and he (Dr G.) could not, in this case, admit any real resemblance of the atrophied parts to the so-called waxy condition of other organs. The liver and spleen, in the waxy state, were hypertrophied, never atrophied. As to the placenta, Dr G. had never seen in it anything approaching to a true waxy degeneration.

Dr Sanders observed that the diseased portion of placenta shown by Dr Duncan was certainly not scirrhous, if by that term were meant a variety of cancer. It was merely indurated and atrophied in consequence of an exudation perhaps inflammatory, undergoing changes of a chronic kind. Dr S. agreed with Dr Gairdner that the lesion was not of the true waxy character, and bore no correct analogy to the waxy conditions of the liver, spleen, or kidneys.

A lengthened discussion ensued, on the obscurity and confusion of pathological nomenclature generally; in which the term "waxy degeneration," or rather "waxy deposit" as indicating the translucent appearance of the Malpighian bodies of the spleen was objected to by Dr W. T. Gairdner, who considered the term "waxy" to apply merely to the condition of organs, in which they presented the hardness, smoothness and some of the other physical characters of wax. This view was opposed by Dr Sanders, who conceived

that we had sufficient data for distinguishing under the microscope, and to a certain extent chemically, the peculiar translucent (or so-called waxy) metamorphosis in diseased organs, particularly in the spleen, liver and kidney; he further considered, that the translucent material which was formed in waxy degeneration of all these organs was pathologically identical; and he thought we ought to dismiss altogether, or at least to put in secondary rank, the mere naked eye resemblance to wax or glue or bacon (as in the terms colloid, baconny, lardaceous, etc.) in furnishing pathological characters; and whether the term waxy was retained or not, at all events, some word to express the degeneration was required without reference to the induration which the organs might present in the extreme degrees of the affection.

Drs Bennett and *Haldane* made some further remarks. Dr Bennett especially dwelt upon the importance in histological investigation of separating fact from theory, and again appealed to the undoubted fact, now demonstrated to the members of the society, that the villi of the placenta exhibited by Dr Duncan, presented exactly the same colourless transparent appearance, with bloodlessness, so characteristic of what was called waxy degeneration. It might be as Dr Gairdner thought, the result of a compressed and collapsed condition of the villi; it might not correspond to certain views of what was waxy in the ideas of those who applied that term to anything resembling wax, or of others who fancied a certain chemical reaction necessary. These were opinions and not facts. Discussions on such subjects were likely to be interminable, unless pathologists could distinguish between identity of structure, similitude of chemical composition, and essentiality in the nature of mordid changes.

As the result of the discussion, in which many of the members joined, it appeared to be the general opinion that it was necessary to take into account both the naked eye distinctions, and those furnished by minute investigation; and that the real difficulty was to find a nomenclature which would express the distinctions observed by the different methods of examination in a clear manner, without either introducing too complicated a system of names on the one hand, or on the other, denoting by the same word appearances which, although perhaps often united, were not essentially identical.

The *President* considered that most important services would be rendered in the present transition state of our knowledge by an efficient nomenclature based on well understood principles, and sufficiently pliable to admit, without confusion, the new facts which were being daily added to science. Modern chemical nomenclature, for instance, was found to be objectionable by the constant changes which the advances of the science introduced into it. He thought no more important subject could engage the thoughts of the members during the approaching recess. He then adjourned the Society until November.

MEDICAL PRACTICE AMONG THE POOR.

I TRUST that I shall not be thought wanting in humanity, if I suggest in this paper little more than a business view of the relations that subsist between the sick poor and the main body of the doctors. Mr Souchong, who supplies tea to the poor in ounce packets at an enhanced price, and not always in the state in which it left China; Mr Sirloin, who sells them the chips and fragments of his meat at a good profit; Mr Wick, who gets the halfpenny out of the poor man's penny candle, may hold up their hands at the hardheartedness of an apothecary who meanly connects thoughts of the sick poor with thoughts of his own day-book and ledger. Be it so. Many a night, when Mr Souchong was snoring soundly with his cash-box on the chair at his bedside, I and thousands of my brethren, in town cellars and garrets, or in country cottages by lonely hill-sides, have sat sleepless by the bed of a poor man or woman tossing with pain, have had our hands grasped firmly by sufferers who held to us as to dear life; and forgetting our own wearinesses, have laboured to be strong

in help, and strong in sympathy, to cheer the downcast, and to comfort those that mourned. Of course we are hardhearted. Mr Souchong, who happens to be a poor law guardian, and who knows it, says so.

The profession looks to an undecerning public for patronage which is much too unwisely and unequally distributed. It is full of struggling men whose competition with each other would be fierce if it were not restrained by gentlemanly feeling, and a rigid code of etiquette. In such a profession jealousies and morbid sensitiveness must—as they surely do—exist. The folly of the ignorant among the public opens many a profitable path to meanness. Worldly advantages are offered most freely to all medical men who will be humbugs. Some surely must be too weak to withstand temptation of this kind ; and, indeed, it is well known that so far as we could do so with honour we have all sought to satisfy the public by including a very considerable mass of humbug in the routine of our daily practice. We are not to blame for this, any more than we are to blame for the heartburnings that arise among ourselves out of the general impoverished state of the profession ; called upon as it is to give its services gratuitously to three-fourths of the population of the country. We accept cheerfully, I say again, that last-mentioned necessity ; but it is not requisite that we should work, as we so generally do work, unthanked.

Apart from a sense of duty done, the sole profit that a medical man gets out of attendance on the sick poor, is experience. But he gets that out of the sick rich who pay him for his cares ; yet he is content to take it as his only profit from the poor. His need of experience is great. He requires it first in hospitals ; in which poor men, women or children are collected for gratuitous treatment by the foremost men of the profession ; who in that way bear their share of the general burden (no payment being attached to hospital appointments) ; and, at the same time, impart much of their own practical knowledge to their juniors. Then the juniors, when they have received their diplomas, begin practice by the acquisition of some more experience among the poor ; and, with that view, seek small salaried positions as house surgeons in provincial infirmaries, or as assistants—doers of the parish work—in the pay of established surgeons.

These are the young men entitled whippersnappers ; to whom the poor are said, by Messieurs Souchong, Sirloin, and Wick, to be shamefully and neglectfully handed over. Mr Souchong, Sirloin, and their friends refuse on their own parts to take counsel of a whippersnapper ; so do their betters with considerable unanimity. They wait until he has more experience ; that is to say, until he has tried his prentice hand sufficiently among the poor. He would be happy enough to attend viscounts and bankers ; but he is bidden by society to try his hand first among beggars. He does so in all good faith and earnestness ; whereupon cries virtuous society, it is a shame to intrust to whippersnappers poor men's lives! Now, I believe that, as the world of physic goes, the poor are not in this respect much to be pitied ; and that on the whole, they perhaps get more true help in the way of medical attendance then several of the classes next above them ; including nearly all the poorer half of what are called the middle classes. The poor man in any town, if dangerously ill, may go into a hospital, where he not only gets the help of such professional advice as, except himself, only the wealthy can afford to summon to their cases, but he gets the advice carried out for him by a system of skilled watching and tending such as many a duke is unable to secure. In every town, almost every surgeon or physician famous for his skill in treating any given form of disease, sits at some hospital or dispensary at certain hours to prescribe gratuitously for the poor who come to him ; securing himself the opportunity of watching and comparing a great number of cases, and on the other hand, giving to the poor opportunities that very seldom can be compassed by a tradesman's purse. Even the out-door visiting by whippersnappers is rather a gain to the sick poor than not. The young practitioner, fresh from his studies and his hospital training, has not, indeed, independent experience ; but he has the best and latest knowledge fresh

in his head, and the experience of first-rate practice that he has been witnessing, still at his fingers' ends. This is not the case with men in active practice, who cannot keep pace with the growth of scientific knowledge in their own profession. Thus the whippersnapper *may* know more than the old established man; whose very success makes him a man of routine and leaves him no time for study. Perhaps, however, this youth is a fool who has lost time and misused his opportunities. Granted. Perhaps the old man, too, was such a fool in his youth; and, if he was, assuredly he never had it in his power afterwards to conquer the ignorance with which he started. He has learned only to hide it; to find a substitute for knowledge sometimes in assumption—sometimes in gruffness or in some assumed eccentricity of manner. But the young fool who, distrusting himself, is not bound by any delicacies of position to abstain from seeking information of his seniors, is a much safer counsellor to a poor man—or even to a rich man—than the old fool who is pledged to maintain a character for wisdom.—*Household Words.*

THE IMPROPER TREATMENT OF THE MEDICAL PROFESSION BY GOVERNMENT.

"The French say that the medical profession has achieved for itself no adequate honour or reputation in England. In France, during the last half century, there is no council board, no administration, no society, in which the medical profession has not found itself represented; whether at the court of the sovereign, or among the peerage, or in the legislature. Physicians of the Institute take their place naturally among the first of the land. Their views, their discoveries, their cures, their professional ideas and suggestions, must be listened to, cannot be neglected, and may never be treated as intrusion; nor had Napoleon fewer physicians and surgeons for friends, councillors, and dignitaries of state, than he had of any other profession. But in England, all such interests find themselves either unrepresented, or not represented worthily, and the best of her physicians is good only to amass money, or at the highest get a baronetcy. What important or salutary medical influence has made itself felt in the public administration since the wounds of Waterloo were healed, and where, in all these years, except to born lords or baronets, have we had the means of looking for sanitary wisdom or suggestion? For answer we are referred to the whole history of our sanitary and medical administration. Provided only a man be born a baronet or lord, we are ready to accept him for a born scavenger and born physician as well, nor can any amount of science or learning be esteemed paramount in our regard, except the science of addressing and managing constituencies, or the knack of palavering either house."—*Examiner.*

Most assuredly what we require in Great Britain is some national institution that shall take charge of, and give a tone to medical affairs. In France we observe an Academy established for this purpose, the admission to which is a high honour, and which is the recognised body consulted on all occasions by, and giving advice to, the Government. In consequence, all its medical affairs are well arranged—its universities and medical schools, the grades of the profession, its sanitary arrangements, its military and naval concerns. In England, we have only too often dwelt on the disorder everywhere resulting from its numerous licensing boards, the differences in rights and privileges, the inequality of education, and the degraded position it seems to occupy in the public eye, when compared with the other professions of the Law and Church. If a sanitary board is to be formed, it never occurs to our Government that a medical man should be its president, and receive the L.2000 yearly given by the country. But to show rather its contempt of that profession, a baronet is chosen, a well-known supporter of homœopathy and mesmerism! On the field of battle the medical arrangements are controlled by some commissariat officer,

and the consequence is, while the French medical corps have steamers and ambulances at their disposal, all the British appliances which ought to have been at Alma, are left at Varna. In the navy the young assistant-surgeon is thrust down into the cockpit with the midshipmen, and cannot find a place in the ship to study or think. Throughout the country, the poor-law guardians grind down the pittance of the medical officer below what a gentleman gives a gardener for looking after the welfare of his fruit and vegetables. In our courts of justice, quackery is everywhere supported, and the rights of medical men scarcely tolerated. In our hospitals the being a son or nephew of one of the governors is a far better title to election than the most brilliant reputation as a physician or surgeon. Well, indeed, may the French say that, as a profession, medicine enjoys little honour or reputation in this country. This is in no degree owing to the deficiencies of medical men individually, but to the fact that we have no national organization. If the so-called medical reformers would leave off their petty squabblings about the privileges of their own colleges and grades, and look to a medical reform of national, rather than of local importance, better days might yet be in store for us.

THE LATE DR R. J. MACKENZIE.

We have never noticed the death of a professional brother with more heartfelt concern, than we now do that of Dr Richard James Mackenzie, at the early age of 33 years. The circumstance of an hospital surgeon, and lecturer on surgery in the Edinburgh School of Medicine, voluntarily, and at his own expense, joining the army of the East, with the sole view of becoming practically acquainted with military, as he had previously been with civil, surgery, has thrown an interest over his sad fate, which entitles it to be recorded in this Journal. Many other good men have fallen, and are probably destined to fall in their country's cause, and in the discharge of their duty. But his death may be truly estimated as a sacrifice to the cause of medical science, and of medical education. It was induced by high aspirations and noble hopes, and consequently, his memory deserves from us not so much the language of fulsome panegyric, as a truthful record of the aims and objects which have been so lamentably frustrated.

Dr Mackenzie graduated at the University of Edinburgh in 1842, and was one of the Presidents of the Royal Medical Society in that year. He became house surgeon of the Royal Infirmary under Mr Syme, and subsequently extended his professional education by attendance on the Parisian and German hospitals. On his return, in 1844, hs became a Fellow of the Royal College of Surgeons, and on the retirement of Dr Argyle Robertson, Mackenzie succeeded him as a lecturer on surgery in the Extra Academical Medical School. In 1848, he was appointed assistant surgeon to the Royal Infirmary. During the last two years he was the junior acting surgeon of the Institution, and next session he would have been the senior surgeon, and given lectures on clinical as well as on systematic surgery. His reputation for the last five years has been steadily increasing. He was highly esteemed as a lecturer, was a skilful and successful operator, and his communications to this Journal, especially one on excision of the knee-joint, had commenced to attract the attention of the profession to his boldness and independence of thought as a surgeon. In short, his position was that of a rising man in the Edinburgh school of medicine, who gave every promise of one day adding his name to the distinguished roll of surgeons it has produced.

Under these circumstances, when Sir George Ballingall announced last spring, the probability of his resigning the chair of military surgery at no distant period, and Mackenzie shortly afterwards determined to visit the army in the East, it was at once felt that this step was the result of a happy conception. It was evident that Dr Mackenzie's known ability, if associated with that practical experience which can only be acquired by attendance on troops in

the camp, on the march and in the field, would render him eminently qualified for the appointment. From him also might be expected a full account of the surgical incidents of the war, and the powers of observation and description displayed in the short note from him which we published in our last August Number, proves that, in this respect, his friends and the profession would not have been disappointed. We had a conversation with him two days before he left Edinburgh. He was then busied in packing a vast miscellaneous kit, containing books, maps of the country, instruments, quinine, chloroform, and every thing that he thought might be useful on such an expedition to himself or others. He was in perfect health and spirits, in the prime of manhood, accustomed to bodily exercise and fatigue, looked cheerfully forward to his enterprize as the means of usefulness and distinction, and left his native city not only with the best wishes of a large circle of friends, but, we believe, also with the sincere regards of every medical man it contained.

He had been furnished by the Prime Minister with an introduction to Lord Raglan, and from the letter which we published in the August number, p. 191, it will be seen that he made his way to Schumla, and regretted his not having been at Silistria in time to save Captain Butler's life, which he thought he might have done. The remarks it contains on the gun-shot wounds he saw in the Turkish hospitals, on the diseases prevalent at the time, and the treatment pursued, though necessarily hurried, exhibit an observing mind and sound professional knowledge, but as they are known to our readers they need not be repeated.

Omer Pacha offered him the post of surgeon to the Turkish army, but as he found such a rank was not very highly estimated, he preferred wandering about and making independent observations. During this period he experienced great fatigue, and not being able to obtain a servant, he had to groom his two horses and forage for himself. When the British army encamped near Varna, he joined it and was attached by Lord Raglan, unofficially to the 79th regiment. At the suggestion of the colonel, he wore the shell jacket and tartan, but not the kilt, from which the surgeons are exempt. Of the time he thus spent he wrote to a medical friend as follows :—

"I have spent an odd summer of it altogether—very different from what I expected, as you may believe. Had any one told me last April that I should not see a shot fired till the middle of September, of course I never should have left home, but were I to return even now, I cannot say I should regret having come out. I have seen a great deal that is interesting, both professional and otherwise, and have studied pretty well the anatomy, physiology, and pathology of an army in the field. From my having been attached to the Highland Brigade, I have of course seen very little except of the first division ; but the Guards and Highlanders give one a tolerable idea of the line. We had, as you know, a nasty time of it for a while, not so much from the actual mortality, as from the vast amount of sickness. At that pestilential place Aladyn and at Gerochler, being well was very much the exception, and the climate was terribly distressing. The men got unmistakeably dispirited, and even those that were well, growled sadly at being, as they said, brought out there to rot without firing a shot, or even getting a sight of the enemy. Our change down to Galata worked a vast improvement. The cholera at Gerochler was the most virulent specimen of disease I have ever seen. Many were struck down at morning parade and died in three, four, or five hours. I need scarcely say that any treatment in such cases was utterly vain. Taking the whole of the cases, however, our mortality was not great, something about 40 to 45 per cent. You may have observed that the mortality of the 79th was wonderfully less than any of the other five battalions of our division. We were constantly congratulated on our "luck," which I suspect can be explained. As usual, premonitory diarrhœa prevailed to a great extent, and traps without end were laid to catch the fellows who had diarrhœa. We had sentries placed on the *latrines*, non-commissioned officers on the look-out, and Scott and my-

self frequently pounced on fellows whom we had recognised crossing the ground to the rear two or three times of a morning. Every man who went to the rear more than twice a day, was ordered, on pain of an hour's pack drill, to report himself instanter. The fevers we have had have been chiefly remittents, intermittents being very much the exception. I have nothing new to tell you about it. In a healthy climate I believe few of the cases would have gone wrong, but the debilitating effects of the climate told sadly about the eighth or tenth day. Many of the patients died suddenly without any alarming symptom to give warning of mischief. So common as this has been that the period, when at home we should have felt comfortable about a patient, who was taking moderate doses of wine, was that in which here life went out like the snuff of a candle."

This sort of camp duty, however, was not all that Mackenzie went out to see, and the various letters he sent home expressed great annoyance at the delay. At length, on the 6th of September, he was on board the transport ship "Dunbar" in Kavarna Bay. From thence he wrote to Professor Simpson, enclosing Dr Hall's general order ,discouraging the use of chloroform, which concludes with stating, that "the smart of the knife is a powerful stimulant, and it is much better to hear a man bawl lustily, than to see him sink silently into the grave." Of this, he says in another letter, that "there is some truth in the use of chloroform being attended with risk in gunshot wounds. But surely the surgeons of the army have sufficient sense not to give it when a man is in a state of collapse bordering on death. Dr Hall's order is intended to discourage the use of it altogether. No one, I should think, will take any notice of it." To Dr Simpson he says—"If an abundant supply is not forthcoming when it is wanted, as it is shrewdly suspected will be the case, it will be very disgraceful." He then says, "we are all to land, surgeons included, at once, whether we are under fire or not, and we have all our own knapsacks to carry, with three days' salt pork and biscuit on our backs; besides which I have my instruments, bandages, splints, etc., in a haversack across my shoulders. We are to have no tents, and will have to sleep three nights in the open air."

On the 12th September, at 8 P.M., the "Dunbar" dropped anchor eight miles north-west of Sebastopol, and we are much indebted to Dr Douglas Maclagan for the use of a long letter which Mackenzie wrote to him that evening. "The troops," he says, "are in capital condition, in spite of all our sickness, and all that may be said to the contrary, and they are all eager for the attack. I feel the same myself, and am only sadly annoyed at its having been delayed so long. Our sea voyage has done much to revive the men, who a month ago looked very shaky. Cholera, however, has not ceased. The 42d, who are towing us, have lost 9 men, and the 33d, 14 men from cholera since we sailed. We have had two or three cholera cases, which however proved quite manageable." On the following morning he writes:—"You can form no idea what a splendid sight our Armada presents. Several hundred sail in sight. The men-of-war formed up into a long line last night between us and Sebastopol, like a set of big fellows standing between the little boys and the opposing force in a *bicker*. We are all to carry our kits. I weighed mine yesterday, and between knapsack, haversack, rations, etc., I shall have within a few ounces of 50 lbs. on my back!"

Mackenzie landed with the troops, and bivouacked two or three nights in the open air, which, in a subsequent letter, he says, did him no harm whatever. He advanced with the army, and in the battle of Alma, he and his friend, Dr Scott, the surgeon of the 79th, wandered out of their place, and were exposed to the fire of the batteries. On this occasion he displayed great coolness, remarking to his companion as a round shot passed just over their heads, "that was a narrow escape." During, and after the battle, he was very active in assisting the wounded. It is said that, in addition to extracting numerous balls, and dressing a multitude of wounds, he performed 27 capital operations, including two amputations at the hip joint. From what we know of his sur-

gical powers, we can readily understand that on this occasion his coolness, rapidity, precision, and dexterity in operating, must have naturally excited the admiration of all who witnessed it. These qualities exhibited, as they probably were to a great extent in public, and among the soldiers of the 95th and other regiments (the 79th, to which he was attached, not having suffered much), must have attracted more than ordinary attention. For, when next day, the first division of the army were drawn up on the heights of Alma, prepared to resume their march, and he was seen coming up the hill, the men, with one voice, asked permission to give "three cheers for Mackenzie." This was immediately granted; so that on approaching them he was greeted by the huzzas and waving bonnets of his countrymen. This incident, which strongly reminds us of the acclamations with which Ambrose Paré was received by the beleagured soldiers of Metz, is alluded to in the last letter Mackenzie ever wrote, with evident pride and gratification.

We are not yet in possession of the professional details of that memorable day on the Alma, or of what happened to Mackenzie immediately afterwards. But he accompanied the army on its onward march, and was seen by Dr John Wood of the 42d, and by Drs Combe and Dumbreck, on the 24th of September, as they thought, in the enjoyment of perfect health. He had, however, undergone excessive fatigue, besides incautiously partaking of grapes, and on the evening of that day he was seized with cholera. Dr Scott says, that from the first the disease was evidently so virulent, as to preclude the possibility of hope. He himself was fully aware of this,—calmly gave directions concerning his affairs, and sent messages to his friends at home. When he had lost his voice for some time, and was sinking, but was still conscious, the chaplain of the regiment read the prayers for the dying, and at their conclusion his friends who surrounded the bed were surprised at his still possessing the power to utter a loud and fervent, "Amen!" He died in the morning, and was buried, amidst the universal grief of his comrades, at Balbec, on the 26th September. Nor, after what we have said, will any one feel inclined to dispute the truth of a statement, made by the Correspondent of the *Morning Chronicle*, namely, "That his loss was more sincerely lamented than that of any other man who fell at Alma."

We cannot conclude this necessarily hasty notice better than by giving publicity to the minute of his colleagues in the Extra-Academical School, in the propriety and good feeling of which we cordially sympathize.

MINUTE OF THE EXTRA-ACADEMICAL LECTURERS, EDINBURGH, REGARDING DR MACKENZIE.

Surgeon's Hall, Edinburgh, October 21st, 1854.

At a meeting of the extra-academical medical lecturers of Edinburgh, held here to-day, it was unanimously resolved to put on record an expression of the deep regret with which they have learned the untimely death of their late colleague Dr Richard James Mackenzie.

Esteemed by them for his skill and accomplishments as a practical surgeon and teacher of surgery, and endeared to them by his upright character, obliging disposition, high spirit, and frank, open, honourable nature, they looked forward with pride and pleasure to welcoming his return from the scenes of warfare in which he was so nobly and usefully engaged. This expectation has been suddenly and mournfully disappointed. It is a consolation to them to know, that, throughout a protracted and harassing campaign, in which he underwent much toil and hardship, their late colleague won the admiration and gratitude of all his comrades by his courteous demeanour, cheerful courage, conspicuous medical skill, and great kindness to the sick and wounded.

The thought of this—mingled as it is with the feeling, that the noble example which he set his brethren, and the honour which he has reflected on the whole medical school of Edinburgh, cannot receive from him any further exaltation or extension—but increases their sense of the greatness of their loss,

and adds to the sorrow with which they give this imperfect but sincere expression to their grief, at the early death of one so worthy of love and esteem.

(In the name of the lecturers),
JOHN STRUTHERS, *Secretary.*

VARIETIES.

"Now" is the Time for Study.—Begin well and the future course is easy and safe. A man rarely makes up in after life for the loss sustained by a false start in the beginning. Every year and every day has its own work, and he who is toiling at the duties of the past instead of those of the present, is paying off an old debt at the sacrifice of future good. He is a bad economist who mortgages future industry in order that he may enjoy present leisure. He is playing a losing game with life: the odds are heavily against him, and he has no right to hope for success. "Now," should be every wise man's motto. What is all time—past, present, or to come—but an everlasting "Now?" It is experience, action, and hope, concentrated in one focus. A man who does not act "now," forfeits the benefits of the past, and defeats his hopes of the future. It is present action alone that can realise either experience or desire. "Now" is man's only possession; it is his sole capital for the exercise of his talent and industry: it is at the same time a great living fact, and the solution of a wondrous spiritual mystery; it is the marriage-knot that binds together the actual and the speculative, and is the highest exponent of universal wisdom. —*Ross's Address to Students*, p. 6.

Origin of St Thomas Hospital, Southwark.—Seven hundred years ago, St Mary Ovarie, with the money which she inherited from her father, the Charon of the Thames, laid the foundation of an eleemosynary establishment, from which has sprung the present valetudinarium of St Thomas; but in the fifteenth century it was seized by Henry the Eighth as church property. After his death the citizens of London, with the *sanction* of Edward the Sixth, formed a board of inquiry as to the best mode of relieving the misery of their more unfortunate brethren. The result was, that they purchased of Edward the Sixth the Manor of Southwark, including the sight of this hospital for L.647, 2s. 1d. The hospital having been unoccupied for a short time, was fast falling into decay, so the worthy citizens in 1551, at an expense of 1000 shillings, repaired and fitted it for the reception of 300 patients. In 1552 they appointed a chaplain, with an annual fee of L.10; clerk, L.10; steward, L.6, 13s.; butler, L.5; cook, L.3; physicians and chirurgeons, each L.15. You see, therefore, by the relative amount of salaries, and the difference in the value of money, that the founders of the hospital considered their medical officers entitled to renumeration.—*Lancet.*

Cholera in the Middlesex Hospital.—It is difficult, without appearing to exaggerate, to convey any adequate idea of the state of the wards during the four first days of September, or of the feelings of admiration with which the House Committee and Medical Officers viewed the noble conduct of all those resident in the establishment. While daylight lasted, the sunshine, though it revealed every minute detail, relieved, by its cheerfulness, some of the horrors of the sad and harrowing sight. But, as night closed in, the dim light shed by a solitary burner, and by the pale moonbeams that struggled through the windows, lent a still more ghastly hue to the livid features, the skinny hands, and the deeply-sunk eyes—in general nearly closed, as if in death, but sometimes bloodshot and glaring—of the poor patients, who were screaming in agony or groaning in mortal weakness, on every hand. Add to all this the sobs and shrieks of new-made orphans and widows, and the clank of the shell, as in its ceaseless round it "vexed the drowsy ear of night"—and you have a very feeble representation of a scene before which many a stout heart might have quailed, and by a single glance at which not a few who presented themselves

to be engaged as assistant-nurses, were scared away, without so much as entering the wards. Yet hour after hour, and night succeeding day, did all the members of the Hospital staff—Apothecaries and House-Surgeons, Matron and House-Steward, the few pupils who were in town during the College vacation, sisters, nurses, and porters—discharge, without for a moment shrinking from, tasks the most laborious and the most revolting.—*Dr Stewart, in Medical Times.*

No Disease Absolutely Incurable.—There are many diseases which no physician yet has been able to cure; but it does not follow from this that all these diseases are absolutely incurable. There are so very few that can be pronounced, in their own nature, incurable, that I would wish you to annex no other idea to the phrase, incurable disease, but the idea of a disease which you do not know how to cure. How many patients have been dismissed from different hospitals, as incurables, who yet have recovered perfect health, sometimes by the efforts of unassisted nature, sometimes by very simple and safe remedies, and sometimes by the random and desperate prescriptions of ignorant and impudent quacks? To pronounce disease incurable, is to establish indolence and carelessness, as it were by a law, and to screen ignorance from reproach.—*Gregory on the Duties and Offices of a Physician*, 1770.

Union of King's and Marischal College, Aberdeen.—The project so long talked of, of uniting the two Aberdeen Colleges into one University is, we understand, very likely to be accomplished. King's College is to give instructions in the Faculty of Arts, whilst Marischal College will be appropriated to the Faculties of Theology, Law, and Medicine. We understand that some of the townspeople are still opposed to the plan, but we heartily wish it every success. Aberdeen possesses every requisite for the establishment and support of a great medical seminary, and by concentrating talents now diffused among two schools, the reputation of the University must necessarily be greatly increased.

The Edinburgh Royal Infirmary.—The new surgical hospital is now fully occupied, and the surrounding ground nearly completed. Although every thing is well arranged, we must confess to having experienced a tinge of regret on seeing the last portion of the old city wall, on the south side of the town, levelled with the ground. This portion of the Infirmary now rivals in commodiousness and extent any of the large hospitals in Europe. It contains 250 surgical beds alone, whilst the whole of the buildings at the disposal of the managers, including medical, surgical, obstetrical, cholera, fever, and lock departments contain at least 700 beds.

The Resident Clerks of the Edinburgh Infirmary.—We do not think that it is as yet sufficiently known among students, that the resident and clinical clerkships, which formerly used to be paid for, are now given gratuitously to the most meritorious young men that can be found. This boon was granted by the managers at a considerable loss to the Institution, on the express requisition of the physicians and surgeons, who annually recommend the most deserving students for election.

Lecturing and Teaching.—Lecturing after a fashion is easy enough; teaching is a very different affair. The one requires little more than good information, some confidence, and a *copia verborum;* the other establishes several additional requisites. In teaching, the object is not merely to convey the idea, but to give a lively and lasting impression; something that should not merely cause the retention of the image, but in such connection as to excite another process, "thought."—*Macilwain's Life of Abernethy.*

The Times' Envoy to the Hospitals of the East.—One of the correspondents of the *Times* newspaper, found fault with the medical arrangements of the hospitals for the reception of our wounded soldiers in the East. It is a

remarkable fact, and evinces how earnestly the people of this country, sympathize with the wounded, that upwards of L.7000 were subscribed in one week, to alleviate their condition. The functions of Government and of the medical staff are, in consequence, to be superseded, amended, commented on, or interfered with, by a gentleman belonging to the *Times* office, sent out to apply this fund. It may be well said, that the Press in this country is all-powerful.

Nurses for the Army.—At the commencement of the war, a number of old Chelsea Pensioners were sent out as an ambulance corps to attend on the sick. But they have been found in practice, rather to require assistance themselves than to render any to the sick and wounded. These are now to be replaced by trained female nurses under the direction of Miss Nightingale of London, who we hope will emulate the noble example always displayed on these occasions by the French Sisters of Charity.

The True Preventive of Cholera.—"I firmly believe, that if the blood of a person of sound constitution be kept in a state of perfect purity by the moderate, but not excessive use of wholesome food and drink; by free respiration of pure air; by adequate exercise, not pushed to the extent of over-fatigue; and by personal cleanliness, he is as completely protected against the invasion of cholera as he who has been effectually and recently vaccinated is proof against small-pox."—*Dr Carpenter's Address, Medical Times.*

Medical Appointments in Paris.—M. Jobert has been elected to the chair of Clinical Surgery of the Faculty of Medicine of Paris, vacant by the death of M. Roux. There were nine candidates for the office. M. Claude Bernard has been elected a member of the *Academie des Sciences*, in room of the late M. Roux.—*Archiv. Gén. de Méd.*

PUBLICATIONS RECEIVED.

Principles of Comparative Physiology. By William B. Carpenter, M.D., F.R.S., etc. With three hundred wood engravings. Fourth edition. London. 1854. 8vo. Pp. 770.

Transactions of the Pathological Society of London. Volume V. London. 1845. 8vo. Pp. 371.

Lectures on the Physical Diagnosis of the Diseases of the Lungs and Heart. By Herbert Davies, M.D., etc. Second Edition. London. 1854. Small 8vo. Pp. 364.

The Book of Prescriptions, containing 2900 Prescriptions, collected from the Practice of the most eminent Physicians and Surgeons, English and Foreign. By Henry Beasley. London. 1854. 12mo. Pp. 543.

On the Relative Merit of the two Operations for Stone. Two Lectures. By F. C. Skey, F.R.S., etc. London. 1854. 8vo. Pp. 55.

Clinical and Critical Contributions to Obstetric Science and Practice. 1. On Uterine Polypus, its Nature, early Detection and Treatment. By Robert Barnes, M.D., etc. London. 1854. Small 8vo. Pp. 44.

A Memoir on Strangulated Hernia: from Cases occurring in the London Hospital. By Nathaniel Ward, F.R.C.S., etc. London. 1854. 8vo. Pp. 33.

A Discourse on Medical Botany. By Earl Stanhope. London. 1854. 8vo. Pp. 47.

Self-development: an Address to Students. By George Ross. Reprint London. 1854. 8vo. Pp. 20.

Thirty-fourth Annual Report of the Directors of the Dundee Royal Asylum for Lunatics, with the Report of the Medical Officers. Dundee. 1854. 8vo. Pp. 39.

A few Practical Observations on the Injuries Incidental to Warfare. The Substance of three Lectures. By G. Borlase Childs, F.R.C.S., etc. London. 1854. 12mo. Pp. 47.

The Identity of Sporadic and Epidemic Cholera, etc. By James J. Robertson, Esq. Edinburgh. 1854. Small 8vo. Pp. 24.

Cholera: its Treatment and Prevention. By Henry M'Cormack, M.D., etc. Belfast. 1854. 12mo. Pp. 8.

Percussor Stethoscope. By B. Miles Richardson, F.R.C.S.L. Reprint. 8vo. Pp. 2.

Forhandlingar vid svenskaläkare-sällskapets Sammankomster från och med Octob. 1852 till och med Dec. 1853. Stockholm. 1854. 8vo. Pp. 270.

Svenska läkare-sallskapets nya handlingar. Attonde bandet. Stockholm. 1854. 8vo. Pp. 417.

We have now received the April, May, June, August, and September Nos. of the *Archives de Médicine*. No others have come to hand.

We have also received the first seven Nos. of the *American Monthly*.

MEDICAL

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Notes of Hospital Practice.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh.

ABSENCE OF CHLORIDES FROM THE URINE, DIAGNOSTIC OF THE ONWARD PROGRESS OF PNEUMONIA. CRITICAL DIARRHŒA IN THAT DISEASE.

IN the last April No. of the Journal I gave an account of certain facts which confirmed the statements of Simon, Redtenbacher, Beale, and others, as to the absence of chlorides in the urine during the onward progress of pneumonia. On returning to the clinical ward in summer, these inquiries were resumed. Three cases of the disease came in, all of which confirmed the diagnostic value of this change in the urine.

CASE I.—*Single Pneumonia; Absence of Chlorides in the Urine; Critical Diarrhœa on the* 21*st day; Recovery.*

James Murray, æt. 53, a hawker, admitted June 30th. Has been much subject to coughs and colds, from exposure to the weather, when following his employment. On the 24th inst., at noonday, he was seized with rigors, sharp cutting pains in the right side, anorexia, thirst, and headache. These symptoms confined him to bed, and became so severe that he says he has been occasionally delirious. The treatment has consisted only of a blister, which was applied to the right side of the chest. On admission percussion over the chest anteriorly is normal, but posteriorly, there is marked dulness over the lower two-thirds uniformly on right side, where there is much pain on coughing, and on taking a deep inspiration. On auscultation over the dull portion, crepitation is audible, especially at the base, with tubular breathing above and bronchophony. No expectoration. Pulse 112, of good strength. Tongue furred, great thirst, loss of appetite complete. Severe headache, urine of a reddish colour, and contains no chlorides. Other organs healthy. *To have* ℥iss. *tr. hyosciam. at bedtime, and* ½ *grain antim. tart. and* ♏ *x. of sol. mur. morph. every second hour. July* 3*d.*—To-day the chlorides have appeared in the urine in small quantity—pain in side diminished. Pulse 110, soft. Physical signs the same. *To have* ℥ij. *of wine—discontinue tart. ant. R. potassæ acet.* ℈iv.; *sp. æther nit.* ℥ij.; *aquæ* ℥iv. *M* ℥j. *to be taken three times a day. July* 4*th.*—Urine contains abundance of chlorides. *July* 9*th.*—Still dulness, crepitation, and bronchophony over right chest. No expectoration. *July* 13*th.*—Blister to be applied to the right side. *July* 15*th*—This morning he

was seized with vomiting, followed by purging. He had 7 or 8 stools. To-day he is much better. The right chest unfortunately could not be examined, on account of the soreness of the blistered surface. But from this time he rapidly improved. On the 24th still slight crepitation. Has never had expectoration. On the 26th the crepitation still continuing, I ordered him to lie on the abdomen. On the 30th crepitation absent. *August 4th.*—Dismissed cured.

This case presented a rare negative symptom in pneumonia, viz., the complete absence of expectoration, all the other symptoms and signs being present. In this and the two subsequent cases I looked for the usual critical appearance of lithates in the urine about the fourteenth day, but in vain. It is somewhat remarkable that all three cases, instead of presenting the usual signs of crisis by urine, were seized with sudden diarrhœa, of a severe character, but of short duration, which ushered in convalescence. Whether this was attributable to any choleraic disposition, or peculiar state of the atmosphere, must be unknown; but I have never previously seen diarrhœa so uniformly critical in cases of pneumonia. The crepitation continued to linger in the minute bronchial tubes long after the pneumonia had disappeared. This I attribute in such cases to congestion from decubitus, and find it easily removable by causing the patient to lie as much as possible on the abdomen.

CASE II.—*Single Pneumonia; Absence of Chlorides in the Urine; Critical Diarrhœa on the 14th day; Recovery.*

Thomas Shepherd, æt. 23, a brassfounder, admitted July 11. He says that he has always been a strong and healthy man, until the 3d inst., when, on rising in the morning, he was seized with rigors, dull pain in the left side and loins, thirst, anorexia, headache, and cough. On the previous evening he had walked far, perspired much, and gone to bed exposed to a draught from his window. On the 5th a medical man administered purgatives and other medicines. On the 7th dyspnœa became urgent, and the feverish symptoms were augmented. He was then bled to the extent of 18 or 20 oz., which occasioned much relief. On admission, the left side of chest does not expand equally with the right. Anteriorly the left lung is resonant, but posteriorly dull on percussion over its two lower thirds. On auscultation over the dull part loud tubular breathing, with crepitation and bronchophony are heard. Respirations, 24 in the minute. Sputa scanty, of gelatinous consistence, tinged of a rusty colour. Pulse 84, soft, but of good strength. Tongue red and dry; appetite diminished; urine contains no chlorides. Other functions healthy. *To take ¼ gr. of antim. tart. and 5 ♏ of sol. mur. morph. every second hour. July 13th.*—To-day the tubular breathing is gone; crepitations abundant and coarser. On adding a drop of nitric acid to the urine, a faint white haze is visible. *July 14th.*—Urine to-day exhibits abundant chlorides on the addition of nitric acid. Pulmonary signs the same. *July 16th.*—To-day was seized with pains in the abdomen, and diarrhœa. *July 20th.*—Diarrhœa has continued until to-day, but is now checked. Pulmonary crepitation ceased; no expectoration, only slight bronchophony and dulness. From this time he rapidly recovered, and was dismissed well, July 31.

CASE III.—*Double Pneumonia; Absence of Chlorides in the Urine; Critical Diarrhœa on the 21st day; Recovery.*

James M'Naughton, æt. 34, admitted June 30; a shoemaker. States that he has been much addicted to the use of intoxicating liquors. From the 21st to the 23d inst. he was in a continuous state of intoxication, and on the morn-

ing of the 24th he awoke with dull pain in the chest, great dyspnœa, cough, and expectoration of matter which, he says, resembled pure blood. He has undergone no medical treatment. On admission, the respirations are 44 in the minute. On percussing the chest anteriorly there is slight dulness on the right side inferiorly, but posteriorly the dulness is very marked over the inferior 3-4ths of both lungs. On auscultation dry tubular breathing is heard over the dull parts, with bronchophony, but on taking a forced inspiration coarse crepitation, deep-seated, is audible, respiration at both apices and over chest anteriorly puerile. Pulse 120, weak. Tongue covered with a yellowish fur, thirst, no appetite, headache; general appearance sallow—indicative of exhaustion, and complains of great weakness. Urine of deep cherry red colour, sp. gr. 1020, contains no albumen or sediment, and no chlorides. Other functions normal. *To have* $\frac{1}{3}$ *of a grain of antim. tart. every three hours—*$\bar{3}$*iv. of wine daily. July* 3*d.*—The wine was increased to $\bar{3}$vj. daily, his symptoms having undergone no change. *July* 4*th.*—To-day chlorides have appeared in small quantity in the urine, which presents the same cherry-red colour. Crepitation audible in left lung posteriorly, right lung as before. Sputum lighter, with less of the prune juice appearance. *July* 7*th.*—Since last report there has been marked improvement. To-day the urine contains abundant chlorides. Crepitation over both sides of chest posteriorly. *To have* 40 ♏ *of sp. æther. nit. and* 20 *gr. of potas. acet. in solution thrice daily. July* 10*th.*—Over the whole of back posteriorly coarse crepitation — still bronchophony, and abundant sputum, tinged with blood. *July* 15*th.*—Last night was seized with diarrhœa. He had six copious watery stools. To-day no crepitation audible; respirations natural, except in left supra-scapular region, where bronchophony is still audible, but not so harsh as formerly. Urine now clear and in every way normal. From this day he rapidly improved, and was dismissed, August 2d, quite well.

This last was a very severe case of double pneumonia, in a broken-down and dissipated individual, who was saved by wine, and in whom a choleraic diarrhœa, accompanied by vomiting, proved critical on the 21st day. In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, whilst their presence indicated its cessation, and was generally accompanied by the returning crepitation and commencing absorption of the exudation. It still remains to be determined whether the absence of these salts is a cause or a result of exudation into the lungs—whether the interference to the respiratory function, by diminishing the amount of oxygen absorbed, gives rise to those chemical changes in the blood which react on the urinary secretion. If so, what are the nature of these changes? Indeed, a crowd of questions will be suggested to the mind of the physiologist, from the establishment of the remarkable clinical fact of which we are now speaking. That such is an important diagnostic sign I have now no doubt, and it was singularly well tested in the following case, in which there were many signs and symptoms of pneumonia, complicated with heart disease. The question on admission was whether with heart disease and bronchitis pneumonia might not be conjoined, and I was assisted in answering in the negative by the abundance of chlorides which the urine contained.

CASE IV.—*Bronchitis and Pulmonary Congestion, from Morbus Cordis, resembling Pneumonia, but no absence of Chlorides in the Urine.*

John Dickson, æt. 44, pensioner, admitted July 21st, says that on the even-

ing of the 19th he was seized with chilliness, followed by sweating, heat of skin, thirst, impaired appetite, and expectoration of a frothy fluid, resembling liquorice juice. He has for some time felt an uneasy feeling in the epigastrium, which, since his recent illness, has amounted to pain. Yesterday he experienced great dyspnœa and anxiety, symptoms which have continued until now. On admission there is excessive dyspnœa, with expectoration of a tenacious sputum, of a reddish-brown colour. On percussion there is no comparative dulness, but posteriorly the resonance is impaired on both sides. On auscultation anteriorly the expectoration is everywhere much prolonged, and posteriorly there is considerable crepitation with bronchophony. Pulse 92, of good strength. The heart's sounds are entirely masked by the prolonged wheezing expiration and agitation of the chest. He cannot lie on his back or left side, is easily agitated, and frequently experiences palpitations, and cannot sleep. Abundant chlorides in the urine. Other functions normal. *R. sp. æther sulph. ℥ss. aq. cassiæ ℥iv. One tablespoonful to be taken in water occasionally—to have* $\frac{1}{4}$ *grain of ant. tart. in solution every two hours. July 25th.*—Since last report the dyspnœa has diminished, and the crepitation posteriorly continue, but the wheezing anteriorly is less. Still gelatinous sputum, specked with rusty-coloured blood. The apex of the heart cannot be felt, but a double blowing murmur is now recognizable, accompanying both the first and second sounds—the systolic loudest at the apex, and the diastolic loudest at the base. *Omit the antimony. July 31st.*—The pulmonary symptoms and signs have now greatly subsided, whilst the cardiac lesion has become more distinct. For this latter he remained in the house until the commencement of November, when he was dismissed greatly relieved.

ANEURISM OF THE ARTERIA INNOMINATA.

History.—Catherine Syme, æt. 56, a seamstress, admitted May 2, 1853. She says that her habits have always been temperate and regular. Fifteen years ago she had an attack of acute articular rheumatism, which afterwards became chronic, and rendered her incapable of working for eighteen months. For six years past she has been subject to occasional attacks of giddiness and swimming in the head, accompanied by a loud noise like the clanging of machinery. Fourteen months ago, in the night, she was seized with a fit of intense dyspnœa, threatening suffocation, and accompanied with a loud crowing noise on inspiration. The attack lasted about eight minutes. Three months afterwards, she experienced a somewhat similar but milder attack, also in the night, during sleep. She now observed that her voice was becoming rough and hoarse; and a few months later, she felt slight difficulty in swallowing, at a point corresponding to the upper border of the sternum. In the early part of January 1853, after unusual exposure to cold, the dyspnœa returned every morning, gradually became urgent, and generally terminated in the expectoration of a small quantity of mucus. There were also palpitations, and she became subject to sudden startings from sleep. A week ago, all these symptoms became so much aggravated, that she was unable to leave her bed.

Cardiac and aneurismal signs.—The cardiac dulness measures two inches across. The apex beats between the fifth and sixth ribs, a little to the inside of the nipple. Heart's impulse is somewhat diffused, rhythmical, and of good strength. A blowing murmur accompanies both cardiac sounds, that with the first sound being loudest at the apex, and that with the second being loudest at the base. Immediately above and towards the outer side of the right sterno-clavicular articulation, a pulsating tumour, about the size of a hen's egg, is visible to the eye. It is felt lying beneath the sternal and inner portion of the clavicular origins of the sterno-mastoid muscle, presents a distinctly rounded outline, and anteriorly slightly overlaps the trachea immediately above the upper border of the sternum. The impulse is strong and diffused, and a loud, clear, abrupt murmur is heard over it, synchronous with the second sound of the heart.

Concomitant symptoms.—The pulse is regular, 106, middling and of good strength, equal at both wrists. There is a paroxysmal cough, harsh, prolonged, and of a clanging metallic character, always worst in the morning, when it is accompanied by urgent dyspnœa, and a loud crowing inspiration. Sputum scanty and gelatinous, containing a few flocculi of pus intermingled. Voice hoarse and weak. Chest everywhere resonant on percussion, but not unusually arched. Respiratory murmurs very faint, but normal in character. Expiration much prolonged. Appetite impaired. When swallowing solid food, she says, the bolus seems to meet some obstruction at a point corresponding with the upper border of the manubrium of the sternum. The countenance is anxious, face livid, and the superficial veins of the chest and lower part of the neck are very large and turgid. Sleep restless and easily disturbed. Other functions normal.

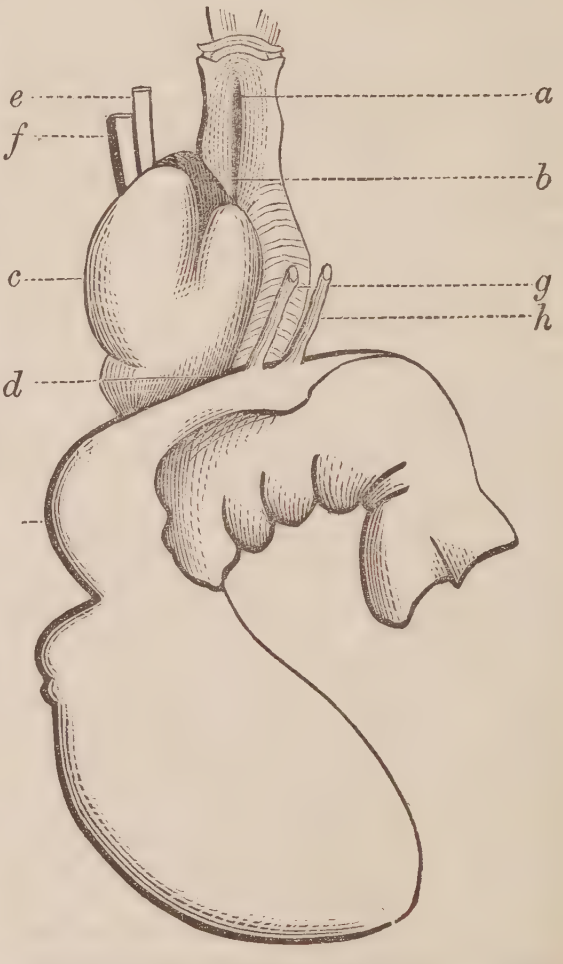
Progress of the case.—The symptoms previously noticed, continued with occasional remissions, until the *20th of June*. On that day, it was noticed that the blowing murmur synchronous with the second sound at the base of the heart, was much less distinct, and that the murmur with the first sound at the apex was replaced by one with the second. The veins over the upper part of the chest, have been gradually enlarging. On the *6th of July*, a careful examination elicited the following results. Pulse 88, soft, equal in both wrists. The impulse is very strong over the tumour, and on auscultation, there are now heard two sounds, the second being loud, abrupt, and exceedingly clear—no blowing audible. These sounds diminish gradually in intensity as the stethoscope approaches the left edge of the manubrium of the sternum, where the two cardiac sounds are heard quite normal. As the instrument descends towards the heart's apex, the second sound gradually assumes a soft blowing character, which, *at* the apex is loud and distinct. The first sound is quite normal. Posteriorly above the right scapula, the sounds of the tumour are heard at a distance, but disappear towards the centre of the back, and are inaudible along the vertebral column. *July 8th.*—For the last few days the dyspnœa in the morning has been very urgent, and the cough coarser and of a metallic clanging sound. To day the paroxysm continued 15 minutes, and even now at the visit, the breathing is noisy, laboured and hurried, the dyspnœa urgent, and the paroxysm of cough severe and at short intervals. She cannot expectorate easily. The voice is feeble, and the countenance expressive of great anxiety. To relieve these symptoms tracheotomy was attempted by Mr Syme, but having made two incisions, and cut through the integument, and subcutaneous fat, such an amount of venous hemorrhage occurred that he desisted, applied a ligature to the large veins, and declined to perform laryngotomy. *July 9th.*—The loss of blood caused considerable relief, and she passed a tolerable night. A double blowing murmur is now audible both at the apex and base of the heart. That accompanying the second sound is loudest over the apex, while the one accompanying the first is heard loudest over the ensiform cartilage. *July 12th.*—Last night, about half-past eleven P.M., a severe paroxysm of dyspnœa, threatening suffocation, came on. The house-surgeon, Dr Dobie, enlarged the incision, made by Mr Syme, upwards, and inserted a common sized tube into the trachea and larynx, after dividing the cricoid cartilage. To-day she is again better, the operation having been followed with immediate relief. She still breathes, however, with difficulty through the tube. The countenance is livid and anxious, extremities cold, pulse feeble and fluttering, surface bedewed with a clammy sweat. She now gradually sunk, and died at half-past eleven P.M., on the 14th. The embarrassment of the respiration being apparently increased by the difficulty of expectoration. Immediately before expiring, she ejected through the tube about ℥j. of dark grey coloured fœtid pus, of the consistence of thick cream.

Treatment.—The treatment throughout the progress of the case was directed to alleviating the cough and expectoration, by means of anodynes and expectorants, and diminishing the paroxysm of dyspnœa by means of diffusible stimuli. Cupping over the sternum, and the occasional application of leeches

were employed, and for some time these remedies undoubtedly caused great relief. The surgeons of the Infirmary were unanimously of opinion that the aneurism did not admit of relief from any operation. Latterly the propriety of tracheotomy or laryngotomy was discussed as a paliative, and ultimately tried with the effect already described.

Sectio Cadaveris, July 15.—The edges of the wound through which the larynx had been opened were thickened, the surrounding muscles discoloured and infiltrated with pus. The posterior half of right lung was partially covered with recent lymph, not adherent, and the pleural sac contained two or three ounces of sero-purulent fluid. The lower lobe of the right lung was hepatized; and on cutting into it, several abscesses up to the size of a cherry were found. The left lung was œdematous, and its bronchi were filled with muco-purulent matter—otherwise healthy. The liver presented the incipient waxy appearance. The spleen was dark in colour and pulpy, almost diffluent in consistence. The kidneys were crowded with minute cysts, and the cortical substance considerably atrophied.

The heart, aorta, and parts connected with the aneurism, were removed *en masse*, and carefully dissected, with the following results:—the heart and its valves quite healthy with the exception of slight thickening of the margin of one aortic valve. The arch of the aorta immediately above the valves considerably dilated, and the whole of its internal surface thickly studded with atheromatous and calcareous plates. The whole arteria innominata dilated into an aneurismal swelling of a round and somewhat flattened form, having a diameter of fully three inches. The trachea is pushed by it towards the left side, as represented in the accompanying figure, in consequence of which the incision that was made in the median line during the operation, was within one-eighth of an inch of the aneurism. The tumour, by pressing on the right side of the trachea, caused much bulging into and diminution of its calibre. The left innominatal vein was nearly obliterated. The remains of its interior contained a softened clot resembling pus, which communicated by a small opening through the aneurismal sac, with a portion of the laminated clot, which occupied about three-fourths of its internal cavity. The opening into the sac from the aorta was about the size of half-a-crown, and presented a sharp circular margin. Posteriorly the nerves were reddened, and for the most part enlarged, and firmly united to the posterior wall of the tumour. The superior laryngeal nerve was healthy, being above the tumour; but the inferior was compressed and embedded in thickened cellular tissue.



Sketch of the aneurism and adjoining parts; *a*, opening into the larynx; *b*, line of original incision which inferiorly came close upon the tumour; *c*, aneurismal tumour; *d*, point where the obstructed vena innominata had ulcerated into the tumour; *e*, right carotid; *f*, right jugular; *g*, left carotid; *h*, left vertebral.

This case terminated in the usual way, by pressure on the nerves

of respiration, causing dyspnœa, and at length partial latent pneumonia. Two considerations suggest themselves with regard to it, one of a theoretical and the other of a practical character.

1. The double clear sound over the aneurismal tumour I have been in the habit of attributing to the flux and reflux of the blood over a sharp vibrating opening into the tumour. In few aneurisms were these sounds more decidedly present than in the case under consideration, and few after death presented an opening having the margin in question better formed. The sounds heard over the heart, however, changed their character as the disease advanced. At first, double valvular disease was suspected, but latterly, when the murmurs became reversed, and it was most carefully determined, by repeated examinations, that the murmur at the apex was with the second, and that at the base was with the first, sound, they were attributed to propagation downwards from the aneurismal tumour. I am by no means satisfied, however, that this theory is correct with regard to cardiac murmurs, so distinct as those in the present case, associated with aneurismal tumours and a healthy heart. I content myself, therefore, for the present, with placing the facts on record, as their accuracy is undoubted, and they were confirmed not only by my own repeated examinations, but by those of the clinical class and of the clerks, all of whom took great interest in the case.

2. The question of putting a ligature on the vessel having been decided in the negative, although in such a case, Brasdor's operation seems to be applicable; the only other question of treatment was the prolongation of life. The source of danger was evidently the dyspnœa, and the frequent attacks of spasmodic laryngeal obstruction, so common in aneurismal cases from pressure of the tumour on the recurrent nerve. The trachea was also considerably pressed upon and pushed aside, but this could not have accounted for the paroxysms of suffocative dyspnœa, for although diminished in calibre, it was still largely open for the admission of air. On the other hand, the recurrent nerve was found after death thickened, and embedded in dense cellular tissue immediately behind the tumour. Under such circumstances it has been proposed by passing a tube into the larynx or trachea, to avert the effects of these spasms. In the present case, tracheotomy could not be performed, and whenever the deep-seated venous obstruction is of such a character as to cause enlargement of the superficial veins, laryngotomy is the operation that should be attempted. This at length was accomplished with momentary relief, but I have no hesitation in saying that the difficulty of expectoration, and the consequent clogging of the air tubes, led to results equally distressing and fatal as the spasmodic attacks. It has, indeed, been said, that in these cases the operation is generally delayed too long, and that by waiting until there is much secretion of mucus and diminution of strength no very good effects can be reasonably expected. But in cases of aneurism, it is at best only to be considered as a palliative, and considering how very difficult expectoration must always

be under such circumstances, I consider it very doubtful whether it is ever justifiable except as a *dernier resort*. Certainly the case now recorded is any thing but favourable to the practice.

POLYDIPSIA.

Margaret Shearer, a French polisher, æt. 34, a mother, admitted May 31. States that a year and a half ago she went to work at six o'clock, A.M., in her usual state of good health, and at eight o'clock, two hours afterwards, was suddenly seized with great thirst, which has continued ever since, accompanied by excessive discharge of urine. About three months afterwards, she was obliged to give up work on account of a pain in the loins. At various times she has experienced loss of appetite, nausea, fulness of the abdomen, palpitations, constipation or diarrhœa. Thinking that her strength has diminished of late, she entered the Infirmary. On admission, the amount of urine passed in twenty-four hours was 424 ounces—pale in colour—of sp. gr. 1005, not coagulable by heat or nitric acid, and containing no sugar, as determined by Trommer's test. She is a stout able-bodied woman, and speaks of occasional slight complaints. She has a pale countenance, furred tongue, and dry skin; but in every other respect is quite healthy. Dr Alison, who first treated her, ordered warm baths and astringents, and afterwards galvanic shocks to be passed through the epigastric region. On taking charge of the case in the middle of June, I ordered bitter tonics, and the diet was carefully arranged, and the amount of water drank limited, and mixed with milk and a little magnesia. No change, however, occurred, and she confessed that she could not admit of restraint with regard to the amount of drink. During the whole month of July, she was weighed daily, and the amount of water drank and emitted from the kidneys carefully measured. Her average weight was eight stone, which underwent little variation. The amount of water drank varied from 370 to 520 ounces, the average being 440 ounces. The amount passed varied from 350 to 500 ounces; and it was observable that it was always from 20 to 50 ounces less than the quantity drank. The sp. gr. varied from 1001 to 1005, and was frequently tested for sugar with the uniform result of its never being present. The bowels were generally open every other day, and the stool was of normal consistence and healthy appearance.

From the 9th to the 14th of July, I tried the influence of narcotics, and she took three grains of opium daily, with ʒiss. and then ʒij of solution of morphia. Under this treatment she frequently appeared drowsy and stupid, but sound sleep was never prolonged, and no diminution of the thirst and diuresis was perceptible. She then took large doses of gallic acid, and subsequently, at her own request, cod liver oil, under the use of which she became stouter, stronger, and the appetite improved. August 22d.—All other treatment was suspended, and she was ordered to take ten minims of the liq. iodini. comp., which was continued to the 14th of September without any effect. On the 16th she was ordered *R. mass. pil. aloet et myrrhæ ℈j; ferri. sulph. ℈ij; ext. hyosciam, ℈j. ft. massa in pil. xij. dividenda. Two pills to be taken twice daily.* On the 26th there was diarrhœa, when the pills were discontinued, and an astringent mixture ordered. The report on the 1st of October is, general health good, and from an observation made for the first seven days of this month, it appeared that the thirst and diuresis had somewhat diminished, the amount of urine varying from 280 to 350 ounces. There was no further change up to October 10, when she left the house.

I prefer calling this case polydipsia to diabetes insipidus, as frequent careful inquiry established the fact that it commenced with thirst, and that the increased flow of urine was a simple result of the quantity of water drank. In the present state of science, no reason-

able theory can be conceived, explanatory of the fact, that a woman, apparently in good health, is suddenly seized with great thirst, and thereupon drinks two or three gallons of water daily, passes a corresponding quantity of urine, and that this continues for nearly two years without any marked change in her health. Where there is no scientific indication, the treatment is wholly empirical, and even the results of experience are wholly negative and useless. Astringents, diaphoretics, galvanic shocks, narcotism by means of opium, cod liver oil, iodine, and purgatives all failed. The latter, by increasing the alvine discharges, diminished somewhat the excretion of urine, but we could not flatter ourselves that she was in any way benefited by her four months' treatment in the Infirmary.

SUPERIORITY OF THE HAMMER AND PLEXIMETER OVER THE FINGERS IN PERCUSSION DEMONSTRATED.

Nothing has surprised me so much as that clinical teachers in their works should repudiate the hammer and pleximeter, for nothing is more easily demonstrated than the fact, that by means of them the results of percussion are made audible to large classes, instead of being confined to the few around the bed. Hitherto it has been urged, with great plausibility, that as skill with the fingers, though more difficult, will ultimately enable the practitioner to arrive at the same results, the sooner the student acquires the art the better. But my own experience in teaching has convinced me, that such are the difficulites to surmount before clear tones and exact percussion can be practised by means of the fingers, that in nine cases out of ten, they are not mastered by the student at all. Besides, the observation so readily made, that a finger at least half an inch thick, composed of bone, muscles, and skin, constitutes as good a pleximeter as a piece of ivory one-tenth of an inch thick, is so manifestly contrary to physical laws, as to be absurd. Still I am far from denying that those who, by means of constant practice, can produce clear tones with their fingers can, in the majority of cases, arrive at useful results in this way. I profess to be capable of using my fingers as well as most persons, although percussion is so exact and so easily performed by means of the hammer and pleximeter, that for the last twelve years I have seldom used my fingers. It so happened, however, that a man called Macleod, æt. 56, entered the Infirmary last October, and on proceeding to examine his chest, no hammer could be found. Percussion with the fingers produced marked dulness, evidently superficial, with a tympanitic sound below, under the right clavicle, auscultation determined tubular breathing with great increase of the vocal resonance. The man had been in the house five years previously with a violent inflammation, as he said, of the chest, from which he had perfectly recovered, latterly asthma had come on, for which he entered the Infirmary. What was the cause of the dulness? was it chronic pleurisy or was there a cavity?

Next day, on percussing with the hammer and pleximeter, a cracked-pot sound was immediately made evident. I subsequently had the pleasure of convincing my friend Dr Markham in this case, that most careful and skilful percussion with the fingers could only with the greatest difficulty elicit the sound, which was easily made evident by means of instruments. The progress of the case confirmed the diagnosis that the cracked-pot sound indicated. On getting better he insisted on leaving the Infirmary, and now his strength is greatly diminished, and all the signs of a cavity are present, furnished both by percussion and auscultation. Since then I have had several occasions to point out that chronic cavities often escape detection from the imperfect percussion produced by the fingers.

ARTICLE II.—*Advantages and Disadvantages of Colour-Blindness; its Prevention and Cure.* By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.

(*Concluded from page* 403.)

IN discussing the evils (which must take precedence of the advantages) of colour-blindness, I do not propose to enumerate minutely the disadvantages which it entails on its subjects, but to confine myself to certain of the more formidable difficulties or dangers in which it involves both themselves and others who trust to their judgment in reference to colours.

It will be convenient to discuss the subject under three divisions.

1. *Does colour-blindness admit of cure?*
2. *Does it admit of temporary palliation or correction?*
3. *For what professions is colour-blindness a disqualification?*

1. *Does colour-blindness admit of cure?*

Congenital colour-blindness is certainly incurable; and the case of Mr B. (*ante* vol. xvii., p. 56) shows that when induced by injury or disease, it may become as irremediable as if it had been an inherited peculiarity; but certain forms of this affection are transitory and admit of cure.

Mackenzie, Wartmann, Ruete, Cunier, Boys de Loury, W. Cooper, and others, have recorded cases of temporary colour-blindness resulting from "congestion, hepatic derangement, and dyspepsia,"[1] which disappeared under appropriate treatment. Such cases are probably very common, and appear to exhibit the same phenomena as congenital cases, from which, indeed, they differ only in being temporary and curable.

An opposite opinion, so far as permanent colour-blindness is concerned, has been expressed by Dr Pliny Earle (himself a member of

[1] Wartmann, 2de Mémoire, p. 20. W. W. Cooper, article Vision, Cycl. of An. and Phys.

a family in which seventeen cases of this affection of vision have occurred), but, so far as appears, he only contends that "a succession of years modifies the Daltonism of the same individual," without stating in what way, and to what extent, advancing years induce a change.[1]

So far as I can ascertain from the examination of the cases of colour-blindness within my reach, the amount of modification in the perception of colours, induced by age, is inappreciable, even though no allowance be made for that alteration in all the powers of vision which time produces on every eye. Thus: Dalton was certainly as colour-blind at the Oxford meeting of the British Association in 1832, where he compared the colour of his scarlet D. C. L. gown to that of the leaves of trees, as in 1792 when he first discovered his colour-blindness; nor did any change, so far as his associates were aware, occur in his perceptions of colour up to his death in 1844. Mr Milne of Edinburgh is still (October 1854), as colour-blind as he was when Mr Combe described his case thirty years ago,[2] and as he had been for years before his case was described. Prof. N., whose case I have referred to previously (*ante* vol. xvii., p. 501) was examined as to his perception of colour, some thirty years since, by Sir David Brewster, who has recorded his case. He writes me recently, "I am under an impression that some change in appreciating colours took place in my eye between childhood and youth. As a child, red gooseberries seemed to me altogether blue, so far as I remember; latterly I have observed what I fancy red in this variety of fruit:" and again, "I suppose sometimes that I can distinguish red in some objects, but probably this is from knowing that they are usually of this colour:" but he adds, "at any rate, I am quite sure I should make a dangerous railway signalman, as I most certainly would not know a red flag from a green one." This gentleman further states, in answer to some queries, that *pink* still appears to him by daylight *blue*, and by gaslight *green*, and that he continues to confound carmine-red by daylight with blue. There plainly has been nothing deserving the name of improvement in his case.

Lastly, the Countess of D., whose case has been described previously, as it showed itself in 1853, has not appreciably altered in her colour-blindness since its peculiarities were described by Wollaston many years ago.

Those cases may suffice to illustrate the permanency of this affection of vision; but they are not singular. Among my colour-blind acquaintances there are probably none who would not sacrifice a great deal to see perfectly; and nearly all have endeavoured to cure themselves of their visual idiosyncrasy, but not one reports a cure, and the best educated and most observant among them are the most

[1] *Amer. Journ. of Med. Sciences*, 1845, quoted in Wartmann's 2de Mémoire, p. 45.

[2] System of Phrenology, First Ed., Art. Colouring.

decided in declaring that they have given up all hopes of amendment.

It is difficult to convince many that this conclusion is a just one. Those whose own sense of colour is delicate, and who are led by taste or profession to live much among coloured objects, are slow to believe that any eye can be so peculiar in its endowment, as to make the blunders which the colour-blind do, even in reference to what they would call "a staring" red or green. Such colourists insist, that carelessness, indifference, or improper education, lies at the bottom of the mistakes which the "supposed" colour-blind make, and profess themselves willing to undertake their cure; of which, however, they record no case.

I have no doubt that a certain amount of improvement in the judgment of colours may be induced in most colour-blind persons, who have not attempted self-education in reference to their chromatic perceptions, or been unconsciously led to study colours. For as they generally perceive blue and yellow without difficulty, and distinguish, within certain limits, the tints and shades of many colours, their perception of those must, and certainly does, admit of improvement by education. But this improvement does not lessen their tendency to confound certain colours and certain tints and shades. When we find an engraver who, for the greater part of his life has been gazing all day at paintings, purchasing a red window curtain for a green one: a tailor, whose eye has been for hours daily fixed on cloths of very varied colours, matching green tape with scarlet linen, at the risk of losing his situation: an experienced field-geologist compelled, when surveying a red sandstone district, to take a companion with him, to point out where grass ends and sandstone begins: and a teacher of chemistry evading, as much as possible, the questions of his pupils concerning the colours of bodies; we cannot doubt that after education has done all that it can towards developing the sense of colour in the colour-blind, they remain as helplessly prone to make their characteristic blunders as before. A crowning example of this has recently presented itself to me. In the establishment of a painter and glass-stainer, who is an obstinate disbeliever in the existence of colour-blindness, my attention was recently directed by his fellow-workmen to a youth, who had been set to re-paint the devices on the shafts of a sheaf of arrows. These devices, consisting of alternate circles of red and green, had not been effaced, but only dulled; yet the painter executed his task by painting all the *red* rings *green*, and all the *green* rings *red*. The case was remarkable for the direct reversal of the colours in question, and this by one who had them before him to compare, both on his palette and on the arrows. Yet the party who committed this mistake was an excellent draughtsman, much esteemed by his master, and surrounded at his daily work by splendid specimens of stained glass. The mistake which he made soon ceased to be a solitary one, for his fellow-workmen having since its occurrence, put him to

the test, found him uncertain in his judgments of many colours, and on examining him I found him commit the characteristic errors of the colour-blind. Here, then, was the possessor of an educated colour-blind eye, making such mistakes, as no normal-eyed person, however uneducated his power of vision might be, could or would make.

Education, then, can do nothing towards curing colour-blindness, nor in truth can anything else. Dr Trinchinetti, an Italian physician, believing that the seat of this affection of vision is the crystalline lens, gravely proposes its extraction, as a radical cure of the evil![1] I would not refer to this proposal, which requires no formal condemnation, were it not that I have met with a case which appears to prove (what needed, however, no proof) that colour-blindness would survive removal of the crystalline lens.

I have recently been visited by Mr J., who has had both lenses removed for cataract. The one lens was removed some years ago, by Dr James Duncan of Edinburgh, the other recently by Dr Macmurdo of London. His vision is good, and his ordinary perception of colours normal, but in reply to some casual questions on these points, he surprised me by stating that, whilst suffering, on a recent occasion, from vertigo (to occasional attacks of which he is subject), he was astonished to perceive that all the persons he met in the street were dressed in *green*, "recalling," as he said, "the descriptions of Robin Hood's men." This false perception continued for some hours, and has since on several occasions returned. Here a susceptibility to colour-blindness remained, though the lenses were gone, and had the affection been congenital, instead of temporary, we may justly infer that it would have equally remained.

2. *Does colour-blindness admit of temporary palliation or correction?*

As colour-blindness is certainly incurable, the possibility of palliating or temporarily remedying it becomes an important question, and the prospect of a satisfactory answer is far from hopeless. I shall discuss, first, the methods of assisting the colour-blind which have been proposed by others, and afterwards suggest certain which have occurred to myself.

1. *Comparison of doubtful with known colours.*—A means of guarding against mistakes, relative to colours, which is unquestionably of some service to the colour-blind, is the reference of doubtful hues to a chromatic scale, accurately tinted and named. But this means of correcting error soon finds its limit, as the mismatchings of red with green, olive with brown, and purple with blue, characteristically made by the colour-blind (although both colours are presented to the eye together), are sufficient to show. And, in truth, I strongly suspect that the first chromatic judgments of the colour-blind are often more trustworthy than their later ones, and that in

[1] Wartmann, 2de Mémoire, p. 33.

general they are at least of equal value. This remark specially applies to the confusion of red with green, where each so rapidly calls up to the colour-blind eye its complementary, that it is quickly disabled from determining which was the primary and which its complement.

2. *Protracted gazing on coloured bodies.*—Szokalski and Seebeck have recommended, as a means of ameliorating the condition of the colour-blind, their steady gazing, first, on a coloured surface, and then on a white or black one,[1] but from what has been stated in the preceding section, and previously in reference to the general education of the colour-blind, this proposal seems of no value; nor is any proof offered that it has been of service.

3. *Use of coloured transparent media.*—It has occurred to various observers that assistance might be afforded to the colour-blind by causing them to look through coloured transparent media at the colours which were doubtful to them. Wartmann observes on this point: "There exists a very easy means of rectifying, to a certain extent, the error of the appellation of colour. This means consists in examining coloured objects through a transparent medium, as a glass or a liquid of a certain known tint. Suppose this tint red; the impression of a green body and of a red body the same at first to the naked eye, will become manifestly distinguished by the use of the transparent screen. This method appears to have been practised for the first time by Professor Seebeck, the father, towards the year 1817. Nothing can equal the surprise of a Daltonian when the errors which he commits every day in the appreciation of colours are thus disclosed to him. Unhappily the tint of the coloured glasses and their number cannot be prescribed in advance; the impossibility of a rigorous classification of the innumerable varieties of Daltonism obliges us to choose them *à posteriori* for each particular case. We may add, that their employment only remedies mistakes in the specific nature of colours, and leaves in general those which apply to one and the same tint."[2] Dr Steebach has carried out this proposal the length of recommending the use of coloured spectacles.[3] Trinchinetti advises that the glasses should be of the colour which is the complement of that liable to be seen falsely.[4] To this Wartmann justly objects, because "Daltonians do not judge correctly of complementary colours;" but his own proposal to employ transparent media of other colours is liable to as great objections.

I am far from wishing to affirm that a colour-blind person may not be aided in correcting his chromatic errors by such a use of coloured glasses as Seebeck and Wartmann recommend, but after many trials with my colour-blind acquaintances I have found none who could turn the suggestion to practical account. My trials have been chiefly made upon those who confounded red with green, to

[1] Wartmann, 2de Mémoire, p. 50.
[2] Taylor's Scientific Memoirs, 1846, p. 185.
[3] Wartmann, 2de Mém., p. 50.
[4] Ibid, p. 39.

whom Wartmann, in illustrating the use of coloured transparent media, specially refers, but I have met with no success. Nor does this surprise me; I should have been surprised had it been otherwise. How could it be expected that an eye to which the same glass seems sometimes red, sometimes green, should be assisted in distinguishing between opaque bodies of these two colours by looking through the glass? The colour of the glass is not a constant quantity for the colour-blind, any more than the colour of the bodies looked at through the glass. If the last were actually red, and appeared of this tint to a colour-blind eye looking at red, the latter, we may presume, would appear redder; but if the glass appeared green whilst looking at red, this would probably also appear green, for the same cause (whatever it be) which falsified the colour of the transparent medium, would falsify that of the opaque one. The only coloured transparent media which can be expected to serve the colour-blind are such as do not alter in apparent colour to their eyes, and the choice must therefore be restricted to those which are blue or yellow. I shall presently seek to show that yellow media promise to be serviceable, but the consideration of this belongs to another place.

4. *Employment of touch to distinguish colours.*—Wartmann has pointed out the interesting fact, that in one well marked case, a colour-blind person "corrected by the help of touch, a part of the erroneous judgments which he formed regarding colours,"[1] and he had reason to believe that several other "Daltonians" were similarly endowed.

The first example of colour-blindness I encountered (Case II., Mr P.) possessed the power in question to some extent, and I found it useless to show him the same pieces of coloured paper more than once, as he recognised them the second time, but not, as he distinctly declared, in virtue of the coloured impression they made on his eye. It was only in part, however, by touch that he effected this recognition, for slight differences in shape, accidental rough points, folds, wrinkles, and the like, caught his quick eye, and assisted him in reaching his conclusions. It seems to me exceedingly probable that this gentleman, whose sense of touch was practised and delicate, could have taught himself to detect by means of it differences between pieces of paper, cloth, linen, silk, and the like, dyed with different substances. The physical properties of pigments are sufficiently diverse, to alter in different ways the surfaces or substances which they colour. Wools, *ex. gr.*, dyed with certain substances are much harsher to the touch than those dyed with others; the mineral pigments, such as Prussian blue, or chromate of lead, in general producing rougher surfaces than the organic dyes, such as indigo. A wool dyed with a mineral green might thus be distinguished by the touch, from one dyed with a vegetable

[1] 2de Mémoire, p. 35.

red, although the colour-blind eye could detect no difference between their tints.

These conclusions are strengthened by a consideration of the fact, that even among those who are totally blind, examples occasionally occur of a power to distinguish colours by the touch.[1] The fact has been denied. But many of my readers must have seen the power in question exercised by a blind man in Stirling, whom I have frequently conversed with in my boyhood. He was regarded as one of the celebrities of the place, and was brought to strangers to earn a small fee, by telling the colour of their dresses by feeling them. This I have seen him do with men's dresses promptly and correctly.[2]

It seems quite possible accordingly, that colour-blind dyers, weavers, clothiers, and other workers among textile fabrics, may use their fingers to correct the errors of their eyes; and further, that the keen vision which (colours apart) they generally possess may, acting in unison with touch, turn to account those superficial, interstitial, and molecular differences which are the concomitants of different colours, in consequence of these inhering in different chemical compounds.[3]

[1] Wartmann, 2*de Mémoire*, p. 36, gives a list of references to the published cases, amounting to some three, but thinks the existence of a tactile discrimination of colours not yet sufficiently established.

[2] Alexander Lyon, the party referred to in the text, familiarly known in Stirling as Blind Alick, was born in 1761, and died in 1836. A sketch of him is in existence from the pencil of George Harvey. He lost his sight in early infancy, and grew up untutored till he was eighteen years old, when the indications which he displayed of a very retentive memory, led to his being sent to school, through the interference of some benevolent friends. Here he acquired an acquaintance, believed to be complete, with the contents of the Bible, through hearing it read, verse by verse, by his schoolfellows; and in after life he could repeat any portion of it on a moment's warning. His knowledge in this respect was often tested, but was never known to fail. It was the fruit solely of a powerful memory, exercised upon the only book which he had the opportunity of hearing largely read in his early days, and did not imply any regard for its precepts, of which his life was a daily contradiction. So far as I can learn, he acquired the power of distinguishing colours by manipulating the dresses of his schoolfellows, as a means of distinguishing them from each other, when they tried to deceive him as to their identity, by using feigned voices. It does not appear that "he could indicate the colours of silk or cotton fabrics by touching them," as I learn from Mr Shirra, an intelligent draper in Stirling, who knew Blind Alick well.

[3] Since writing the above, I have received the following communication from Mr Shirra, referred to in the preceding note. It supplies a confirmation of the conclusions in the text all the more valuable, that it was written in total ignorance of my opinions, and not in answer to any queries.

"I have heard of a woman, blind from her infancy, residing in Cambusbarns, who practised the occupation of a 'pirn-filler' (that is winding yarn on bobbins). Her sense of feeling enabled her easily to judge of the different sorts she was engaged with. If she were winding blue, and had finished one spindle, she would lay her hand on the next spindle, and could tell easily whether it was blue, green, purple, scarlet, or black. White she distinguished by her sense of smell, it being 'smeekit,' or fumigated with sulphur.

"I have heard that practical dyers are able to distinguish coloured yarns by

In addition to the methods of mitigating colour-blindness which have now been discussed, I suggest the two following, the first of which, at least, is probably of more importance than any as yet made known to us.

1. *Substitution of artificial light for daylight in the examination of colours.*—The very striking difference between the aspect of certain colours to many of the colour-blind by daylight and artificial light, has been so fully referred to previously, that here I only discuss the practical application of the fact. The colours whose aspect varies most, are red (including crimson and scarlet) as contrasted with green; and pink, crimson, and other tints or shades of purple (especially red-purple), as contrasted with blue.

It is only some among the colour-blind who report themselves as seeing colours thus differently by different lights. But the number thus circumstanced is large, and the conclusion to which Dalton came, that there was much less difference between the judgments on colours of the colour-blind and of the normal-eyed, by candlelight, than by daylight, probably admits of wide application.

It is at least certain, that to very many an alteration in the quality of the light by which colours are regarded, causes these, as Dalton expressed it, to be "astonishingly changed." Supposed greens become by candlelight, reds; and blues are turned into pinks and crimsons. The contrast in appearance under the opposite illuminations is so great, that among the cases of colour-blindness which I have recorded, there are as many as six persons who have discovered that they could mitigate their peculiarity of vision by the employment of artificial light. Thus, Mr N., Prof. Y., and Dr E., have resorted to candlelight as a means of increasing their enjoyment of flowers, which, if crimson, appeared, when seen by it, much richer in colour than by daylight. Dr Y. has discovered that by gaslight he can profit by chromatic distinctions, which are lost upon him by day. Mr R. can recognise, by lamplight, reds, which he mistakes for greens in the sun; and most striking of all, Mr S.'s friend (*ante* vol. xvii., p. 501), who deals in coloured goods, has long been in the habit of appealing to a gas-flame in a dark room, to decide between scarlet and green, and crimson and blue. Those cases are sufficient to show that every colour-blind person may find it worth while to try the effect of artificial light in correcting his false visual perceptions.

What quality of light is best fitted to secure this correction remains to be determined, and an inquiry into the matter could not

the sense of touch, as well as the sense of sight, some peculiarity being imparted to the wool by the combination of substances used in dyeing, which gives it a peculiar character, sufficient to be known by mere contact."

It will be seen that wool alone is referred to in this statement. The colours of linen, cotton, silk, velvet, and the other fine-thread fabrics are plainly much less easily detected by touch than those on wool.

fail to supply much interesting information. From a few trials, I feel satisfied that the whiter and more intensely luminous artificial lights, such as the lime-ball and the electric charcoal light, induce the same confusion in the perception of colours as sunlight does.

Between the ordinary artificial lights, there is not apparently an appreciable difference, but candlelight alone, has been specially examined. Dalton ascertained that in the prismatic image of a candle-flame, the red extremity was much more vivid than that of the solar spectrum, and from the mode in which he refers to the candle spectrum, it seems certain that there was no such dark shade, or dimly illuminated band at its less refrangible end, as there was generally to his eye in the spectrum of the sun.

It should thus seem that to the colour-blind (in so far, at least, as they are represented by Dalton), the red rays of a candle-flame are visible, to an extent those of the sun are not, and in consequence red bodies are much less liable to be considered green; and pink, or crimson bodies, to be considered blue by candlelight than by daylight.

In speculating on the cause of this difference, with a view to its practical application, we cannot fail to recognise as chiefly important, the feeble intensity of candlelight as compared with that of sunlight, and the unlike ratios in which the coloured rays are distributed in the two kinds of light. So far as luminous intensity is concerned, the difference is immense, and as one peculiarity of the colour-blind eye is to see no colour in very lightly tinted bodies, *i.e.*, where much uncoloured light simultaneously falls upon the retina; and another is to perceive form and outline by very faint light, we may attribute in part the improved vision of the colour-blind by a candle, lamp, or gas-flame, to its feeble intensity compared with sunlight. It is plain, however, from the case of Mr N. (*ante* vol. xvii., p. 496), that if the intensity of the light fall below a certain point, then the reds which were formerly seen, change into black, or become totally invisible. There is a practical limit thus set (varying no doubt with each eye), to the employment of faint light as a means of assisting the colour-blind.

It is to the chromatic qualities, however, more than to the feebler luminosity of artificial light, that we are probably to look for the service which it renders those whose vision of colours is abnormal; for it does not appear (though perhaps the matter has not been sufficiently investigated) that there is any degree of intensity of solar light by which they make fewer mistakes than by full daylight; whilst, on the other hand, it has been fully shown that in twilight, even the normal eye betrays, in a minor degree, that blindness to red which is so characteristic of chromatopseudopsis.[1]

[1] Very bright and very faint light both diminsh the sensitiveness of the colour-blind eye *to colour*, and therefore there must be a point of medium luminous intensity, where colours are best seen by it; but as the reports of cases of colour-blindness, in effect record the sum or mean of the experience of

The colour of ordinary artificial light is sensibly different from that of daylight, compared with which it is yellow or orange; in other words the proportion of blue rays is smaller, and of yellow and red rays larger than in daylight.

It seems probable that the preponderance in artificial flames of yellow light to which the colour-blind eye is keenly sensitive is a great cause of its better vision by such flames; but the presence in these of red rays which impress the abnormal organ of vision, as the red light of the sunbeam does not, must also largely contribute to the improved perception of colours. There must also be some proportion of red and yellow rays, in other words some quality of artificial light, best fitted to approximate to normality, the vision of the colour-blind. That in a candle we have this quality of light at a maximum is matter of surmise, and experiment alone can show among the artificial lights in ordinary use which approaches most nearly to it. The colour-blind could largely assist themselves by comparing their arrangements of coloured objects by different flames with a settled chromatic standard; and others could assist them by noting what colours they saw in the prismatic spectra of such flames. I have but one fact to mention in reference to this question. In the course of the trials made along with Professor Kelland on the visibility of the red end of the solar spectrum to the colour-blind, the opportunity was taken to test the vision of those present by a soda-flame, fed with oxygen, and made to illuminate wall papers of very varied colours. To those regarding themselves as normal-eyed, these became all undistinguishable greys, whilst Dr Y. and Mr R. saw, as they declared, many colours, and pointed out two sectors of a seven-tinted circle as *green:* the one was green, the other red, so that they seemed to see colours at least as well by the soda-flame as by daylight.[1]

its subjects under all ordinary intensities of light, and register their most perfect judgments, it is plain that the difference between their best and worst decisions on colours, by light not extremely bright or faint is small; not greater, certainly, than that between the similar decisions of those whose vision is normal.

[1] A point of much interest in connection with this subject, but on which I can communicate nothing certain, is the extent to which the substance or texture of a coloured body, and the nature of the dye, affect its relation to solar and artificial light as seen by the colour-blind. From what we know of normal vision, however, we may infer that silk and wool, *ex. gr.* of the same colour by daylight, will not necessarily, to their eyes, undergo the same kind and amount of change by candlelight. Thus, green silks and wools, which by daylight match very well in the estimation of the normal-eyed, often prove very indifferent matches by gaslight, owing partly, doubtless, to the difference of textile arrangement, partly to the difference of dye. I have recently been shown a lilac silk ribbon which to those (all of normal vision) who have seen it by daylight, appears to match closely with a lilac worsted. By gaslight, however, the latter becomes a cold grey, harshly contrasting with the silk which appears less brilliant, but otherwise little altered in tint. The colour-blind, we may be certain, have similar experiences, and we must not wonder if artificial light assists them more in seeing certain coloured stuffs than others.

It is not probable, however, that the different colour and lesser luminous intensity of artificial light, are the only causes of its usefulness to the colour-blind. Much of this may depend on the different amount and intensity of the thermic and actinic forces in light from different sources, which assuredly vary immensely, and, as yet, has been imperfectly measured in most cases.

Further, two flames may have the same colour, and yet contain rays of very different refrangibilities. Thus the extra-spectral (or fluorescent) rays of Mr Stokes are greatly more abundant in some blue flames than in others (as they also vary in colourless flames), and two of those which to the ordinary observer will appear identical, will, nevertheless, affect the eye (normal or abnormal) very differently, for, whatever theory of light we adopt, we must suppose that rays so unlike in their refrangibility, as the least refrangible blue of the visible solar spectrum, and the most refrangible blue of the extraspectral space make a different impression on the retina. The same remark applies to rays of other colours, and trial can alone decide how their distribution in a flame affects its influence on the colour-blind.

2. *Employment of yellow or orange transparent media to reduce daylight to the quality of ordinary artificial light.*—If artificial light assists the colour-blind in distinguishing colours, it should seem possible to correct their daylight chromatic errors, by conferring upon the light of the sun the properties (whatever they are) which enable artificial light to restore colour-vision to the colour-blind.

It seemed possible to effect this to some extent by passing daylight through yellow or orange media, and, accordingly, I obtained four pieces of glass stained with oxide of silver, and transmitting a pale yellow or orange light. They are referred to in the sequel as 1, 2, 3, and 4, being numbered according to their intensity of colour from the palest to the deepest. Two of them, 3 and 4, appeared yellow when looked through, but reflected from one surface a bluish-green light. These glasses, as will presently appear, proved of most service to the colour-blind.[1]

Specimens of the four glasses were sent to Professor Y., Mr R., and to the colour-blind friend of Mr S. (*ante* vol. xvii., p. 501.) Professor Y. derived no benefit from them; Mr R. found two of the falsely fluorescent ones of some use, and they were markedly serviceable to Mr S.'s friend.

[1] Mr Stokes refers the twofold colour of silver-stained glasses almost entirely to a *false* internal dispersion of light, and not to that true dispersion which he has named *fluorescence* (*On the Change of Refrangibility of Light. By G. Stokes, M.A., Phil. Trans.* 1852; *and R. Hunt's Researches on Light, 2d edit.* 1854, *p.* 312); but the latter phenomenon was displayed to some extent by the glasses alluded to in the text, which were coloured throughout their entire thickness. The mass of the glass owes its tint to a silicate of the oxide of silver which becomes wholly or partially reduced to the metallic state at one surface during the process of staining, rather as an unavoidable result, than as a thing aimed at by the stainer.

Mr R. writes,—"I have delayed replying to your letter until I could give your experiments a fair and equitable trial. The glasses (all of them indeed) deepen or rather brighten the reds, but do not affect the greens so much. I can distinguish the reds by the aid of the glasses much better than I can the greens."

In a second letter he adds,—"Yesterday and to-day being very bright days, I have tried a series of experiments with the yellow glasses, and in all cases found No. 4, the most decided and certain in its results. No. 4, you will perhaps recollect, is a combination of two colours, one on each side of the glass. This gave a decided and very marked difference between red and green. Scarlets it made very bright and light, while crimsons were made decidedly red and fiery."

From Mr S. I received the following statement written by the party (*ante* vol. xvii., p. 501), who was in the practice of resorting to a room containing a gaslight to distinguish between scarlet and green, and crimson and blue. "With the glass marked 4, I can see bright colours nearly as well as by lamplight, so that I can see the same difference between green and scarlet, and crimson and blue, as by artificial light, but it being rather dull I cannot see any but very bright colours. With No. 3 I can tell which is green, but not quite so well as with 4; Nos. 1 and 2 are of very little use."

Three additional pieces of glass, all to some extent falsely fluorescent, but paler in tint than No. 4, were sent, and were thus reported on:—

"I find the last three pieces of glass answer admirably, as they are clear, and show the colours almost exactly the same as they appear to me by lamplight, so that I can easily see the difference in most bright colours. . . . .

"All three of the pieces show a difference sufficient for me to know green, but No. 3 makes green look nearly black, while red looks brighter."

It will be seen from these statements that there is a prospect of success from the use, by the colour-blind, of glasses tinged yellow or orange, or of window-screens of these colours for rooms in which coloured things are to be examined; how far the false dispersion of the glass affects its utility I cannot determine.

I have not included the proposal just discussed, in the consideration of the suggested use of coloured glasses by other writers, because it was not the chromatic characters of yellow transparent media which led me to suggest them, but their power to confer upon daylight sent through them, certain of the properties of artificial light, as this affects the colour-blind eye.

That the colour of the modified light is one cause of its action on the eye cannot be questioned, and Sir David Brewster's opinion that yellow light has a special exciting power over a torpid retina, may be fitly connected with this conclusion; but on that opinion I do not build much, nor was it known to me, when I suggested

the use of silver-stained glasses to the colour-blind. Their eyes are not torpid, but, so far as perception of form and outline is concerned, preternaturally sensitive to the action of light; nor do sufferers from ordinary amaurosis see better by candlelight than daylight, but the reverse.

I do not refer to these matters because the theory of the beneficial action of yellow glasses is practically important, but because I wish to impress upon the reader that it is not the mere colour of a glass or other transparent medium, that is to be regarded as the criterion of its probable utility to the colour-blind. The latter (and I only profess to assist those among them who distinguish colours better by candlelight) must select transparent media for themselves, preferring those, *whatever their colour*, which cause daylight transmitted through them, to appear as artificial light does; and it must not be forgotten that the chemical composition and molecular structure of such media on which their relative transparency, fluorescence, diathermancy, and power to transmit or arrest actinic force depend, are probably of as much importance as their colour. Thus; of four yellow glasses stained respectively with uranium, silver, iron, and organic matter, it is probable that though their tints may be the same, they will not be of equal service to the colour-blind; for the first is highly fluorescent, the second moderately so, the third and fourth scarcely at all; and a similar remark is applicable in some degree to glasses stained with the same substance, but different in structure and composition.

Further, we must not expect to make other than an imperfect approach to the conversion of daylight into artificial light, by transmission of the former through coloured or other media, for we are as yet very ignorant of all the points of difference between them.

3. *For what professions is colour-blindness a disqualification?*

The little which art can do towards palliating colour-blindness, and the hopelessness of curing it, demonstrate the necessity of excluding from certain professions, where that is practicable, those who suffer from it. Children in whom it is discovered should be dissuaded from callings such as the house-painter's, the dyer's, and weaver's, which have largely to do with colour; and where there is a choice, should not be strongly encouraged to follow professions like that of the analytical chemist, naturalist, botanist, geologist, or physician, which, though they admit of prosecution to a great extent by those whose sense of colour is defective, cannot be studied in some of their most important relations as sciences or arts, by such as are. Unless, therefore, the bias is strong towards the professions in question, it should be turned in another direction, though where it is powerful, as in Dalton, it will vanquish the obstacle which colour-blindness occasions.

So far as regards the fine arts, I believe that an instinctive or

readily developed consciousness of an imperfect apprehension of colour, will keep the colour-blind, who have a strong love for them, from attempting to excel as colourists (though certainly this rule does not always hold), and that such persons will be found to possess an eye for form, and outline, light and shade, chiaroscuro, aerial perspective, and the other achromatic aspects of pictorial objects. This admits of ample employment and gratification in such mono-chromatic works as crayon, Sepia, and Indian ink sketching, in lithography and engraving, and most fully of all in architecture and sculpture. It is in truth much more the economical than the æsthetical chromatic arts which are forbidden to the colour-blind.

The professions, for which colour-blindness most seriously disqualifies, are those of the sailor and railway servant, who have daily to peril human life and property, on the indication which a coloured flag or a lamp seems to give. Fortunately a ship is seldom under the guidance of a single person, and in her Majesty's vessels the colour signal-men are selected from a large number, and are ascertained to have a quick eye for colour. In merchant ships, the choice must necessarily be made from a much smaller number, and the appalling yearly list of lost vessels which appears in our Wreck Returns, awakens the suspicion that more than one of these fatal disasters may have resulted from the mistaken colour of a light-house beacon or harbour-lamp, which on a strange coast, and with perhaps the accompaniments of a snow-storm or a thick fog, has been wrongly deciphered by a colour-blind pilot.

On railways the danger attending mistakes of signals is much greater than at sea, especially in this country, where trains travel at a very high speed, and succeed each other at very short intervals. Incidental reference has been made to this subject in various parts of the preceding pages, and it is much too extensive to be discussed at length here. Thus much only can be said: The most marked peculiarities of the colour-blind are shown in mistaking, (1) bright red for green; (2) dark red for brown; (3) red for black; and (4) dark or light shades of all colours for each other. The caution-signal, *green*, is thus liable to be mistaken for the danger-signal, *red* and the latter (when it appears black) not to be seen at all.

Our railway directors have, indeed, been most unfortunate in their selection of red and green for coloured signals, especially as seen through the day on flags. These colours are mistaken for each other by certain of the colour-blind, even when fresh and bright. As they darken with use, they become objects of perplexity to an additional number of such persons; and at a distance, whether new or old, they are mistaken by many who can distinguish them near. Further, they are liable, when imperfectly illuminated, as in twilight, in fogs, or snow-storms, to become altogether invisible; so that, in short, just when it is of greatest importance that they should

be seen, they are most liable to be misapprehended, or not seen at all.

For this great evil there are two remedies. If the present system of signals is continued, no colour-blind person should be admitted as a railway servant. I am happy to say, that the publication of my papers has induced the Great Northern Railway Company to require that, in future, all their porters shall be tested as to their freedom from colour-blindness before they are admitted. For this wise measure the public are indebted to the exertions of Graham Hutchison, Esq., one of the directors of the company, whose attention was turned to these researches by Dr W. Mackenzie of Glasgow.

The other and better remedy, is to make the shape and movements of signals the chief index of their purpose, and to dispense with colour, except as a subordinate aid to this. Were this done, the colour-blind would not only be admissible as signal-men, but from the quick and accurate perception of form and outline, and the power of exercising this in faint light, which characterises many of them, they would often prove the best of all signallers. I must reserve, however, for another place the full discussion of this important subject, and with this remark I bring this series of papers to a close.

ARTICLE III.—*The Physiology and Pathology of the Non-Vascular Tissues generally; with more especial reference to the Non-Vascular Tissues of the Eye.* By JOHN MACEWAN, Jun., M.D., Licentiate of the Faculty of Physicians and Surgeons of Glasgow; Member of the Glasgow Medico-Chirurgical Society, of the Glasgow Medical Society, etc., etc.

"IF we are to estimate the importance of a part by its necessity to the human frame, the heart with its system of vessels must occupy the highest place, as the loss of it ensures the immediate death of the animal, which ensues not even with the removal of the brain itself." Now, the heart with its system of vessels—arteries, capillaries, veins—constitute what properly are included under the head of the vascular system; it being the function of this system to carry the blood loaded with nutriment to the different parts of the body. Through these different parts the capillaries ramify, and the nutritient liquor sanguinis exuded through their coats is supplied to the tissues. Now the vessels themselves vary in number in different structures. In some structures, as in the muscular, the capillaries are very numerous, the spaces between them being very small, whereas, in other structures, as in the ligamentous, the spaces between the different capillaries are very large; so that it will be at once seen that in all vascular structures there is of necessity a considerable extent of tissue which is nourished without being in contact with blood-vessels. And this is the first step we reach in

our inquiry regarding the non-vascular tissues. We find that in every tissue, as demonstrated by the microscope, there is a certain portion (be it the 50th or the 5000th part of an inch) which is nourished without being in contact with the capillary vessels.

Now, into certain organs anatomists have never succeeded in tracing blood-vessels at all, and it is these organs which are said to be *non-vascular*. But in this paper it will be our endeavour to show that although non-vascular—that although no vessels have been traced into them, still, in accordance with the general law bearing upon all tissues in the body, they are supplied freely with blood, and that the very position of these tissues in the body excludes the possibility of vessels being permitted to circulate through them, without ever and anon giving rise to lesions dangerous in the extreme, and to consequences highly injurious to the functions of the part.

FOR, WHAT ARE THE TISSUES WHICH ARE THUS NON-VASCULAR? We have,

1*st*, *Cartilage and Fibro-Cartilage*, which, from their situation in the joints, etc., would be constantly subject to repeated concussion, to compression, to attrition, etc., and consequently the integrity of the blood-vessels would be in constant danger of being destroyed.

2*d*, *The Epidermoid Appendages*, as the *epidermis*, *nail*, *horns*, *feathers*, etc., etc., which, like the last, are unceasingly exposed to friction and laceration.

And, *lastly*, we have in the eye, the *cornea*, the *humors* and their different *capsules*, and the *granular layer in the retina*, through which the rays of light must pass before they can act upon the grey nervous portion of the retina, and consequently were red blood circulating through them they would not be as they are in a state of health perfectly transparent, and as a natural consequence vision would be impaired.

If, therefore, no blood-vessels are traceable into these non-vascular tissues in their healthy state, a question naturally presents itself —*How are they nourished?* Now, in answer to this we would commence by remarking that it is a well-established law, or perhaps more properly speaking, a self-evident principle, that in all structures, whatever may be the degree of their vascularity, the tissue the furthest removed from the blood-vessels must be nourished by the nutrient liquor sanguinis equally well with that which is in immediate contact with them. The supply of appropriate blood nevertheless must be in or near the part to be nourished. We cannot exactly say how near it must be; but probably all that is necessary is, that the nutritive material should admit of being imbibed in sufficient quantity into the substance of the part. For imbibition must be regarded as the means by which all parts supply themselves with nutritive material, thus deriving it from the nearest blood-

vessels; and the blood-vessels themselves being only the channels by which the materials are brought near. Now, anatomists have never been able to trace blood-vessels into these tissues called non-vascular. Some, as Mr Bowman,[1] for example, find that mercury may be made by pressure, to track along branching channels in the cornea, which channels appear regularly arranged as if they were blood or other vessels, but Virchow,[2] His,[3] and others, think that they are only spaces accidentally made among the corneal layers. The cornea, as every anatomist knows, is a laminated structure; we have in its substance the anterior elastic lamina and the proper lamellar or fibrous structure of the cornea itself; and these different laminae again are not perfectly distinct, but are found to be united at different parts by means of a tissue which, like the structures it unites, is itself laminated. By means, then, of these two structures the cornea becomes more or less porous, and hence Bowman's supposition regarding corneal vessels.

All physiologists (as far as I have read) grant that unless when in a state of disease red blood does not circulate through these non-vascular parts; but some, though perfectly erroneously, suppose that there are blood-vessels in these which they are inclined to call serous or serum carrying vessels; vessels so small that they do not admit of a red blood corpuscle, but through which serum alone circulates. Coccius,[4] for example, describes and figures serum carrying vessels as offshoots from the cornea capillaries like short blind processes. Now, proper investigation has shown that there is no such thing as these serum carrying vessels, but that, on the other hand, the arteries which anatomists have traced no farther than the circumference of these non-vascular tissues, actually terminate in veins without the limits of these tissues, as the figures in plate xvi., accompanying Mr Toynbee's excellent paper[5] in the Philosophical Transactions will clearly show; and what Mr Toynbee has shown to be the case with the cornea will be found to apply to all the non-vascular tissues in the body.

A question then presents itself. If the blood-vessels reach only the circumference of these tissues, how is the blood when it is exuded from the blood-vessels at the circumference carried throughout the whole tissue? Now, in explanation of this we would remark, that all these tissues are found to be very analogous in structure, that they all contain corpuscles or cells varying in number in different

[1] Bowman. Lectures on the parts concerned in the operations on the eye, etc. London, 1849.

[2] Archiv. B. 4, H. 1.

[3] His. Ueber der Bau der Hornhaut; in the Transactions of the Physico-Medical Society in Wurzburg, B. 4, H. 1. 1853.

[4] Coccius: Ueber der Gruahrungsweise der Hornhaut, und die Serum führende Gefasse. 8vo. Leipzig, 1852.

[5] Joseph Toynbee: Researches to Prove the Non-vascularity and the Peculiar Uniform Mode of Organization and Nutrition of Certain Animal Tissues, etc. Philosophical Transactions of R. S. of London. Part I. 1841.

tissues, some of them being almost entirely made up of them, while in others, as the cornea and vitreous humour, only a few exist; but in those tissues where a few corpuscles only are present, the laxity of their consistence admits of their ready penetration by the nutrient fluid brought by the blood-vessels to their circumference. Mons. Schwann[1] has clearly shown that these corpuscles or cells have a vital action, that gradually those cells at the circumference of the tissues become elongated, are formed into bundles of fibres, which fibres eventually make up the tissues themselves. This will be best explained in his own words by a quotation from his published researches. The organ in the eye which he takes up in explanation of this is the crystalline lens. Says he—"The mode in which the lens is nourished has always been an enigma. Having no vessels, it has either been regarded as a secretion of its capsule, or its mode of life has been considered as generally resembling that of vegetables. We shall find the latter to be the correct view, and the singularity of the mode of its nutrition disappears altogether when we become acquainted with the fact, that the growth of the organised tissues resembles that of vegetables. The general statement that the lens has the vitality of a vegetable does not however express much, unless the relation of its elementary structure to the cells of plants be proved. The lens is known to be composed of concentric layers, made up of characteristic fibres, which, not to go into details, may be said to pursue a general course from the anterior to the posterior surface. In order to become acquainted with the relation which these fibres bear to the elementary cells of organic tissues, we must trace their development in the fœtus. . . . . The following conditions of the crystalline lens may be formed in the mammalia. In a fœtal pig, three-and-a-half inches in length, the greater part of the fibres of the lens is already formed, a portion however is still incomplete, and there are many round cells awaiting their transformation. The perfected fibres form a sphere in the centre of the lens, but there is no laminated structure as yet perceptible in it. The fibres may readily be separated from each other, and proceed in an arched form from the anterior towards the posterior side of the lens. This sphere, composed of the perfected fibres, becomes surrounded, in the circumference of the lens, with a thick and broad zone of fibres, which are as yet imperfectly developed. They have much the same course as the others, *i.e.*, they form arches from the anterior towards the posterior surface. They do not however reach the axis either in front or behind, but the fibrous zone is thickest in the middle, gradually diminishing towards the anterior and posterior surfaces of the lens, and terminate altogether without the fibres meeting anywhere in front, or reaching the axis. No laminated structure can be perceived in the zone; but the fibres may be readily in-

[1] Schwann: Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants. Published by the Sydenham Society. London, 1847.

sulated throughout its entire breadth. When the ends of these fibres are examined, they are found to be either simply rounded off, or to terminate in a small round dilatation, or to pass over into larger cells; or, on the contrary, it may be more correctly expressed by saying that the layers of globules or cells are elongated into these fibres. The transition from cells to fibres may be either very gradual or somewhat sudden; but even in the latter case, the contour of the cell passes immediately over into that of the fibre, so that the latter is not merely affixed to the globule, but is a true continuation of it. Now, these cells which become elongated into fibres, perfectly accord with other neighbouring cells which are as yet quite round; and these again accord with the cells forming the greater portion of the lens in the embryo chick. They are round, extremely pale, smooth, transparent, and of every various size. Some contain nuclei; some enclose young cells. The presence of the nucleus and the fact of the outline of the cells being too sharply defined for mere shadows, would be sufficient to render their cellular character probable." M. Schwann further shows that if we examine the lens of a chick after eight days' incubation of the egg, we shall find round cells, but detect no fibres; and from this fact taken in connection with this other, now clearly demonstrated, that in the more fully developed lens of the fœtal pig we find many fibres and fewer round cells, and, at the same time, cells which become elongated into fibres, he very rationally draws the conclusion that we cannot but regard the fibres generally as elongated cells. It is true that a cell membrane cannot be distinguished in the fibres, nor can it be distinctly recognised on the round cells. If, however, the arguments above cited rendered its existence on the round cells certain, they must avail equally in the case of the fibres. There is no longer, therefore, any more difficulty in explaining the process of nutrition in the non-vascular tissues, than there is in that of plants. The cells grow by their own independant force, and blood-vessels are unnecessary, as the nutrient fluid can be conducted from one cell into another. And as a brilliant conformation of the view held by Schwann, that the fibres of the crystalline lens are really cells, however much they may deviate from the fundamental type of the cellular form, in other words, that in formation they resemble vegetable fibres, Sir David Brewster has clearly shown, that the fibres of the crystalline lens are dentated, presenting in this a perfect analogy to a certain form of vegetable fibre.

I may state that Barry, another physiologist, while he perfectly agrees with Schwann in saying, that the fibres of these tissues are formed from original cells, differs from him in opinion as to the the manner of their formation. For while Schwann states that the fibres are prolonged cells, Barry is rather of opinion that they are formed upon the plan of the tubes of muscular fibre, several cells coalescing into one to form a fibre. These cells

unite, and the partition wall between each gives way to from one continuous fibre. However this may be, the main fact holds good that these cells are present, that they take up the nutrient fluid, and by their vital action assist its circulation through the tissue, and that ultimately the cells are transformed into the proper tissue of the organ.

The only difference, then, which appears to exist between the mode of nutrition in the vascular and non-vascular tissues is this; that the former derive their nutrient fluid from the blood which circulates through the capillaries contained in their substance; and that the latter are penetrated by the nutrient fluid which exudes from the large vessels by which they are surrounded, and that its distribution through them is assisted by the vital properties of the corpuscles which they contain; in both cases the particles of which the tissues are composed attract from the nutrient blood the elements which nourish them. To use, then, the words of Mr Paget,[1] we see that in this way, "the blood-vessels serve alike for the nutrition of the vascular, and, as we call them, the non-vascular parts, the difference between which parts in this regard is really very little. For the vascular, the nutrient fluid is carried in streams into their interior; for the other, it flows on one surface; but in both alike, the parts to be nourished have to imbibe the nutrient fluid; and though the passage through the walls of the blood-vessels may effect some unknown change in the materials, yet all the business of formation is, in both alike, outside the vessels. Thus, in muscular tissue, the fibrils in the very centre of the tissue nourish themselves; yet these are distant from all blood-vessels, and can only by imbibition receive their nutriment. So, in bones, the spaces between the blood-vessels are wider than in muscle; yet the parts in the spaces nourish themselves, imbibing materials from the nearest surface." Mr Paget illustrates this fact still further; but as we have already alluded to it in the commencement of this paper we need not proceed further with this illustration. It is important, however, to bear the fact in mind, so that we may see the close connection subsisting between the vascular and the so-called non-vascular parts; so that also we may not fall into the error and confusion which result from speaking of the action of vessels, as if the vessels really made and unmade the parts. We have no knowledge of the vessels as any thing but carriers of the materials of nutrition to and fro. These materials may indeed undergo some change as they pass through the vessels' walls; but that change is not an assuming of definite shape; the vessels only convey and emit the raw material; it is made up in the parts and in each after its proper fashion. The real process of formation of tissues is altogether extra-vascular, even sometimes very far extra-vascular; and its issue depends in all cases chiefly, and in some entirely, on the affinities (if we may so call them) between the part to be nourished and the nutrient fluid.

[1] Lectures on Surgical Pathology. 2 vols. London, 1853.

And as another proof that such is the manner in which non-vascular tissues are supplied with blood, viz., by vessels circulating only at their circumference and not passing into their substance, Mr Toynbee has clearly shown, in his paper formerly alluded to, that the terminations of these vessels, in the immediate vicinity of the non-vascular tissues, present certain convolutions, dilatations, plexuses, and other peculiarities which differ in various parts, but in all instances enable a large quantity of blood to circulate slowly in the neighbourhood of these tissues; from which, it may be inferred, that they are subservient to the nutrition of the latter.

Such, then, being the manner in which these tissues are supplied with nourishment, it need not now be asked, *How are they affected with remedies?* or in other words, *How do remedies reach them so as to produce upon them their appropriate effects?* For the answer to this question simply is—as in other tissues, so here. The medicine enters the blood; with the nutrient liquor sanguinis it enters the tissue in the manner already pointed out; and having entered it, acts upon it in the manner peculiar to itself. But we must not stop here. For as we shall have an opportunity immediately of pointing out, these tissues, when in a state of disease, become altered as to vascularity—a fact at which we have already hinted; and this alteration is of such a nature, that the medicine has fuller opportunity of affecting the tissue than it possibly could do in a state of health. For, in the pathological state of these tissues, it is found that the vessels, which, when the part is in a state of health, proceed no further than their circumference, during disease push their way into them. This fact is well illustrated in some cases of *corneitis*, in which the whole cornea becomes so much covered by these vessels circulating red blood, that it assumes a red colour, and has been compared in this state to a piece of red cloth; a symptom which has therefore been styled *pannus*. Again, as has been well pointed out by Mr Bowman, "If we puncture or incise the cornea, the first effect is a change wrought in the natural actions of nutrition then existing in the wounded part, a change which can only be described as a mechanical interruption to these actions; and which, from the resultant train of phenomena, has been often called a stimulus. This is speedily followed by the presence of an increased quantity of blood in the vessels that are nearest to the wounded part, viz., in those of the conjunctiva and those of the sclerotica; and thus the materials from which the breach is to be made good are brought in greater abundance to the part that requires them. We cannot doubt, that as these vessels, comparatively so remote, are thus affected, so the part of the corneal tissue intervening between them, and the exact seat of injury is pervaded by a corresponding change, of which the general expression is this, that it is one of exalted nutritive vigour; the play of forces, and the interchange of material, which mark the nutritive function, being more energetic and more rapid than before. And whatever phenomena of this kind

occur in the intermediate tissue are concentrated in an especial manner about the wound itself. In a short time, even in the course of a few hours, as I have ascertained in the case of the lower animals, the vicinity of the injured part begins to contain in abundance those minute particles (corpuscles, formerly alluded to), nuclei, or cytoblasts, as they are called, which exist naturally, though sparingly, in the corneal lamellæ, and the relative quantity of which may be regarded in most tissues as an index of the intensity of the nutritive function. These particles, I say, hastily indeed and imperfectly formed, are speedily found choking the interstices of the tissues in the lips of the wound, and covering its surface, so as to occupy whatever space was left between its opposite sides, and bringing them into temporary union. From the presence of these embryo materials of new tissue, intermingled among the elements of the old, is derived that slight milky opacity that envelopes and marks the seat of a wound, and which, if the injury be extensive, may engage a considerable extent of the cornea in the direction of the neighbouring blood-vessels. The breach being filled, the new material is gradually transformed into products resembling those tissues among which it has been poured; the blood-vessels at the border of the cornea resume their size, and at length, in the most favourable instances, all vestiges of the wonderful process which had taken place vanishes away." Again, in inflammation of the crystalline lens and its capsule, which so often give rise to the important disease called cataract, this pathological state of vessels which circulate red blood, permeating tissues in their healthy state non-vascular, is clearly exhibited, as the following vivid description by Dr M'Kenzie,[1] following in the steps of Professor Walther, will at once show. Says he, "The red vessels observed in the pupil during inflammation of the anterior hemisphere of the capsule always constitute a sort of vascular wreath, situated about a quarter of a line's distance from the pupillary edge of the iris; this wreath forms a concentric circle within the pupil, and is found, on examination, to consist, not of one or a few vessels circularly disposed, but of a number of vascular arches. To this vascular wreath there run in a radiated form numerous vessels from the circumference of the capsule. Other vessels seem to extend from the pigmentum of the iris; but such are not constantly present. It is only in cases where the disease has lasted some considerable time that they appear. In other cases, according to Professor Walther, vessels seem to be prolonged rather from the capsule into the posterior surface of the iris. Those which run from the iris to the capsule never arise from the edge of the pupil, but at a little distance from it, on the posterior surface of the iris, so that nearly a line's breadth next the pupillary edge is free from these vascular sproutings. From the vascular wreath already mentioned, vessels are seen spreading towards the centre of the anterior capsule, and

[1] M'Kenzie: A Practical Treatise on the Diseases of the Eye. London.

these again forming clusters and arches. Although the continuation between the vessels seen in different parts of the pupil seems interrupted at some points, yet there can be no doubt of their being continuous, although from their extremely minute size they can be distinguished only where enlarged and clustering altogether. Posterior to the red vessels seen in the capsule, there appears in some cases a net-work of more delicate vessels, which seem to be seated in the lens itself. The larger trunks of this net-work are not always derived from the circumference of the lens, but evidently come, says Professor Walther, from its posterior surface, directly forwards, and then divide into branches. The presence of these vessels in the lens he has repeatedly and distinctly observed. He states that they present one of the most beautiful phenomena, and that the only things which come near them are the finest injections of the choroid, such as those which are in the possession of Soemmerring, and have been represented by him in his work on the Anatomy of the Eye." It may be stated that this formation of new vessels in the inflammation of the so-called non-vascular parts is just what we find taking place in the properly so-called vascular parts, these vessels being formed contemporaneously with the development or organization of other tissues such as the cellular, in order to bring and carry away the materials concerned in the nutrition and further development of these tissues, and formed from the already vitalized effusions of the organizable materials of the blood.

Inflammation, then, being once established in any of these non-vascular tissues, the different effects of inflammation may rapidly follow. Now the effects of inflammation in a tissue may be divided into two classes, those which are *productive* and those which are *destructive*. As an example (perhaps the most common) of a *productive effect of inflammation* in a tissue, we have the *effusion of coagulable lymph*. This, at its first exudation, is a pellucid liquid which passes through the blood-vessels, especially the capillaries, of the inflamed part; and its most characteristic general properties are, that it is capable of spontaneously organizing itself, even when its external circumstances remain apparently the same; and that thus organized, it may proceed, by development, to the construction of tissues like the natural structures of the body. Examples of coagulable lymph being effused after inflammation of some of these non-vascular tissues in the eye, are not uncommon. Thus, for example, after aquo capsulitis, or inflammation of the cartilaginous membrane which lines the internal surface of the cornea, we find that not only does a muddiness arise from an increase in the quantity of the aqueous humour, but also in severe cases coagulable lymph is effused from the lining membrane of the cornea; and if the iris be at the same time in an inflamed state, this effusion may become the medium of adhesion between the iris and the cornea. Again, in many cases of corneitis, we find that the surface of the cornea has lost its natural polish, and from this circumstance, even when little opacity is pre-

sent, the eye appears dull, and vision is indistinct. And recent observations by Virchow [1] on the effects of inflammation, purposely excited in the cornea by various stimuli, have shown that the changes in it during inflammation are not due to any free exudation of lymph in it, but to alteration in its proper constituent textures. They consist chiefly in swelling and enlargement of its corpuscles, the appearance of minute fatty molecules in them, and the increase and enlargement of their nuclei. The intercellular substance becomes at the same time turbid, more opaque, denser, more fibrous, and sometimes finely granulated; and in some cases fatty molecules appear in it. The changes thus produced in the cornea are not heads essentially different from those that follow its idiopathic inflammations. As a remarkable productive effect of inflammation in these non-vascular tissues, we find that even *ossification* itself is apt to ensue in some of them as an effect of inflammation; some cases of ossification of the cornea, as also many of the crystalline lens and its capsule being on record.

As examples of the *destructive effects of inflammation* we have ulceration and suppuration, examples of the occurrence of which in these non-vascular tissue are, like the examples of the productive effects, by no means uncommon. Thus *ulceration of the cornea* is a disease of no very unfrequent occurrence. These ulcers present themselves under two distinct varieties, superficial and deep. This disease is the common consequence of inflammation of the cornea, but it is frequently produced by the contact of matter in purulent ophthalmia, and the irritation of lime, or by any sharp pointed bodies which may be insinuated into the cornea; or, in fact, any irritating substance, mechanical or chemical, which may be introduced into the eye. The inflammation induces the formation of pus, this breaks and an ulcer is formed. The cause of ulceration is commonly local, the inflammation suspending the normal nutrition of the part and promoting its solution. It is often preceded by induration from the amount of solid deposit, and the ulceration commences in the centre of the induration because the nutrient influence of the vessels is most reduced by the pressure at that spot. But as Dr Williams [2] has remarked (and it is a remark well worthy of attention both in a pathological and in a remedial point of view), a very poor condition of the blood (hypinosis) is often much concerned in determining this result, and seems to be sometimes sufficient to cause ulceration without any distinct previous induration or even inflammation; the parts that suffer being either those which become congested by posture, or those most remote from the nourishing influence of the blood—such as the non-vascular tissues, the cornea, cellular parts of

[1] "Der normale Bau der Cornea und die Pathologischen Abweichungen in Demselben." Wurzburg, 1851. By Fred. Strube—by whom the observations were made, under the superintendence of Virchow.

[2] Williams' Principles of Medicine. London, 1848.

cartilages, etc. In cases of extreme anæmia, when the fibrin and albumen of the blood are very defective, ulcers of this description arise and are to be counteracted by measures the very opposite to antiphlogistic. A similar result was found by Magendie to ensue in animals fed on sugar, starch, and other non-azotized articles of food. In these instances, the ulceration and destruction of textures may be referred to prevalence of the solvent power excited by the oxygen of the blood on materials whose vital power of resistance is much reduced, and for which there is no renovating supply in the plasma of the blood.

But besides ulceration, *suppuration as a destructive effect of inflammation* is also exemplified in these non-vascular tissues. The best example of this perhaps is the disease called onyx (from ονυξ, the nail) or abscess of the cornea. It is an immediate consequence of severe inflammation of the anterior textures of the eye. On the authority of Dr M'Kenzie, onyx is apt to be the result of different ophthalmiæ, and under the use of the remedies most applicable to the particular ophthalmia in which it originates onyx is frequently removed by absorption, in the course of a few days, or even hours. But in neglected cases more and more matter is effused till at length the cornea is completely put on the stretch and looks like an abscess ready to burst. Often in this state the posterior lamellæ give way, and a spurious hypopium is formed, but more frequently ulceration is the result, in the progress of which the cavity containing the pus is opened and the matter slowly discharged. The ulcer which has thus served to open the onyx goes on to penetrate completely through the cornea, so that the aqueous humor is discharged, the iris falls forward into contact with the ulcerated cornea, adhesion between them ensues, and the case ends in staphyloma.

From the rapid survey which we have here taken of the physiology and pathology of the non-vascular tissues, a subject of intense interest to every medical practitioner, but one which (as we suppose) does not receive that attention which is its due, the following conclusions may be drawn :—

1. That *all tissues in the body are nourished by the nutrient liquor sanguinis.* This may be called a self-evident general principle.

2. That *non-vascular tissues differ from the other tissues in the following points :—*

That no blood-vessels circulate through them.

That they are supplied with vital corpuscles which aid the passage through them of the nutrient liquor sanguinis brought to their surface by the blood-vessels circulating around or near them.

And, that these corpuscles are finally resolved into the tissue itself.

3. That *these non-vascular tissues become vascular during disease.*

ARTICLE IV.—*Contributions to Teratology.* By A. MERCER ADAM, M.D., Physician to the Dumfries and Galloway Royal Infirmary, and Member of the Physiological Society of Edinburgh. (*Concluded from page* 240).

NO. VIII.—THE ACEPHALOUS FŒTUS, AND THE PHYSIOLOGY OF ITS CIRCULATION.

Under the term *Acephali*, are ranked all those monstrosities in which the head is entirely absent, and the other parts of the body are more or less imperfectly developed. They are also called *Acardiac;* for in them the heart is invariably wanting. The extent of the co-existing malformations varies in different cases, so that they are divisible into the following five distinct divisions:—*First*, those in which the head alone is absent; *second*, those in which the head and upper extremities are wanting; *third*, those in which the head, thorax, and upper extremities are undeveloped; *fourth*, those in which the fœtus consists of a rounded mass, with rudimentary lower extremities; and *fifth*, those in which there exists only an amorphous mass, possessing no indications of extremities.

Acephali almost always occur in multiple births, and are born along with one or more well-formed children—generally with one only—occasionally with two (Superville, Wernher, Göller, etc.); and in some rare cases with three (Soemmering, etc.) Authentic instances of acephali being born singly are very rare. Photenhaner relates a case; and M. Cazeaux,[1] on the 21st February 1851, read to the Imperial Academy of Medicine of Paris, a letter from M. Leroy d'Etiolles, relating to an acephalus having been singly born. Sulsmann, Duneaud, and Vallisneri, mention similar cases; but in these instances, hydatigenous moles were simultaneously expelled from the uterus.

The Placenta.—In most cases, one common placenta supplies the acephalus, and the accompanying children. Occasionally there exist two placentæ united together; and in some rare cases each fœtus possesses a separate placenta. The latter arrangement was found in only three out of seventy-two cases of this malformation described in Elben's admirable treatise.[2] The placenta, when common to both, may contain two cavities, one for the perfect, the other for the malformed child. Sir Astley Cooper describes such a case, and mentions that each twin had a separate funis, the vessels of which anastomosed freely and distinctly in the placenta.[3] This placental anastomosis of the umbilical vessels of both fœtuses is of very frequent occurrence.

The Umbilical cord of the acephalus varies in length from an inch and a half (Clarke),[4] to 7 or 8 inches (Mayer,[5] Hempel).[6] In some

[1] De Monstr. Acephal. Berlin. 1835.
[2] De Acephalis, etc. Berlin, 1821.
[3] Guy's Hospital Reports, vol. i. p. 227.
[4] Trans. Roy. Soc., Lond., 1793.
[5] Verhandl. der Gesellschaft. für Geburtsh. Berlin. 1846.
[6] De Monstris Acephalis. By C. F. Hempel. Copenhagen. 1850.

cases, as in that of Méry,[1] a single cord, divided into two, is inserted into a common placenta; but ordinarily, each fœtus possesses a distinct and separate funis. The umbilical vessels are often abnormal in number. The cord may contain two arteries and one vein (Tiedemann, Soemmering); one artery and two veins (Busch); or two arteries and two veins (Mayer, Cloquet). It may be attached either to the centre (Clarke), or to the edge of the placenta (Cooper, Hempel). In most cases, it enters the umbilical region of the acephalic fœtus. The two cords are nearly always separate from one another; but Mayer has recorded a case in which they were united.[2]

Osseous System.—Distinct traces of this system are to be found in even the lowest types of acephali. The vertebral column and the ribs exist, in a more or less imperfect condition;—Dr Clarke's[3] being the only case on record in which the spine was entirely absent. The pelvis usually deviates less from the normal type than the other organs, although, in some cases, it is very much deformed.

Muscular and Areolar Tissues.—The muscles are pale, soft, and flabby; and they have been found in a state of fatty degeneration (Schelhammer, Gourraigne). Tiedemann, Odhelius,[4] and others, mention cases in which there existed no true muscular system; but as these physiologists were not in the habit of employing the microscope in their examinations of structures, I consider the accuracy of their observations to be very doubtful. The muscles, however, may be very imperfectly developed, and they are often very irregular in their origins and insertions. The areolar tissue in acephali is very abundant, and it is generally very œdematous from the infiltration of effused serum into its texture.

Nervous System.—In the higher varieties of acephali, the spinal cord, with its afferent and efferent nerves, is often found in a tolerably perfect condition; but in the less highly developed forms of these malformations it is either wanting, or in a very rudimentary condition. The number of its nerves varies according to the amount of its development. Hempel found 25 pair in one case. The defective condition of the cerebro-spinal system in the acephalic, and also in the anencephalic fœtuses, shews embryogenesis to be totally independent of the influence of this portion of the nervous system. Not so as regards the ganglionic system of nerves, which seems indispensable for the growth and nutrition of the fœtus. This is invariably present, even in the most imperfectly developed acephali. In a fœtus of this sort dissected by Sir Astley Cooper,[5] the sympathetic began in a large ganglion, and formed several smaller ones, from which filaments were sent to the aorta. Tiedemann and Soemmering found the hypogastric and lumbar flexuses, supplying branches to the principal arterial trunks. In every case branches

[1] Acad. des Sciences de Paris, 1720. [2] Loc. cit. [3] Loc. cit.
[4] Ephem. Acad. Nat. Curioo, 1690, Dec. 2. [5] Loc. cit.

of the sympathetic follow the course of, and ramify upon the arterial vessels of acephali—a fact of considerable importance in relation to the physiology of their circulation.

Lymphatic System.—The existence of this system in acephali has not as yet been satisfactorily demonstrated. Reasoning analogically, we may believe in its occurrence, since, in a dissection of Munro's,[1] mesenteric and mesocolic glands were discovered.

Genito-Urinary System.—Prochaska's[2] case is the only one on record in which no trace of genital organs could be discovered in an acephalic fœtus. The kidneys are nearly always present, but the supra-renal capsules are often absent. The external genitals may exist while the internal remain undeveloped; and the converse of this may occur, for Atkinson[3] found ovaria and Fallopian tubes present without either uterus or vagina. The uterus is often malformed, and it may be bicornuous (Katzky),[4] arrest of development having occurred at any early period of fœtal life.

Abdominal Viscera.—The stomach is frequently absent; the pancreas, liver, spleen, supra-renal capsules, and thoracic duct, are generally wanting. Schelhammer's[5] seems to be the only case in which the liver and gall-bladder were found in an acephalus. The rectum is occasionally obliterated, but in nearly all cases we find some part of the alimentary canal, and also the internal and external urino-genital organs. How are we to account for the absence of the viscera? Were they never developed, or did they exist until destroyed by some intra-uterine inflammatory action, of which we may regard the serum as evidence? I look upon the serum, effused within the thoracic and abdominal cavities, and infiltrated into the cellular tissue, not as the consequence of inflammation, but as the mechanical result of the sluggish and tardy circulation which necessarily exists in these fœtuses, permitting the liquor sanguinis to exude by exosmose, just as happens in ordinary cases of dropsy arising from some obstruction of the natural current of the blood. I attribute the absence of the viscera solely to defective development. "The connection," says Vrolik,[6] "between the containing and the contained parts, is very distinctly proved by the acephali. The very general presence of the lumbar part of the spine determines the existence of kidneys; that of the pelvis, the existence of the urinary bladder, and the genital organs. The very imperfect condition of the thorax is in relation with the absence of the heart." Although I agree with Vrolik in his hypothesis as to the abdominal viscera, I think his theory concerning the relation between the thorax and its contents is not borne out by facts, for in some acephali the thorax is tolerably perfect, while the heart and lungs are entirely absent.

In the acardiac fœtus we always find more or less trace of the

[1] Edin. Phil. Trans., vol. iii.
[2] Elben. Op. cit.
[3] Phys. and Med. Journ., No. 65. 1804.
[4] Act. Med. Berol., Dec. 1, vol. ix., 1721.
[5] Loc. cit.
[6] Article "Teratology," Cyclop. Anat. and Physiology.

intestinal canal. In the least developed varieties of acephali, the existing portion is generally the lower end of the tube, and this is often attached to a mesentery or meso-colon. In less malformed acephali we find the colon, and in their most perfect varieties, occasionally the small intestines. Malacarne[1] found the intestinal canal ending superiorly in a dilatation, which showed development to have been arrested at the period when a swelling forms on the primitive tube to form the future stomach. In two cases, Tiedemann found it terminating in the substance of the funis, a fact which demonstrated the development of the alimentary canal from the umbilical vesicle.

Physiology of the Circulation in the Acardiac Fœtus.

I have stated that, in the acephalic fœtus, the heart is invariably absent. Gilibert,[2] Katzky,[3] and Vallisneri,[4] have described rudimentary hearts as existing in acephali, but these very exceptional cases were probably merely dilatations of the main arterial tube. Poujol thought that there are no arteries either in the malformed fœtus or in its funis; and Winslow, on the other hand, believed in the existence of arteries only, and no veins. These learned anatomists must have been in error here, for no other investigator has confirmed their observations.

In an acardiac fœtus (of the second class, according to my division), dissected by Dr Munro in 1792, he found that the umbilical vein, on entering the abdomen, divided into various branches, which were sent to all parts of the body. And it was very remarkable that "vessels everywhere accompanied these branches of the umbilical vein, corresponding with them in size, as well as situation, and joining together formed trunks, from which, at the sides of the pelvis, two vessels were continued, one of them on each side of the vesica urinaria and urachus to the umbilicus, which they perforated, and then went along the umbilical cord towards the placenta, resembling the umbilical arteries." Sir Astley Cooper[6] found the funis containing *one* umbilical artery, and a very large umbilical vein. The latter, on entering the abdomen, divided into two vessels, one of which supplied the abdominal and pelvic viscera, and the lower extremities; while the other, passing to the spinal cord, formed a sinus venosus at its upper end. The aorta begun in two branches at the upper part of the spine; the rudimentary subclavian arteries turned on the ribs and supplied the integuments of the upper half of the body. It "*had no communication with the veins but by means of the extreme branches of each.*" At its lower part it divided into two iliac arteries, from the left of which the umbilical artery proceeded, with the accompanying vein, to the placenta.

Ogetti pice inter di Ostetrica. Padua, 1807.
[2] Adversaria. Medic. Pract. Prim., 1791. [3] Loc. cit.
[4] Elben. op. cit. p. 4. [5] Trans. Roy. Soc., vol. iii. p. 217. [6] Loc. cit.

In a case recorded by Dr Houston,[1] the umbilical vein opened into the right external iliac vein. The veins were totally devoid of valves. Branches of the arteries "*arose by fine capillary roots out of every texture in the body. These, coalescing, gradually formed larger trunks, which, when united, constituted a sort of aorta or central vessel*, like the aorta in fishes, on the fore part of the spine. . . . The umbilical arteries took origin from the internal iliacs, and ran to the umbilicus, at which point they entered the cord in the usual manner. . . . No communication existed anywhere between the arteries and veins except at the capillary terminations of the vessels."

These dissections, as well as others described by Hempel, Mayer, and Richard,—which limited space precludes me from quoting,—seem to show that the vascular system in the acephalic fœtus consists of two main vessels, one venous, the other arterial; that the former is continuous with the umbilical vein, and the latter with the umbilical artery; and those both are ultimately united to one another by means of a fine capillary anastomosis. The portal vein, like the liver, is absent.

Many theories have been advanced to explain the physiology of the circulation in the acardiac fœtus. In discussing this vexed question and advancing some hypotheses of my own in explanation of it, I would do so with great deference to the opinions of the many distinguished physiologists who have written on the subject.

I. Winslow[2] and Poujol denied the existence of any circulation whatever. The incorrectness of this view needs no refutation.

II. Mèry[3] and Le Cat[4] imagined the blood to be propelled by the force of the maternal heart. These authors, however, were unaware that no direct interchange of blood takes place between the mother and the fœtus.

III. Dr Young, aware of the frequent anastomosis of the umbilical vessels in the placenta, ingeniously conjectured that the acardiac circulation might be carried on by the propelling power of the heart of the perfect fœtus. This hypothesis was also adopted by Sir Astley Cooper, who supposed it to be effected by an inverted circulation in the funis of the acephalus, and by a placental anastomosis between the umbilical arteries of both fœtuses, by means of which the blood of the perfect child was conveyed *to* the acephalus by the umbilical *artery*, and *from* it back to the placenta by the umbilical *vein*. But it seems highly improbable, not only that the whole circulation should be thus inverted, but that the heart of the perfect child should possess sufficient power to propel the blood, first through the placental anastomosis, next through the blood-vessels of the acardiac fœtus, and, lastly, from them back to itself through the placenta once more. Moreover, were this theory correct, the nor-

[1] Dub. Journ. of Med. Science, vol. x.
[2] Mem. de l'Acad. des Sciences, 1740.
[3] Ibid., 1790.
[4] Philosoph. Trans, vol. lvii.

mal child, in consequence of its blood passing to and from the acardiac fœtus through the placenta without efficient maternal renovation, would be supplied with very impure blood, and we might therefore expect to find it ill-developed, but this is not its condition, for it is generally plump and well-conditioned.

These objections are also applicable to the more recently enunciated theory of Dr Marshall Hall,[1] that the blood is propelled by the agency of the heart of the normal co-twin "through the capillaries of the placenta into the umbilical vein, and through the umbilical vein and a series of capillaries to the aorta."

IV. "May we not venture to conjecture," asks Dr A. Cooper,[2] "that the peristaltic or living muscular power of the arteries is principally subservient to this important end?" Munro, Clarke, Breschet, Lobstein, Tiedemann, Müller, and other physiologists, have been of this opinion; and I shall presently endeavour to prove that the muscular contractility of the vessels plays no important part in the circulation of the acardiac fœtus.

V. Dr Houston[3] regards the placental circulation, in such cases, as normal, but thinks that the course of the blood *through the acephalic fœtus is reversed:* viz., "that that fluid enters by the veins, and returns by the arteries." He considers that the circulation is carried on by the vital powers of attraction inherent in the vessels of the acephalus itself. His views seem to me to be the most correct yet advanced in explanation of this difficult question.

It will be seen that there exists much difference of opinion as to the channels by which the blood is sent to and returns from the acephalic fœtus. Previous to Sir Astley Cooper's time, Breschet, Tiedemann, and Brodie, conceived the circulation in the cord to be inverted, although they did not, like him, attribute the motive power of the circulation to the heart of the co-twin. Munro, long ago, held that the umbilical circulation remained unchanged; and the following circumstances related by Dr Houston seem to demonstrate indisputably that the course of the blood is normal, being conveyed *to* the acephalus *by the vein*, and returned *from* it *by the arteries.* In the acardiac fœtus dissected by him, about an inch from the umbilicus, there lay, in the substance of the cord, a small tumour, which had the effect of rendering *the vein* "dilated, elongated, and tortuous" on its placental aspect, and *the arteries* enlarged between the fœtus and the tumour.

Assuming this to be the order of the circulation in the cord, we have next to inquire what course the blood takes *within* the body of the malformation. "By whatever system of vessels," says Dr Houston, "the blood entered this fœtus, by them it must have been distributed through all the tissues of the body." Seeing, then, that the umbilical vein, in his case, opened into the right external iliac vein, and that all the venous trunks in the body were (as Gurlt

[1] Monthly Journ. of Med. Science. 1843. Vol. iii., p. 544.
[2] Philos. Transac., vol. lxv.
[3] Loc. cit.

affirms is always the case in acephali) *totally devoid of valves*, I think we must, with Dr Houston, conclude that the blood was distributed by the veins to every part of the body, and that it was thence returned to the placenta by the umbilical arteries.

In fine, I am of opinion that, in every case, the blood enters into and is distributed through the body of the acardiac fœtus by means of its venous system. Thence it is conveyed to the main arterial tube or tubes through the medium of the capillary anastomoses which everywhere exist, and by which alone there is communication established between the arteries and the veins; and, finally, from these arteries it is sent once more to the placenta.

Admitting this then to be the course of the intra and extra-fœtal circulation in acephali, the question arises, by what agency is it maintained? I consider it to be dependent not on any single propulsive power, but on a combination of vital forces.

First, I conjecture that *by means of the inherent muscular contractility of the main arterial vessel or vessels of the acardiac fœtus, the blood is propelled from them to the placenta.*

That arteries *are* possessed of muscular contractility has been placed beyond doubt by the experiments of the Webers, but the amount of influence exerted by this force on the circulation of the blood has been very variously estimated. Some physiologists believe that it aids and supplements the propulsive powers of the heart, and others that no action of this kind is habitually exercised, and that if it were developed to any extent it would tend to retard rather than to accelerate the circulation. We know that in the embryo of the chick, before the foundation of a heart, a free circulation can be perceived in the vascular area; and that the arterial tube, from which the heart is afterwards developed, can then be seen actively pulsating and vigorously propelling the blood. This arterial contractility becomes lost, in a great measure, after the development of the perfect heart, there being no longer the same necessity for its existence. Now, in cases where, at an early period of embryonic life, the vascular system becomes arrested in its development, so as to leave only a main arterial tube instead of a perfect heart, may we not legitimately conjecture that the primitive amount of contractility will remain undiminished to carry on the fœtal circulation in lieu of the absent heart? I think such a hypothesis is quite warrantable, and I fancy that the arteries in the adult body may possess, more or less, a latent power of like contractility, which, however, is never developed, on account of there being no necessity for its manifestation. In the acardiac fœtus the absence of the heart necessitates the arteries to take its place and carry on the circulation; and there seems to me nothing improbable in the supposition that the primitive simple arterial tube, from which the heart is later developed, should continue "in cases of arrested growth" to keep up the flow of blood by means of its contractions. In this supposition I am

supported by the weighty authority of Müller,[1] who says in regard to this :—"One point of the arterial stem may have had contractile power, and have thus supplied the place of the heart, which in the embryo at its earliest period had the form of a vessel. If a circulation really did exist, *it could continue any length of time;* and indeed since, in some of these cases, the spinal cord also is deficient, these monsters seem to prove that the circulation of the blood in their double system of vessels can be carried on without the aid of the brain and spinal cord, and consequently that the contractile parts of viscera which are supplied by the sympathetic nerve may be completely independent of the brain and spinal cord." It is remarkable that the sympathetic with its ganglia, the filaments of which probably govern the contractions of the heart and arteries (Valentin having observed them terminating in a plexus in the middle arterial coat),—is never wholly absent in acephali, even in the lowest types of the malformation, where the spinal cord and its nerves are totally wanting.

The nature of the circulation which we have supposed to exist in the acardiac fœtus, resembles that which is really found in some of the lower animals, as in the annelides, etc. In the earth worm, for example, the circulation is carried on by means of "an elongated, simple, pulsatory, median, dorsal artery, provided with distinct muscular fibres,"[2] to which it is again returned by one, two, or more lateral veins. I conceive the main arterial tube of the acardiac fœtus to act like a dorsal vessel of these lower animals, in maintaining the circulation by its own inherent contractility.

Second. The return of the blood from the placenta to the acardiac fœtus is probably effected by *a vital attractive force inherent in the capillary blood-vessels.*

That such a power resides in the capillaries, capable of carrying on the circulation to a certain extent, quite independently of the influence of the action of the heart, seems to me a well-established fact, proved by the following, among other circumstances :—(*a*), in the vascular area of the chick, long before the appearance of a pulsatile vessel, a distinct current of blood-vessels can be observed ; (*b*), when masses of effused coagulable lymph become vascular, the blood can be perceived circulating in the newly-formed vessels which appear in them, long before these new vessels join the contiguous capillaries ; (*c*), the increased vascularity of inflamed parts, and also of the gravid uterus, argue negatively the existence of some such force, inasmuch as they are not dependent on increased action of the heart.

This vital power has been called by Professor Carpenter, "capillary force," and it is probably dependent on two causes ; *first*, on the peculiar vital attractions and repulsions of the animal solids and fluids, as suggested by Professor Alison ; and *second*, on the laws

[1] Physiology, Baly's Translation, vol. i. p. 197.
[2] Grant. Compar. Anat., p. 440.

of exosmose and endosmose as pointed out by Dutrochet, and recently beautifully illustrated by Professor Draper[1] of New York.

The existence of this vital force appears to me to explain satisfactorily the circulation in the capillaries of the human body, and also in those of the acardiac fœtus. In the lungs of the former, in the placenta of the latter, and in all the tissues, of both, this process unceasingly goes on, by exosmose from the blood of the nutritive plasma, and endosmose into it of the effete matters for excretion.

I conjecture that in the acardiac fœtus the circulation is carried on by the two great forces I have mentioned; that by the contractions of the main arterial tubes, and probably also by the vital attractive force of the placental vessels, the blood is conveyed from it to the placenta; and that it is thence returned to the acephalus, and through its veins distributed to all its tissues, by means of this inherent capillary force.

It may be thought that the free inosculation of the umbilical vessels of the acephalus and its co-twin which I have described as often occurring in a common placenta, militated against the hypotheses I have advanced, and strengthens the theory entertained by some that the circulation is carried on by the power of the heart of the perfect child. I have already endeavoured to show that the acephalus cannot be thus nourished, but I would, in conclusion, adduce the following facts which seem to me further to disprove this theory of the acardiac circulation :—

1*st.* Instances are on record of acephali having been singly born. Doubtless such cases are very rare, but I have mentioned several carefully observed instances related by trustworthy authors.

2*d.* The acardiac fœtus may possess a separate placenta. This also occurs but seldom; but that it does occasionally happen is undoubted. Thus, in Munro's case[2] the acephalous had "its own proper membranes and a placenta."

3*d.* The placenta of an acardiac fœtus may be quite normal. In Brodie's case[3] there was one placenta common to the malformed fœtus and its co-twin, but there was nothing unusual in its appearance, and the two cords took their origin from it, "about three inches distant from each other."

4*th.* A placental anastomosis of the umbilical vessels occasionally happens in cases of normal twin-births. In ordinary cases of twins and triplets, each fœtus possesses a distinct placenta; these are often marginally united together; but not unfrequently there is only one common placenta, on the surface of which the umbilical vessels freely inosculate. There are one or two placentæ of this kind in the Museum at Berlin, and there is also a cast of one in Professor Simpson's Obstetrical Collection in the University of Edinburgh. Smellie, Chaussier, and other obstetricians, admit the occasional

[1] On the Forces which produces the Organization of Plants. New York, 1844.

[2] Loc. cit.

[3] Phil. Trans., 1809, p. 161.

occurrences of double twin-placentæ, having in them a vascular communication between the vessels of the cord. Desormeaux, in 1817, exhibited a placenta of this kind to the Faculty of Medicine of Paris, in which the umbilical arteries freely anastomosed. Guillemeau relates a case of twins, in which the placentæ were united *en raquette*, where the umbilical veins anastomosed freely before entering the placentæ, and the umbilical arteries also inosculated. These cases, I think, render it quite evident that a placental inosculation of the umbilical vessels in multiple births does not necessarily prove either that a common circulation exists between the twins, or that the one fœtus is dependent on the other for its nutriment.

5th. A common circulation, instead of aiding, would retard the nutrition of the acephalus. I have already shown that a venosity of the circulating blood would be induced which would be injurious to the growth of both fœtuses. But it can further be proved, on pure mechanical principles, that a common circulation would actually *diminish* the supply of blood sent to the acardiac fœtus. This was first pointed out by Dr Houston,[1] who says, "The suction power of the heart, no matter how feeble it be, will attract to itself the blood received into the capillaries of any veins within the range of its influence, rather than permit it to flow off in a new direction, for the development and growth of another and distinct being; more especially, if the blood-vessels of that being be not, as Dr Marshall Hall's hypothesis teaches, endowed with any innate power of attraction for that fluid. The perfect fœtus would thus, by its heart, take to itself back again all the disposable blood within its reach, and thereby become the instrument of depriving its acardiac twin companion of its supply, rather than of adding to its store."

ART. V.—*Second Case of Excision of the Knee Joint, successfully treated.* By WM. KEITH, M.D., M.R.C.S.E., Senior Surgeon to the Aberdeen Royal Infirmary, and Lecturer on Clinical Surgery.

(*Read before the Garioch Medical Association, 3d Nov.* 1854.)

JOHN REITH, æt. 14; from Old Machar parish; bulky for his age, with a florid complexion, and of a very scrofulous diathesis, having numerous cicatrices of former strumous abscesses on both sides of his neck, both groins, and all round the ankle joint of the right limb; was admitted into Lazarus Ward, under the care of Dr Keith, on the 8th of May 1854, with disease of the right knee joint, occasioning so much suffering as to render him anxious to have the limb, or at least the joint removed.

Physical signs.—The right extremity is wasted, the leg drawn up and fixedly bent at a right angle to the thigh. The knee is greatly enlarged, tender to the touch, and incapable of being moved in any degree, the attempt causing much suffering; the skin over the whole limb, leg, knee, and thigh, is so dry and scurfy, as to present very much the appearance of ichthyosis; there are two openings in the ham, one on each side of the popliteal space, from which

[1] Loc. cit.

a sero-purulent fluid is constantly discharged, and both openings communicate with the knee joint.

History.—He states that seven years ago, when jumping from a dyke, he sprained his right ankle joint; this was followed by a succession of abscesses, the last of which only healed up at the end of three years of constant suffering. Continuing well and at school for two years and a half, he met with an accident eighteen months ago, which, bruising the knee, was followed by much pain and swelling of the joint. With little done deserving the name of treatment, six months of confinement to bed elapsed, when abscess from within the joint found exit by one of the openings in the ham, after which he was so much relieved as to be able to go out on crutches, his knee bent up as it is at present. The opening being very imperfect, a fresh collection of matter formed soon after, and burst at the opposite side of the ham, and these for twelve months have continued to discharge daily, the joint all the time much enlarged, tender to the touch, and more or less painful on movement up to the present time. Pulse never under 120.

On consultation it was agreed to excise the joint.

Wednesday, 17th May, 10.15 A.M.—Under chloroform; the joint was opened by the semilunar incision of the late lamented Dr Richard Mackenzie. In lifting up the flap so formed, osseous union, partly accomplished between the femur and patella, had to be broken up. 1 inch and $\frac{5}{8}$ths was sawn off the femur, and $\frac{3}{8}$ths of an inch from the tibia, making in whole 2 inches of bone removed. The surface of the patella was pared and smoothed, and the gelatinous, degenerated, synovial membrane peeled and dissected from the face of the flap. Three small vessels were tied, the limb straightened, and the wound closed by six stitches, was dressed with ointment, compresses, and a many-tailed bandage, and laid into a well-fitting Macintyre's metal fracture-frame. After being put to bed 20 drops of laudanum were administered. To have arrowroot and brandy for dinner. 9 P.M.—Stomach irritable, vomited the arrowroot, retains port wine and water. Limb lies comfortably, feels the frame press hard on the outside of the knee, which was at once remedied by a soft pad of lint. It added also to his comfort to have the heel set entirely free from pressure, accomplished by passing a long thickish pad under the tendo-Achilles. To repeat the anodyne at bedtime.

May 18th, Thursday, 1st day, 7 A.M.—Has passed a quiet night and slept a good deal. Tongue clean and moist; pulse 130. Ate half a biscuit with relish early this morning; desires porridge to breakfast—allowed; to have soup with chicken for dinner, and 2 oz. of port wine. 8 P.M.—The limb quite easy. The stomach flatulent and irritable. Colic pains in the abdomen. Apply a mustard cataplasm, and give half an ounce of aromatic mixture occasionally.

May 19th, Friday, 2d day, 7 A.M.—Passed a quiet night; much sleep, and no anodyne given; pulse 120. Feels well to-day and ready for his breakfast. 10 A.M.—Removed all the dressings from the knee and renewed the whole. The wound adherent throughout; no discharge to speak of. The limb feels comfortable. 8 P.M.—Comfortable throughout the day; pulse 120; tongue clean and moist; bowels not moved since Wednesday morning. Give 5 gr. of compound rhubarb pill to-night.

May 20th, Saturday, 3d day, 7 A.M.—Slept well; no opiate; bowels have moved freely; limb quite easy; an erysipelatous blush on his right cheek; his pulse frequent, on all occasions above 130. Paint the cheek with strong mercurial ointment, and to have 20 drops of tincture of sesqui-chloride of iron every four hours regularly. 10 P.M.—A comfortable day. Redness on right cheek, spreading across the root of the nose. Continue mercurial ointment and tincture of iron.

May 21st, Sunday, 4th day, 10 A.M.—Slept well; no opiate; no thirst; pulse 132; tongue clean and moist; both the cheeks now red and swollen; the limb easy; dressed it to-day, and removed three stitches; the wound only partially

united; the old openings in the ham discharging pus freely, of a healthy character. 9 P.M.—Easy; bowels open; appetite good.

May 22d, Monday, 5th day, 10 A.M.—Slept soundly towards morning, restless prior to that, but not thirsty or heated; bowels moved naturally at midnight; the face less red and swollen; continue the iron and ointment. 8 P.M.—Dressed the wound, it looks healthy; pus ripe, but rather copious from the ham.

May 23d, Tuesday, 6th day, 10 A.M.—Slept soundly since 2 A.M.; bowels opened at 8; took a good breakfast, and feels better; tongue clean; pulse 120; there is still a slight flush on his cheeks; continue iron and ointment; dressed the wound, it looks well.

May 24th, Wednesday, 7th day.—As at last report. Dressed the wound; the discharge of pus considerable; continue the medicines, and 3 oz. of port wine daily.

May 25th, Thursday, 8th day —Passed a restless night until 2 o'clock, when he obtained ease by having the bandages loosened; dressed the limb, and detecting a sinus burrowing along the inside of the knee, laid it open and pressed out its contents; a poultice to be applied to that part only.

May 26th, Friday, 9th day.—Slept well; erysipelas going; the skin of his face desquamating; removed the last stitch, and the three ligatures; wound looks well.

May 27th, Saturday, 10th day.—Slept well; wound granulating kindly; discharge moderate; re-adjusted the limb, and supported the frame on a wooden platform; bowels regular; secretions black from the effects of the iron; the skin peeling off his face; to intermit the iron and mercurial ointment.

May 28th, Sunday, 11th day.—Was pained for some hours by the heel resting on a pad or small cushion, which hindered rest, slept towards morning; re-adjusted the whole limb; it lies straight; the wooden platform steadies the frame admirably. The abscess in the ham is daily syringed out, it still yields pus copiously.

May 29th, Monday, 12th day.—Has passed the most comfortable night of any since the day of operation. Appetite good; tongue clean; bowels regular; pulse 120. The wound looks well, and the discharge is moderate. Meat allowed; continue wine.

May 30th, Tuesday, 13th day.—Passed a quiet night; bowels moved at 4 A.M. Complains to-day of a sense of heat at and under the head of the fibula of the right leg; fluctuation was detected along the line of that bone, without any redness or other sign of recent action; an incision, four inches in length, was made along the whole extent of an abscess, and at least 2 oz. of thick pus escaped.

May 31st, Wednesday, 14th day.—Slept pretty well; bowels regular. The abscess in the ham still discharges, and still accumulates a considerable quantity of pus daily.

June 1st, Thursday, 15th day.—Made a free counter opening above the remains of the inner condyle of the femur, into the abscess which has so long existed in and around the popliteal space. Bowels open; appetite good.

June 2d, Friday, 16th day.—Slept well; feels comfortable.

June 3d, Saturday, 17th day.—Doing well; the discharge from the abscesses moderate; the wound along the front of the joint is to-day united throughout its whole extent. Meat continued; wine increased to 4 oz. daily; a scruple of phosphate of lime in combination with 5 grs. of sugar, to be given morning and evening.

June 4th, Sunday, 18th day.—Feels well; allowed a pint of porter in addition to his wine.

June 10th, Saturday, 24th day.—Doing well in every way. He was for the first time lifted out of bed with his limb still in the fracture frame.

June 13th, Tuesday, 27th day —A sense of deep fluctuation at the upper and

outer edge of the patella. A needle knife inserted has given vent to a teaspoonful of ripe pus. The incision has been extended to a full half inch, and the cavity stuffed with lint soaked in turpentine liniment. The bowels rather loose; stop the porter.

June 18*th*, *Sunday*, 32*d day*.—Going on well; bowels regular. The wound of the operation firmly united. Porter allowed again.

June 21*st*, *Wednesday*, 35*th day*.—Feels as if he could lift his leg by his own effort, but is not allowed to try. He has no pain whatever on its being lifted out of the frame while being dressed.

July 1*st*, *Saturday*, 45*th day*.—The joint looks very natural in shape and appearance. The skin of the whole limb is now healthy, from the daily inunction of citrine ointment. The health and appetite are good; continue meat and wine, porter and phosphate of lime.

July 20*th*, *Thursday*, 64*th day*.—Going on well. A sinus still open on the outside of the joint, occasioned by the last superficial abscess; an opening and counter-opening are kept open by a small seton tape, run along the sinus. He feels strong and well.

August 8*th*, *Tuesday*, 83*d day*.—He can now lift his limb out of the frame, supporting the thigh with his own hands; the union at the joint thus bearing the weight of the lower limb, and this without exciting any pain. Health good; appetite keen.

August 14*th*, *Monday*, 89*th day*.—Feels well. Removed the small seton. The old abscess in the ham having still the depth of an inch, was laid freely open with the knife, and stuffed with lint and turpentine.

August 25*th*, *Friday*, 100*th day*.—Was out of bed yesterday, and with the aid of staves walked to the ward door; the first attempt he has yet made.

Sept. 1*st*, *Friday*, 106*th day*.—Opened a small collection of pus at the inner edge of the tibia, on a line with the tubercle which has for four days been gathering, a large teaspoonful of thick healthy pus escaped.

Sept. 4*th*, *Monday*, 110*th day*.—Well and comfortable; the wounds all but dried up.

Sept. 9*th*, *Saturday*, 115*th day*.—A mere ooze escapes from pores where abscesses once existed. The bones of the thigh and leg are firmly united by anchylosis. He is out and going about every fine day, and was dismissed from the hospital cured, 115 days after excision of the joint.

Sept. 25*th*.—Saw him at his own home. He complains of pain in the front of the thigh, just above the patella; the spot is red, tumid, and tender to the touch, presenting every appearance of an approaching abscess; made a free incision into it, and ordered poultice.

Sept. 28*th*.—Redness, swelling, and pain gone, a simple wound only remaining, and it is granulating.

Oct. 5*th*.—The wound healed.

Oct. 30*th*, 166*th day*.—He walked to the hospital, a distance of nearly a mile. The limb is all but straight, the joint stiffly anchylosed, he lifts the limb about with freedom, and that without the slightest uneasiness. While walking he rests on the toes with confidence. The heel wants $2\frac{1}{4}$ inches to touch the ground when he stands erect with the pelvis squared, showing the height of heel which his boot will require to enable him to walk comfortably. The whole limb is plump, and he himself florid and healthy to look at. He has grown sensibly in height and stoutness since the operation, and he congratulates himself much on having saved his limb, whether it keep pace with his growth or not—a point that can be settled satisfactorily only after some years' observation.

Remarks.—No worse subject than the above John Reith could well have been selected for any major operation. A constitution aturated with struma, weakened by long suffering, and his body wasted by copious purulent discharges. The knee joint disorganised,

and the soft parts in the ham permeated in various directions by sinuses constantly distilling pus, yet on the complete removal of the one great source of irritation, viz., the diseased knee joint, the constitution was enabled, by the aid of a generous diet, to rally, to re-establish healthy action, and repair every breach.

To the remarks appended to my former case, published in the *Monthly Journal of Medical Science* for April 1854, p. 329, I would only add, that while performing the operation, it were well to clear away from the face of the flap the whole of the synovial membrane; healthy it cannot be, but whether healthy or not, bony union is desiderated, and any secreting surface of a serous character will retard, and cannot facilitate either adhesive union of soft parts, or *ossification of bones.*

Secondly. Much attention should be given to the direction of the saw when cutting through the femur and tibia, that these bones be cut on planes parallel to each other, else they cannot meet face to face when the limb is fairly extended. All important as it is for early union that cohesion should commence from the hour they come into contact, the line of section in the femur should be close to the edge of the articulating cartilage of the inner condyle, which will remove all the articular surface likely to be implicated in disease, and still leave a breadth of bony surface about equal to the cut surface of the tibia. The more nearly of one and equal extent the better for present union and for after use. I must again repeat, the patella is indispensable, if after strength or comfort is desired, and that no portion of flap should ever be pared off or lost in any way. Skin is always useful, and when even superfluous, is wonderfully retractile and accommodating.

In conclusion, it may be safely affirmed that the nature of John Reith's constitution, and the state of the soft parts around the knee, were alike unpromising prior to the operation; yet on the seat of disease and the source of constitutional irritation being removed, nature's powers were speedily called forth, and have accomplished healthy and solid union of bone to bone, so that the patient could walk with little assistance on the 99th day after excision of the joint. The present opportunity may be taken to state, that John Hay, the subject of the former operation, is at this date skin-whole and sound, walking nimbly on a stiff straight limb, miles in a day, without the aid of staff or cane.

ARTICLE VI.—*Cases of Pure Congestive Fever* improperly termed Cholera. By CHARLES WILLIAM GRAHAM, M.D., of Dalkeith, Surgeon Edinburgh County Militia.

CASE I.—Mary Wilson, æt. 16, of sound constitution and ruddy complexion, after taking her usual supper (porridge), went to bed on the evening of the 28th October 1848. At three o'clock the following morning I was summoned to attend her. She was an hour, previously to this, awoke by severe spasms of

the abdomen, accompanied with retching and vomiting, which still continued; —the appearance of the evacuations from the stomach was whitish, her countenance was sunk, of an earthy appearance, and with a livid areola encircling her eyes, lips livid, skin cool and clammy, great thirst and jactitation, pulse 60, slow. A vein was immediately opened, and blood abstracted to the extent of 14 ounces; while the blood flowed from her arm, her face began to lose the livid appearance, the spasms to subside, and the pulse to rise. Turpentine epithems were assiduously applied to the abdomen, and a pill containing calomel gr. iij. opii. gr. ss., creazoti gtt.; was directed to be given every three hours. When I left her about six o'clock, she was very much relieved, and disposed to sleep. At ten A.M. found that she had slept an hour or two, having had no evacuation either from the bowels or the bladder. Ol. ricini ℥ss. was ordered, which in the course of the day operated well, and the urinary secretion was restored. From this time she continued to improve, and was able for her work a day or two after.

Case II.—James Baptie Taylor, 30th November 1848, æt. 27, has had frequent calls to stool, attended with fits of sickness during the day while engaged at work. At half-past six P.M. I was requested to see him, when I found him in the following state:—Was lying prostrate on his bed, his fingers separate from each other and extended, *joints perfectly rigid and immoveable*, the muscles affected with tetanic spasm, and only those of the abdomen seemed to relax occasionally, eyes sunk and fixed, voice reduced to a whisper, and so weak, as to make him unintelligible, features sunk and of a livid appearance; there was neither vomiting nor purging; breath cold, tongue moist, and somewhat cool; pulse 72, slow, and of good strength. Seeing that there was no time to be lost, I tied up his arm, opened a vein, and when I had abstracted nearly 16 ounces of blood, his pulse became accelerated and stronger. But observing that there was no appearance of the spasms relaxing, and the joints continuing in the same inflexible state, I determined on allowing the blood to flow *until I relaxed the whole system*. This occurred when 35 ounces had been removed; the pulse previous to this became tremulous, and as it were by magic, the joints suddenly regained their flexibility, the muscles their wonted softness, and the power of moving the eye-balls returned. I immediately dashed a quantity of cold water on his face, and gave him a glass of whisky. Vomiting now came on, when he brought up a brownish fluid, with some whitish mucus floating on the surface of it. His pulse was much improved by the action of vomiting, and I again repeated the spirits, which he retained. Hot turpentine epithems were applied to the abdomen and lower extremities, which were renewed every ten minutes; and when this had been continued for nearly an hour, he showed symptoms of uneasiness, and began to complain of the pain occasioned by their application, his voice having somewhat returned. The following pills were then ordered, calomel ℥ss. gum. opii. gr. iss. creazoti gtt. iij., met. in pil. iij. divide. Before leaving him I gave him one, along with ℥ij. spt. œtheris nitrosi in water, and desired another to be given in the course of the night with the same draught. Near to nine o'clock P.M. he made a little water, spoke with a loud voice, and his pulse then was of good strength. I left him lying on his right side, with an inclination to sleep. December 1st, ten A.M., slept two hours during the night; complained of general soreness over the body; bowels not moved, and has made no water since last night; pulse natural; castor oil ℥j.; beef-tea for diet; vespere; has passed several very bilious motions, and has made water freely. Afterwards he progressed daily towards recovery. I should mention, that this man was of dissipated habits; and shortly before my first visit to him, his wife had given him some cayenne pepper and whisky, which may account for the evacuation from the stomach being brownish in appearance.

Of the efficacy of the *early and free use of the lancet* at the onset

of this disease, when the pulse and the constitution will stand it, I cannot speak in terms too highly. Without its use in these cases, in a few hours the disease would have terminated fatally, despite all the medicines I could have given. As the lancet is our sheet-anchor in inflammation, so I verily believe it to be, in the disease *improperly termed spasmodic cholera*, which, in my humble opinion, is a fever of a highly congestive character. When I first saw Baptie, I could liken him to nothing else than a stiffened animated corpse, and thought I could be of no service to him. Had I contented myself with 16 or 18 oz. of blood, he must soon have died; but, being well aware, that in the whole list of antispasmodics, there was none equal to blood-letting, I resolved in pushing it to approaching *deliquium animi*, and on reaching this point the victory was gained, and my patient saved.

Part Second.

REVIEWS.

1. *On Gout: its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M.D. 3d Edit. London, 1854. Pp. 400.
2. *Practical Observations on Gout, and its Complications.* By T. SPENCER WELLS, Fellow of the Royal College of Surgeons of England. London, 1854. Pp. 288.

1. THE work of Dr Gairdner presents many characteristics which must render its perusal a gratification to the reflecting physician. It appears to us easy to gather from it, that it is the production of an acute, thoughtful, and experienced practitioner; while its details seem embued with a pervading spirit of ardour and honesty, which claims from us a liberal acquiescence in their general accuracy and truth. The shrewd observer of man's moral and physical nature reveals himself in not a few of the remarks; an agreeable tincture of learning relieves us from the fear that we have to do with one whose own comparatively narrow sphere of thought and action represents to him the universe; while a clear and forcible style confers upon him the power of mirroring distinctly in other men's minds, those thoughts which he has conceived in his own.

Dr Gairdner offers his opinions, as less the result of reading than of observation and reflection, and as such, in a large degree, we are willing to accept them. We must naturally except from this, however, much of the five considerable chapters which he devotes to a review of the theory of gout, and the gouty diathesis, in as far, at least, as these can make pretensions to a real basis of observation. During the recent second revival of the attempt to explain the phenomena of life and disease on strictly chemical principles, it is

still but too manifest that, with every inclination to acknowledge and admire the vast progress and high position of chemical science, and the genius of its cultivators, we must charge it with assuming too unqualified a certainty for its deductions; and must reserve admissions which it seeks too frequently to extort from us, by the absoluteness with which it founds upon a hypothesis, or asserts a dogma. If the living masters of this distinguished branch of human knowledge be entitled to our utmost respect, we have an equal homage to render to not a few of those whom we have not long lost; and we think that medicine would suffer little, if greater attention were habitually paid to the modest confession of Sir Humphry Davy, whose eminence entitled him to be modest, that "in physics and in chemistry, the science of dead matter, we possess many facts and a few principles or laws; but whenever the functions of life are considered, though the facts are numerous, yet there is, as yet, scarcely any approach to general laws; and we must usually end where we begin, by confessing our entire ignorance."[1]

Nevertheless, no one can be more ready than ourselves to admit the frequent value of a hypothesis, when used with its legitimate object of giving aim and direction to a course of observation or experiment. With a certain amount of fact, or pretext of fact, for its origin, it gathers other facts around it, and whether these confirm it, and raise it into a principle, or destroy it, and throw it back to nothingness, there is still a gain to science. In the one case, we have made a permanent conquest; in the other, we have acquired possession of new materials, which may be reconstructed into a new hypothesis, to serve once more as our guide in a fresh inquiry, with the possibility of a like or a better result. Thus, at least in skilful hands, whether abiding or falling to pieces, there is continually a progress and a struggling onwards towards truth. But every instrument of good, sometimes almost even in the proportion that it is good, may be converted by an erring hand into an instrument of evil. If we assign at once to a hypothesis the value and dignity of a principle, we must frequently commit that fault which, more than all others, has retarded the growth of knowledge; and bequeath to our successors, what we have inherited so widely ourselves, the task of dispelling error rather than of establishing truth. Dr Prout himself, with a similar spirit to Davy, questions that dependence upon chemical formulæ, which so many adopt now unhesitatingly, and apply to the explanation of the most hidden processes of life, as if they left little farther to be desired; and of somewhat of this daily extending overconfidence, in matters requiring farther investigation, we cannot, notwithstanding an occasional reservation, wholly acquit our author.

Assuredly, we have no wish to reject the aid which chemical science can, and really does, confer upon medicine; but we would willingly pause till it is more secure in its own ultimate facts, and

[1] Consolations of Travel, Dial. iv.

till these are received with more universal consent among its own cultivators, before accepting them as a general or solid foundation for practice. Where they are really facts, and can be demonstrated as such in the living system, and are not mere elaborations of the test tube and crucible, such as are possibly widely distinct from any thing occurring in the chemistry of life, or which, at least, are not proved to be of identical nature, and the proof must be ever exceedingly difficult, it would be as unphilosophical to reject them on the one hand, as it would be to receive them absolutely, or speculations, however specious, founded upon them, on the other. Under the latter view, we must remember that chemistry grows rapidly, and that it would be rash to anticipate its maturity. In but a little while, a leading innovator may venture to set aside as jargon, what is uttered at present as the grave language of science; and may smile upon the defects or mistakes of a Liebig, just as that eminent inquirer may now safely disparage the alchemists who first established a contact between his science and medicine, or the middle school of iatro-chemists, so ably, if only partially, represented by Boerhaave, with its alkaline, acid, ammoniacal, and oily acrimonies, once universally acknowledged, but now only dimly known as half-remembered legends. In this state of transition, and of doubtful issue, our only wise method is to prove all things, and hold by the good.

In an early portion of his treatise, Dr Gairdner alludes, while withholding his full assent, to the opinion of Dr Prout, that gout and struma are frequently, if not always associated, and often gradually run into each other; both being alike the results of malassimilation of the albuminous principle. Fanciful as this opinion may appear on a first examination, we suspect that it rests on a better foundation than many speculations to which a readier faith has been conceded. Canstatt,[1] a sound pathologist, little influenced generally by the transcendental views which possess such charms for a large proportion of his countrymen, advances a similar notion; and, for ourselves, it is something more than a coincidence, that, on referring to our notes, we find only a single case of gout occurring in our practice, and that the least exquisite in its symptoms, which did not present itself in an individual evincing a marked strumous diathesis, either in his own person, or in that of his immediate relatives. Yet three of these individuals were men of stalwart and massive frames, such as would have singled them out for observation in the midst of any community; and one of them, at least, was of active habits, and a follower of field sports. In another case the strumous affection had proceeded so far as to have produced extensive personal deformity.

Introductory to the author's comments upon the premonitory signs of gout, we find the following passage:—

[1] Die Krankheiten des Alters, p. 238.

"It has been frequently remarked, that the first attack of gout occurs, for the most part, in the midst of perfect health. I believe that in this observation there is a very considerable deception. It is quite true that persons of great vigour and apparent health are particularly liable to this disease, and that its first invasion often occurs in the midst of undiminished strength; but that state can hardly be reckoned a condition of health, under which lurk the seeds of a formidable and painful complaint. Persons, however, who are conscious of every aptitude for enjoyment, and who are much occupied with their affairs or their pleasures, are little attentive to the small beginnings, those incipient signs of disorder which are the forerunners of gout, and they are frequently caught by it, as is commonly said, in perfect health. It is of great importance, both to the individual patient and to the proper understanding of the disease, to mark and determine such premonitory symptoms. And this is no easy matter, for we can rarely get from patients, who have no habits of observation or self-examination, an exact account of themselves; seldom, indeed, a reasonable one.

"All definitions of gout affirm, that its external manifestations are preceded by dyspeptic affections of the stomach. This, however, is chiefly true of subsequent attacks of the established disease, and by no means applies to its first invasion, which undoubtedly often happens without any preceding dyspeptic symptoms. Persons, indeed, in whom the first stage of digestion is sound and vigorous, and in whom the assimilating process is also complete and apparently healthy, are singularly liable to gout. The earliest sign of an approaching fit of the gout, to which my attention has been drawn, has been a dull pain in the left side of the chest, accompanied by an inability to lie on that side, and sometimes by fluttering, irregularity, or intermission in the action of the heart. These symptoms often continue for a great length of time, without any perceptible increase. Patients not apt to take alarm about themselves, frequently suffer them in silence, and even forget them."—P. 7.

With nearly the whole of this we are willing to concur. It is now many years since we saw our first case of a first paroxysm of gout; but we have still a distinct remembrance of our surprise, to discover that not only our anamnesis could educe no symptoms which had been the forerunners of the attack, but that the affection presented itself with merely local, though most distinct characters, and was not accompanied, even when fully established, by any prominent, or even perceptible, disorder of the digestive organs. The symptoms referable to the heart we have had occasion to observe; but rather as tokens of the general gouty diathesis, than as the special antecedents of an individual, and least of all, of a first paroxysm. Indeed, so slight were the constitutional symptoms, and so divergent from the patient's pre-conceptions, that, we recollect, on several occasions, to have been met with a look of incredulity when we first announced the nature of the attack; although a short interval sufficed to establish the conviction in the mind of the sufferer, as completely as in our own. Eczema, with other cutaneous affections, we have encountered as most troublesome accompaniments of the gouty habit. Dr Gairdner has observed hæmatemesis to be cured by a regular fit of the disease. It has never been our own good fortune to see a male patient restored to sound health, after an attack of hæmatemesis, under any circumstances. So far is this from being the case with females, where the hæmorrhage may be merely vicarious of the catamenia, or connected with their

cessation, and therefore less dependent on important organic lesions, that in the course of a practice of upwards of a quarter of a century, we have only seen one example which proved fatal. We are almost inclined to claim for the female the same immunity with reference to profuse hæmoptysis; which, even in phthisis, is certainly in the male a far more frequent cause of sudden extinction.

In his 2d and 3d chapters Dr Gairdner proceeds to a description of the different species of gout, and to the natural history of its stages, terminations, sequelæ, and varieties. He differs from most, if not from all, observers, by stating that he has distinctly witnessed, more than once, a fit of regular painful gout in infants at the breast. The urine he states is scanty and high coloured, and on standing throws down a more or less copious deposit, which is ordinarily affirmed to consist entirely of urates. This, however, he regards as unquestionably an error; and considers not only that he has seen the close of gout marked by a free discharge of phosphates, but that these salts appear fitfully, showing themselves and vanishing again in a few hours. Notwithstanding the latter circumstance, he has no hesitation in regarding them, quite as much as the urates, in the light of a critical evacuation. It may be difficult to mark a precise pathological limit between gout and rheumatism, and with the views of some observers with reference to gout, we believe it to be impossible; but certainly the presence of the phosphates in the urine has been very generally regarded as more especially characteristic of rheumatism than of the kindred disorder. Of the usually recognised varieties of irregular gout, he is inclined to give a place to the atonic only; which he justly describes as first evincing itself by an affection of the stomach, but as making its chief impression on the nervous system. The following are the outlines with which the author portrays this form of the disorder:—

"The patient is the victim of every kind of vague and inexplicable suffering; pain is the least part of his distress; there is a general sense of uneasiness, sometimes referred to one part, sometimes to another, without constancy, regularity, or rule, which keeps him in continual alarm. He takes the gravest view of his situation, and, when his physician can detect no sign indicating the least risk, he prepares himself for some sudden attack of dangerous disease; and talks to his friends of his approaching death. But though pain is a small part of his complaint, it is often considerable. Hemicrania or megrims beset him; pains of the scalp and occiput torment him; stitches in the side, and difficulty of breathing simulating the distress of true asthma or bronchitis, with copious mucous expectoration, afflict him; and these symptoms often go on for a great length of time without proceeding to any local indication of the disease which is really the cause of the disturbance, so as to leave it a mere matter of conjecture and probability that the whole is to be referred to the influence of latent gout in the system. When these symptoms have long distressed him, he is sometimes relieved by a regular fit of the gout. But the instances of atonic gout which never ripen into a fit, and from which patients suffer very miserably during a long life, are extremely numerous; nor is the relief which follows a paroxysm at all commensurate with that obtained in the regular gout. Not unfrequently, indeed, the sufferings of the patient appear aggravated by the fit."—P. 58.

With the exception of the statement, that a certain class of cases is "extremely numerous," there is nothing in this description which does not appear to us to reconcile itself at once with the results of a sound observation. But when authors speak peremptorily of latent and misplaced gout, giving rise to sudden and fatal affections of the great centres of life, without the primary disease having ever appeared with its characteristic features, we must be permitted to hint our belief, that this is but a throwing loose of the reins to the imagination, or a mere reiteration of a dogma originally so founded, which has been the privileged inheritance of all writers on the subject, since a remote period of its history. Even with reference to certain of the more ordinary constitutional attendants of gout, if the disease is known to arise without such symptoms or complications, and if the like symptoms or supposed complications can exist without the disease ever following in a recognizable form, it may be unphilosophical to deny absolutely their mutual dependence, but it must be still more so positively to assert it. And when still more than this is demanded of us, and the most serious results are attempted to be explained through the action of some latent mischief, which has neither been substantially proved to exist, nor to be adequate for the effect if it really did exist, we feel that, let it proceed upon what authority it may, we are required to attach faith to something which is too vague for science, and which is but casting a thin film over our ignorance, and defects in diagnosis. Unquestionably, gout confers no immunity from all other ills, and still less is it a patent for immortality; but not every thing that occurs in the gouty habit exists because of the gout, and is the effect of the gout; and, least of all, can we legitimately trace to such an origin those diseases, the perfectly independent existence of which, in a large class of instances, is a matter of daily observation, merely because they chance to be associated, in a rarer class of instances, in the same individual. Besides, in many of these diseases, should the gout become manifest along with them, its appearance frequently brings no alleviation, and sometimes even exacerbates the mischief. It is to this chimera of latent gout, that the judicious and largely experienced Heberden points,[1] as inducing errors which draw many others after them; and we are glad to cite his authority, to which we may join that of Andral, both because each is widely esteemed, and because there is something soothing to one's indolence in this description of thinking with other men's faculties. But there is always a risk of a fictitious prominence being given to a subject, by those who devote to it a special attention. We have a curious illustration of this one-sided pathology in the terms "phosphatic diathesis," "uric acid," and "oxalic acid diathesis," which have been transferred from the laboratories of the chemical physicians to the field of general observation. Although the principal deposit here, is in each only one of a class of symptoms, and

[1] Commentarii de Morborum Historia et Curatione, cap. ix.

from its very essence, as an excretion, must be a result, rather than a cause or a mode of action, still its existence has been converted into a motive for assigning a name to a general condition of the system, of which it is only a single and an eventual indication, to the setting aside of others of more essential value and importance. Knowing little, yet believing much, for faith is something different from knowledge, he leaps easily to his conclusions, who, merely because he finds such or such an ingredient in the urine, imputes to it at once, as essence or basis, the existence of an ailment, or accepts it as the type of a modification of constitutional disorder.

The 4th, 5th, and 6th chapters of the treatise are nearly exclusively devoted to a consideration of the theory of the disease; the last of them being occupied with speculations regarding the existence of sugar in the urine. In his investigations on this point, Dr Gairdner found himself embarrassed by the uncertain results of the well known test of Trommer, the application of which he afterwards advantageously modified by adopting the proportions of "a single drop of saturated solution of sulphate of copper, and 20 minims of saturated liquor potassæ," added to a drachm of urine, and boiling them together, as usual, in the flame of a spirit-lamp. Objections to Trommer's test have been suggested by other pathological chemists, and among these by Kletzinsky, whose early skill and ardour in chemical investigation promise to bring distinction on his riper years. Hence he has offered a modification, which has been largely employed by other distinguished inquirers, and which consists in combining a drachm of crystallised sulphate of copper, dissolved in the smallest possible proportion of water, with a drachm and a half of glycerine, and two drachms of caustic potass in saturated solution, and boiling together. After cooling, the sulphate of potass is separated, and the fluid is diluted with a portion of water, and set apart for use. The results of this formula are said to present great uniformity, so that by it the risk may be avoided of encountering opposing statements regarding the occurrence of sugar in the urine of epileptics, of the aged, etc., which has been recently averred by one set of inquirers, and contradicted by others. The experiments of M. Alvaro Reynoso, regarding the presence of sugar in the urine of persons who have been narcotized by chloroform, and upon which Dr Gairdner seems inclined to rely somewhat as sustaining his own views, are precisely among those which have been held to be invalidated through the defective quality of the test. Feuchtersleben[1] relates a story of Marcus Herz, who, finding a non-professional acquaintance engaged in his habitual occupation of reading medical books, and copying prescriptions to be employed upon himself, addressed him,—Dear friend, some day or other you will perish of an error of the press! To some of the more enthusiastic votaries of the application of chemistry to therapeutics, we

[1] Zur Diätetik der Seele (6th Edition), p. 110

might hint the caution, that they should take heed lest a patient should perish from a flaw in a formula. But to Dr Gairdner we need offer no such warning, for every page of his treatise proves that he is too able, too considerate, and too conscientious a practitioner to base his views of practice on grounds so unstable.

Dr Gairdner "cannot look on the disappearance of urea and uric acid in the urine, and their accumulation in the blood, as any thing else than a very frequent symptom and consequence of gout, itself again being the cause of other important phenomena." It is venous congestion which he regards as the first condition essential to the formation of the gouty diathesis. We may select the following passage as presenting a summary of his views:—

"This hyperæmic condition of the viscera, and loaded state of the blood-vessels, is very unfavourable to secretion. The functions of the body are imperfectly performed under such oppression. The consequences are a diminished flow of bile and loaded bowels; suppression of the matters usually evacuated by the kidneys; the disappearance of the unctuous moisture which bedews the interstices of the fingers, toes, and arm-pits; and a rapid addition to the general plethora. The heart now feels the unusual load, and by palpitation and struggle seeks to rid itself of the incumbrance; the veins become more turgid, the foot grows tender, the valves lose their office, and the capillaries must sustain unaided the whole weight of the superincumbent column of blood. If this state be not checked, a paroxysm of the disease soon occurs. This termination is sometimes promoted by standing on the affected limb, sometimes by the heat of the bed during sleep. The pain of the fit continues unabated till the swelling and œdema begin; in fact, till the strained and distended vessels have relieved themselves by the extravasation of their fluids. Patients even observe that the relief of the pain is coetaneous with the appearance of œdema, and the return of moisture to the skin; the swelling, so long as it is dry, hard, and elastic, affording no relief. At the same time, the solid matters of the urine begin to reappear in that secretion, and the bowels, which have hitherto required the strongest cathartics to move them, are now readily opened by the gentlest aperients; the flow of bile, in fact, is re-established. This, I believe, is the true *ratio signorum* of a fit of the gout."—P. 161.

In what has evidently been a large experience, Dr Gairdner has observed the gouty to have a singular immunity from other constitutional diseases, such as tubercle, cancer, and scrofula, and to such a degree as to justify the vulgar observation, that the gout prevents them. We have already stated what has been the result of our own observations with regard to scrofula; and in again scanning over the narrower field of experience in gout, which usually falls to the lot of a Scottish physician, we are compelled to record a very different event also with reference to cancer. Rare as cases of cancer of the mediastinum, and medullary sarcoma of the lung, undoubtedly are, it has been our fortune to have seen an example of each, in individuals liable to gout; one of whom had been repeatedly under our treatment for paroxysms of that disorder. In the former of these cases, there was found, in the midst of a mass of scirrhus, a deposit, the size of a walnut, of gritty, calcareous matter, of the consistence of mortar prepared for

building; of which, however, there was no analysis. The latter was a signal instance of the rapidity with which this dreadful disease may develop itself. A gentleman complained, after sitting in a cold apartment, when dining out, of a dull dragging pain in the left shoulder. More than a fortnight after this he was able to be out, and engaged in his usual pursuits. Not quite a month after, he began to experience difficulty in deglutition, followed gradually by orthopnœa, aphonia, and frequent faintings; and at the close of three weeks he expired, being little more than two months from his first seizure. The progress of the stethoscopic signs marked the extension of the disorder. On dissection, a tumour was found behind the upper part of the sternum, in part of almost scirrhous hardness, and containing a calcareous deposit, but chiefly of encephaloid substance. The latter extended over two-thirds of the right lung, transforming and replacing its tissues. There was a distinct encephaloid tumour in the left lung. In a third instance, a person, who had been long of a gouty habit, perished from epithelial cancer of the face. Certainly, in none of these cases, were we inclined to attribute the cancerous affections to the gout. On the contrary, we should have felt ourselves justified as much in ascribing the gout to the constitutional deterioration arising from the latent taint of the cancer; esteeming it quite as sound logic to include the less within the greater, as the greater within the less. But the more profoundly these topics are investigated, the larger is the number of elements, and of modifications of elements, which force themselves into our calculations; with a complexity of impression and of action so wondrous, and so far beyond all powers of arithmetical statement, that we are provoked to smile at those active dreamers, but remiss thinkers, who hope to see medicine, or, at least, surgery, associated with the exact sciences. As a curious example of the conflicting statements regarding one set of causes, we may mention that, while many maintain that excesses in Venus predispose powerfully to the disease, others refer its origin to the retention of the spermatic fluid within the system; to both of whom a third class stands in opposition, by showing that the disease occurs in eunuchs.

With the author's views as to the "plethoric and hæmorrhagic tendency in gout," it is not to be wondered at, that he should suggest the employment of small bleedings, as, at least, a useful preliminary to other remedies; but we think he is scarcely justified in stating, without more express limitation, that it is the only curative means which Sydenham does not condemn. The qualified admission of Sydenham only permits phlebotomy in the young, and in him only when "ab immodica potatione excalefactus;" and in the beginning of a paroxysm, but not in subsequent paroxysms.[1] Neither does Heberden, whose authority is also cited, give it a decided approval; for, though he admits that it is not always dangerous, it is with the

[1] Opera Universa Medica (Ed. Kühn), p. 395.

assertion that it is for the most part useless.[1] Following Sydenham, however, whose views of the history and treatment of gout may be recognised everywhere, as imbuing those of all subsequent writers, Heberden warns us, in cases of metastasis of gout upon the brain, lungs, or stomach, though, with reference to the lungs, with less absoluteness than Sydenham, that, where we are in doubt as to the existence of inflammation, it will be far more pernicious to omit the bleeding if it be really inflammation, than to resort to it if it be merely gout. But he makes the important remark, that this alleged transference of gout is exceedingly rare in the earlier stages, and rare even in the inveterate disease. For ourselves, we are quite as ready as our author to admit the connection of venous congestion with gout, but we have been accustomed to consider this not so much in the light of a real plethora, as of a predominance of the venous circulation, in which the veins, through a specialty of structure, original or acquired, present a greater than due capacity with reference to the arteries, giving rise to a languid circulation. This state, considered in itself, does not appear to be capable of being remedied by blood-letting; though circumstances may arise, where it is dominant, demanding blood-letting. Inflammation associated with it will be modified, firstly by particularities belonging to itself, and secondly by those having reference to the tissue affected; for we need scarcely state, that in different tissues, inflammation, while preserving its general characters, presents different forms and different tendencies. One of these forms, thus doubly modified, we believe to occur in gout; and there seems grounds for judging that it is the elastic and serous tissues and expansions, or those functions which sustain their integrity, which are chiefly affected. We have, therefore, not hesitated to apply moderate local depletion in gout, where the local congestion, and general vigour of the system appeared to warrant it; and we have resorted to the lancet in apoplexy, in the eminently gouty habit, with the same confidence as that with which we should have applied the ordinary rules for adopting or dispensing with this method of treatment under other circumstances, and in neither case have we seen reason to regret our decision. The experienced Hufeland admits of forms of acute gout, in which local and general blood-letting may both be requisite.[2]

The remarks upon treatment, by Dr Gairdner, are exceedingly judicious, though not uninfluenced by fears as to the early exhibition of colchicum, which, we think, savour somewhat of a preconception, and notions as to its narcotic nature, which seem scarcely tenable; but on these our limits forbid us to enter. We have perhaps already been somewhat too desultory in our observations. If so, we are inclined to cast the blame of this, in part, upon our author, with whom a particular train of thought is apt to reappear at intervals, though possibly with no greater amount of repetition than is con-

[1] Commentarii de Morborum Historia et Curatione (Ed. Soemmering), p. 37.
[2] Enchiridion Medicum (6te Auflage), p. 377.

sistent with the privilege and practice of skilful rhetoricians, who revert to the essence, while varying the terms, of what they desire to impress chiefly on their hearers. Unquestionably we should have been more concise, were it not that an able and suggestive writer is sure to provoke comment, as an unavoidable homage to his genius.

2. When we opened the volume of Mr Spencer Wells, and found ourselves at once engaged in the perusal of a series of truisms regarding the processes of general nutrition, we confess that we were unpleasantly reminded at first of the method of the old monkish chroniclers, who usually judged it necessary to usher in the history of their own secluded convents, by each prefixing a brief sketch of the annals of the world from the period of the creation. We soon discovered, however, that the object was more definite than we had anticipated; and that the well-ordered sequence of fact and observation was made to tell effectively upon the history of the disease under consideration. Upon the whole, we regard the treatise of Mr Wells as exceedingly meritorious; and we have been particularly gratified by the perusal of the second chapter, in which he considers gout as modified by rheumatism, and which includes observations of high practical value. As to that upon the connection of syphilis with gout, we fear that it depends largely upon mere speculation, and that its points of diagnosis cannot be placed upon any safe basis. There are pathologists who have affected to trace half the known catalogue of diseases to an original taint or mingling of syphilis; such idealities easily bending themselves to every suggestion, and the cloud becoming a camel, a weasel, or a whale, as the preconception moulds it. The succeeding chapter, on the morbid anatomy of gout, contains some important pathological remarks; and we refer, as an example, though by no means on the score of its novelty, to his warning against too rash inferences from the presence of oxalate of lime in the urine, into which its introduction may be merely casual, from the use of a variety of articles of ordinary diet, the whole of which he has by no means enumerated.

But while we thus generally and gladly record our high appreciation of the treatise of Mr Wells, there are yet portions of it which we cannot regard with unmingled satisfaction. We have no wish to dwell upon such errors as appear (p. 42) in his statement of the results of the experiments of Lavoisier and Sequin on the pulmonary and cutaneous transpirations, or in his introducing inaccurately, on the authority of M. Ferrus,[1] under the morbid anatomy of acute gout, what he repeats accurately, on the same authority, with reference to the chronic disease, because these evidently are merely inadvertences; but there is, here and there, an indulgence, if not a leaning, towards certain forms of charlatanry, which we think is unworthy of his position, and which we would willingly see excluded

[1] Dictionnaire de Médecine, t. x., p. 321.

from every work on scientific medicine. His would be a narrow view of the practice of the healing art, who confined it to the exhibition of remedial drugs, and disregarded the powerful aid of hygienic influences; and assuredly none of its masters, and we trust few of their disciples, have so contracted its limits. Whether it be the application of water, or of exercise, to the preservation of health, the treatment of disease, or the security of convalescence, there is not one of our great writers who is not rich in precepts, and often enthusiastic in commendation, with respect to the value of such assistance. But these men were too deeply instructed in the great scheme of creation, to imagine that any individual agent could be devoted to universal aims; and they assigned to each its place, and to hygienic influences their just, and a conspicuous place, in their methods of treatment. Where, then, is the necessity of forsaking these august guides, to quote a Swedish fencing-master on percussion and friction, and passive motion, who, with his early followers, Branting and Liedbeck, has arrogated for his methods the most preposterous virtues, and not as adjuvant to legitimate medicine, but as wholly superseding it? We may cite, from our own older medicine, as good a case, and a better authority, than any that the so-called kinesipathy can supply, and we offer it as affording a double lesson in percussion and acupuncture: "I knew," says Fuller, "a hardy labouring man, who, happening to be seiz'd with a violent pain in his hip, for two or three nights, as soon as he came to bed, kept beating his bare hip with a bedstaff a great while together, before he could get any rest, and by that means blunted the pain and tired himself into sleep; though afterwards he removed both the pain and the cause, by running a pack needle himself thro' part of his hip."[1]

Then, with reference to certain uses of water, why should Mr Wells venture to allege, that, if not previously unknown, they were at least generally neglected by the profession, and still remain so to a very considerable and, perhaps, blameable degree? Towards a century and a half ago, there was no medical book in wider circulation than the Dispensatory of Dr Quincy, who thus expresses himself: "What a delightful sight is it to a person of humanity and tenderness towards his fellow-creature, to see the number of crutches, and other artificial aids of a cripple, hang up, as certificates of the benefit so many poor wretches have had from the bathing only in cold water."[2] This does not speak to any ignorance of the management of water; which was well understood, for all reasonable aims, long before that time, and has not been forgotten since. Now, let us see what Dr Gully, the ablest of the water-cure seceders, reports of the success of his system in gout, in brief and emphatic, and, apparently, in very honest terms: "In short, truth obliges me to state, that not one case of gout has been *cured* at Malvern, either in Mr

[1] Dr Fuller's Medicina Gymnastica (5th Ed. 1718), p. 42.

[2] The Compleat English Dispensatory, p. 238.

Wilson's or my own practice."[1] And in Sandberg's account of the practice at the hydropathic establishment of Marienberg, near Boppard,[2] we find that of fourteen cases of gout, only three are claimed as cures. Neither have we the slightest warrant that these cures would prove permanent, nor would we accept as ample even that test of permanence which Dr Gully proposes. What right, then, has Mr Wells to crown these men with honours which they do not claim for themselves, and to admonish us to throw aside our prejudices, and adopt their methods? And no sooner have we turned from kinesipathy and hydropathy, when the unreal shadows of homœopathy appear to loom faintly in the distance. Mr Wells finds a single grain of the iodide of potass, divided into three doses, and given with one drop of the tincture of the flowers of colchicum three times a day, to be a remedy which has "very considerable power in modifying the chemical changes which take place during the assimilation of nutriment and metamorphosis of tissue in the gouty, and in so modifying them as to prevent the formation of those compounds of lithic acid, which, by accumulating in the blood, constitute the essential element of the disease." Well might the older writers style colchicum the *anima articulorum*, if, in such doses, such be its energetic virtues!

We make these remarks with reluctance, because, had it not been for their necessity, the tenour of our little comment would have been wholly laudatory. Mr Wells proclaims his adhesion to the ranks of "young physic." For us this term has no definite meaning, however justly respected may be some of the names who have not sought to repudiate it. We know of no "old physic," nor even of any which has reached its maturity. But we know of one, and we venerate it as a disciple, which is fresh in its vigour, and, though finite in its objects, has been infinite in its labours; which, embarrassed as it is through the instability of its subject-matters, has yet in itself, and its accessory sciences, accumulated a larger store of well-ascertained facts than belongs to any other department of human knowledge; which, in its beneficent sphere of duty, can be justly challenged by no other as inferior in its mastery over the intellectual and material world, and is weak only before Omnipotence; which commits to us the treasures of a vast and prolonged experience, often dimmed and interrupted, yet still progressive; and joining in each of us, in as far as possible, the extended art with the brief existence, has added the knowledge of twice ten centuries to the acquisitions of an individual life. Of any other youth than this we know nothing but its greenness.

[1] On the Water Cure, 3d Ed., p. 272.
[2] Norsk Magazin for Lægevidenskaben, b. v., p. 261.

Part Third.

PERISCOPE.

PATHOLOGY AND PRACTICE OF PHYSIC.

PROFESSOR L. W. RITTER VON MAUTHNER OF VIENNA ON INFANTILE ANÆMIA.

The author considers the subject of anæmia to be of immense importance, and quotes the words of Professor Richter of Dresden, "Poverty of the blood being, next to tuberculosis and cancer, the increasing evil of the age we live in, deserves our most earnest attention, inasmuch as it must infallibly lead to a deterioration of the European races." Valentin attributes most neuralgic affections to anæmia; and Trousseau has shown the great influence which chlorosis has on the diseases of women. Anæmia in the mother is reproduced in the child; but infantile anæmia is not always congenital, for it may result from an over-rapidity of growth and development. Congenital anæmia seems, at first sight, opposed to what we know of the vascularity of the infant at birth. Valentin having shown that the quantity of blood in a newly born child is proportionally greater in amount than at a later period of life; for while a child, weighing from 5 to $6\frac{1}{4}$ lbs., has nearly 2 lbs. of blood; the vital fluid contained in an adult of 30 years old, weighing from 130 to 138 lbs., amounts only to about 31 lbs., or constitutes barely 1-5th of the whole weight of the body. But congenital anæmia is explained by the fact that in infancy, as in old age, the watery constituents of the blood are more considerable in amount than in middle-aged persons.

Congenital anæmia is occasioned by general debility of the mother, which is a fruitful source of abortions. It is also a frequent result of imperfect nutrition during pregnancy, especially among the poorer classes, in whom the vital powers are greatly depressed by want and care. The younger children of such families are generally those which are affected by congenital anæmia,—the worn-out mother being unable to afford them due nourishment, and the father being unable to afford a wet nurse. In twins, anæmia generally affects one or both of the children. Suckling a child during pregnancy, as well as all sanguineous or mucous discharges during gestation, renders the fœtus in utero anæmic. The child begotten by an aged or diseased father is commonly impoverished in its blood. Syphilis also, by exercising some obscure change on the blood, occasions anæmia, a fact which, from its frequency, is of immense importance as regards the present generation. It not unfrequently happens that when a syphilitic infant is vaccinated, a peculiar affection of the glands occurs, resulting from a second poisoning of the blood. This congenital syphilis is evidenced by excoriations of the mouth, nares, anus, and vulva; by a tendency to skin diseases, and by a peculiar thin, smooth, and transparent condition of the epidermis.

Anæmia may supervene at various periods of development, either during teething, or at the approach of puberty (chlorosis.)

Nasse has shown, by experiments on animals, that animal diet renders the blood more coagulable than vegetable aliment, and that it increases the number and development of the blood-globules. Saccharine and farinaceous diets increase the quantity of the lymph-plasma, but they do not further the development of blood-globules. A purely vegetable diet is therefore quite unsuitable for infancy; and it is counter-indicated by the anatomical structure of the intestinal canal at that period of life; the cœcum, wherein vegetable matters are digested, being then very imperfectly developed.

Anæmic children are very prone to inflammatory attacks during dentition, owing to a violent reaction in the circulatory system. Venesection is very

injurious in such cases. Another affection to which anæmic infants are liable at this period, is hemorrhage from the congested vessels of the intestinal mucous membrane.

"When the plastic power of the organism," says Canstatt, "is impaired by rapid, natural, and diseased evolutions, tuberculosis is often suddenly developed, and the latent tuberculous matter is deposited in the viscera." This is very apt to occur during dentition, and Mauthner has known well-marked pulmonary phthisis to be thus developed in infants a few weeks old. This is especially apt to occur in the case of children who perspire a great deal, who have mucous discharges, and who have frequent palpitation of the heart.

The physician should ever bear in mind that sudden attacks, even of a fatal nature, may occur as easily, in children, from anæmia as from hyperæmia. Infantile convulsions are often the results of anæmia, and nothing can be more injurious than to treat such cases as inflammatory.—*Journ. für Kinderkrank*, 1854. *Hft.* 7 *and* 8.

M. CULLERIER ON THE RELATIVE TRANSMISSIBILITY OF SYPHILIS BY FATHER AND MOTHER.

The above author has come to the conclusion that syphilis is not transmissible from the father, but is only manifested in the fœtus when the mother has been infected. If we examine rigorously the cases in which the father is supposed to have been the sole cause of hereditary syphilis, we shall find that, at the period of conception, he has transferred the contagion to the mother, who has then directly infected her offspring. Among other illustrative cases M. Cullerier cites the following: A man affected with secondary syphilis married before he was completely cured, his wife immediately became pregnant, and the child which she bore is now eight years old, and the elder brother of two other children, which are in the enjoyment of as vigorous health as itself. In another case, conception occurred simultaneously with the development of secondary syphilis in the father; yet the child which was born is now six years old, and has always enjoyed the most robust health.

M. Vidal has related a case in which a healthy woman, on being united to a man having secondary syphilis, bore a child which immediately died of syphilis. Four years thereafter she married a healthy man, but the child of which he was the father was also syphilitic. Hence Vidal concludes that the semen makes a specific impression on the ovaries, which not only exerts an influence on the products of the primary conception, but also on those of every succeeding one. Cullerier remarks, first of all, that Vidal was able to adduce no proof that the woman, in the case he cited, was free from syphilis. Vidal has adduced instances (similar to those given in Dr Harvey's excellent papers in *The Monthly Journal*, vols. x. xi.), in which the first conception in animals influenced the product of subsequent intercourse. But these Cullerier does not consider of much importance as being *physiological*, not *pathalogical* facts. In fine, he considers that the hereditary nature of syphilis is incontestable, but that it is always due to maternal and never to paternal influence. The development of disease in the infant may occur at all periods of fœtal life, and at all periods of maternal infection, either during the existence of an infectious chancre, during the presence of secondary or tertiary symptoms, during the interval of these constitutional manifestations, or even when the mother appears in the most perfect health. Hereditary syphilis generally manifests itself *during the first year of life*, and Cullerier avers that when the infant of infected parents has passed this period, it may be considered to be exempt from the disease. Many authors have maintained an opposite opinion, and believed in the manifestation of hereditary syphilis late in life. This is merely a convenient doctrine, says our author, devised to shield the compromised morality of certain persons. Thus an old author, Jacques de Béthencourt, who wrote in 1527, said that when he met with syphilis among monks and nuns, he always had the charity to believe that it was due to hereditary transmission!—*Me-*

moires de la Société de Chirurgie, Tome IV.; and Archiv. Gen. de Med. Sept. 1854.

[*Note by Translator.*—M. Cullerier's theory about the transmission of syphilis is a very consoling doctrine, if it be true, for in this country, as in France, the number of infected fathers far surpasses that of syphilitic mothers.]

DR CULLERIER ON THE CONTAGIOUSNESS OF SYPHILIS AMONG NURSES AND FOSTER-CHILDREN.

M. Cullerier, surgeon to the Lourcine Hospital, has recently published a series of observations to prove the correctness of Ricord's views, and to show that secondary syphilis is contagious only by hereditary descent. These observations were made to decide the question of the contagiousness of secondary syphilis among nurses and foster-children; and they may be ranged in two distinct categories. In the first may be placed five cases of infected nurses who did not communicate the disease to the infants they suckled. One of these nurses had pains in the cranium, baldness, roseola, and secondary ulceration of the tonsils, but the breasts were quite intact. In another case there was no cutaneous affection, but there were confluent condylomata (*plaques muqueuses*), in the vulva, and excrescences in the throat, yet the breasts were quite healthy. In the third case confluent roseola extended over the whole body, and over the breasts to the very nipples; condylomata were on the genitals, and similar lesions were at the commissure of the buccal lips. The fourth nurse had syphilitic lichen on several parts of her body, and at the base of one of the nipples an ulcerated condyloma, involving a great part of the areola. In the fifth case the body was covered with pustules of ecthyma, these extended upon the breasts, and at the nipples they were borne, and had long been kept in an ulcerating condition by the suction of the infant. Dr Cullerier considers that there is no proof whatever of milk subsequently or more slowly causing the manifestation of infection.

In the second category are six cases in which infected infants were suckled by healthy nurses, and in which no evil result or infection occurred to the latter. In four of these cases, besides other marks of constitutional syphilis, two of the infants had condylomatous growths on the lips; in a third, a similar excrescence existed on the tongue; and a fourth, had chronic coryza with very abundant secretion. In none of all these four cases, did the nipple of the nurse present the slightest excoriation or redness.

M. Cullerier considers that these eleven cases, conscientiously observed and impartially recorded, demonstrate the immunity of secondary syphilis among nurses and infants. In conclusion, he cites several cases in which symptoms occasioned by direct inoculation from an indurated chancre, were erroneously set down as instances of the contagiousness of secondary syphilis.—*L'Union Médicale*, 1854.

I.—TREATMENT OF ECZEMA OF THE NIPPLES. PROFESSOR VELPEAU.

Eczema may be present on the breasts without making its appearance on any other part of the body. When this occurs medical treatment should at once be had recourse to, as it is apt to ulcerate the nipples seriously if neglected too long. For topical applications in such cases, Velpeau prefers the following pommades:—

1. Cold cream of roses, 30 grammes. Bicarbonate of soda, or sulphate of lime, 50 centigrammes.—*Misce.*

2. White salve (*Cerat blanc à l'eau*), 30 grammes. White precipitate (*Hydrarg. Precip. Alb.*), 4 grammes. Camphor, 20 centigrammes.—*Misce.*

He advises the crusts to be detached in the first place, by means of linseed-meal cataplasms; and the denuded surface to be then rubbed with one of the foregoing pommades, or with one made with sulphur. Should these means fail, a cure is nearly always accomplished by pencilling over the surface with

the solid nitrous argenti, and repeating this every alternate day. Baths, either simple, sulphuretted, or alkaline, greatly facilitate the operations of local remedies. Velpeau also advises the simultaneously internal use of burdock (*arctium lappa*) of dock (*rumex patientia* or *hydrolapathum*), of the saponaria officinalis, and of the solanum dulcamara. The alkaline mineral springs should be drunk when it is practicable.—*Gaz. des Hopitaux*, 29th Aug. 1854.

M. BEHIER'S CASE OF HYDRO-PERICARDIUM TREATED BY PARACENTESIS.

Eugenie Viel, æt. 22, admitted to the hospital on 30th January ; states that for the last three years she has had a pleuritic attack each winter. Complains of considerable dyspnœa; respiration frequent, difficult, and interrupted by cough ; voice very feeble. Has had a gradually increasing sense of oppression about the chest, which commenced with rigors, followed by feverishness. Left side of chest dull on percussion, both anteriorly and posteriorly ; respiratory sounds inaudible in it, with the exception of some sonorous râles at the apex. Right side everywhere resonant ; mucous râles audible over whole lung. Expectoration purulent ; pulse 106, small, irregular and intermitting. Impulse of heart tumultuous; organ displaced to right side ; apex beats beneath sternum ; cardiac sounds loudest at right margin of this bone. *Blisters to be applied to the chest, anteriorly and posteriorly ; and to take expectorant mixtures.*

2*d Feb.*—Respiration more difficult ; extreme anxiety ; pulse very frequent, irregular, and thready. M. Behier resolved on thoracentesis, and having placed the patient in the supine posture, supported by pillows, he introduced a trocar (having a bag of gold-beaters' skin attached to the canula) between the 7th and 8th ribs, at a place which would be crossed by a vertical line drawn from the external border of the nipple. No fluid escaped, but the entrance of the trocar seemed arrested by a hard resisting tissue. There was no bleeding save from the external wound. A second puncture was made higher up, and more in front, in the 6th intercostal space, immediately below the nipple. The instrument was inserted obliquely inwards and backwards, and the stilette having been withdrawn, a serous fluid, slightly tinged with blood, flowed slowly through the canula, until about 300 grammes had escaped. The canula, while *in situ*, was observed to oscillate synchronously with the systolic movements of the heart, and the hand, when placed upon it, could distinctly feel the cardial impulse. The canula having been withdrawn, the external wound was closed by means of adhesive plaster. The operation relieved the patient ; and the breathing became much easier after it.

3*d Feb.*—Vesicular murmur audible in left lung, before and behind. Heart has resumed its normal position ; pulse 104 ; inspirations 26 ; no pain nor local inflammation. *Ordered a laxative.*

4*th Feb.*—Rested ill last night; considerable oppression; heart still in normal site ; pulse 102.

Shortly after this bronchitis supervened, but it was arrested, and on the 22d the patient was pretty well. On the 25th the uneasiness returned ; subcrepitant râles were heard, and the sputa became pneumonic. She died on the 28th.

Post-mortem.—Right pleura contained reddish serum. Left also contained serous effusion, and exhibited traces of old adhesions. The pericardium contained 100 grammes of yellowish transparent serum. It was so firmly adherent, by false membranes, to the pleuræ, that the place of puncture could not be discovered, although it was very carefully examined. On the first occasion the trocar had penetrated the false membranes.—*Revue Médico-Chirurgicale. August* 1854.

BERGERET ON THE COMPOSITION OF PUS.

The above author recently laid a memoir before the Academy of Sciences of Paris, in which he endeavoured to prove that pus was composed of a liquid, containing animalcules which he designates *pyozoaires*, which are found either

1st, floating free in the fluid; or, 2d, enclosed within membranous vesicles (globules). The free pyozoaires continue to move about in the pus for three or four days, and after their death they are found united together into small masses, by an amorphous matter. The animalcules of the globules generally live only for 24 hours, but the duration of their life is subject to many variations. When they cease to move they roll themselves up into a spiral form, and then constitute the nuclei of the globules. Water prolongs the duration of their movements, and occasionally it causes the pyozoaires which have become spiral nuclei, to commence moving again, and to burst the membrane enclosing them. Acetic acid, on the contrary, immediately arrests the movements of the pyozoaires within the globules, and also those of the free animalcules, but much more slowly. M. Bergeret considers the enveloping membrane of the globules to be formed by the mucus suspended in the liquid.—*L'Union Médicale. August* 15, 1854.

SURGERY.

ON THE CURABILITY OF CANCER, AND ITS DIAGNOSIS BY MEANS OF THE MICROSCOPE.

(*Continued from page* 468.)

Meeting of the Imperial Academy of Medicine, 17*th Oct.*

M. Jules Cloquet thought, with M. Velpeau, that the use of the microscope in the diagnosis of cancer, threatened to divert professional attention from clinical observation. The essential nature of cancer—not only of the encephaloid, but of all other varieties—was to reproduce itself with all its primitive characters. He had operated on a great many epithelial cancers of the lips, almost all of which had recurred; and he had removed many fibro-plastic tumours, several of which had returned. Cancers varied from one another only in their degrees of malignancy and fatality. He regarded the melanotic as the most formidable species, and could remember no instance in which he had removed a cancer of this kind without its having recurred. Next in order to it in malignancy he ranked the encephaloid, fibro-cartilaginous, fibrous, colloid, cartilaginous, epithelial, and fibro-plastic varieties. The danger of reproduction varied according as the cancer was encysted or ramified, ulcerated or not so, complicated or not with glandular engorgements, etc. We were constantly finding our prognosis to be fallacious in cases of cancer. In illustration of this he cited several cases in which malignant cancers of the breast of unfavourable prognosis, had been removed, without any recurrence of the disease.

He thought the following conclusions were warranted:—1*st*, That microscopical examinations should never be allowed to supersede clinical observation; but that both should, in every case, be resorted to. 2*d*, That tumours, having all the characters of cancer, could occasionally be removed without recurring, although their prognosis was most unfavourable. 3*d*, That other tumours might return, the circumstances of which were apparently the most favourable. 4*th*, That although there were cases in which an operation seemed clearly indicated, and others in which it was as clearly counter-indicated, there were many others in which the most experienced practitioner could not decide the question with any degree of certainty or satisfaction.

October 24*th.*—M. Robert.—My remarks on this occasion will still continue to be strictly practical in their nature, and I shall endeavour to demonstrate clinically the utility of the distinctions pointed out by the microscope,—in the use of which I confess myself to be by no means perfect, but the employment of which I have several times found to be highly useful for the elucidation of doubtful cases.

First of all, let me allude to M. Velpeau's address. In it he stated that there are statistics on record which are directly opposed to those of Munro, Mayer

Boyer, Macfarlane, etc., which I quoted. He alludes to those of Hill and Flajani; but these, in my opinion, are not worthy of being placed in opposition to those which I cited. But even granting that they do counterbalance the statistics I quoted, this does not in the least affect the evidence borne by the experiences of Lebert and Broca. The conviction which I have expressed, therefore, seems to be supported by the opinions both of ancient and of modern surgeons.

The cases brought forward by M. Velpeau from his own private practice are divisible into two classes: 1*st*, Those of a remote date (of the nature of which I have expressed some doubts), and 2*d*, Those of recent times; while the cures were not of more than six, five, three, and two years, at the publication of his *Maladies du Sein*. Those of the *second* class I frankly accept as *true* cases of cancer, having not only been pronounced such by an authority in whom I have every confidence, but having also been demonstrated to be of this nature by means of the microscope. The cases included in the former and more numerous class have not been defended with any great warmth by M. Velpeau. I am still of opinion that M. Velpeau has not yet waited long enough to be able to announce the *radical cure* of those rare cases of *true* cancer in which he has operated. We know that recurrence may take place after a long space of time, and M. Velpeau has himself given two instances of distant recurrence in his work, in one of which it happened at the end of fourteen, and in the other at the end of twelve years. But further, M. Velpeau has detailed four cases, one of which occurred in 1847, the others in 1848 and 1850, in which tumours containing cancer cells were removed, and the patients recorded as cured. Chance has thrown in my way intelligence respecting one of these cases; a lady, who was seen by one of the first surgeons in Paris, and who died last year of a relapse of her malady.

As to the clinical infallibility of M. Velpeau, he grants that there are difficult cases, and when he says that he is never deceived, he means in the most marked cases of cancerous or other tumours. In this respect, however, he is no farther advanced than the rest of us. But in the two extreme phases of the evolution of mammary tumours, for example, our diagnosis is surrounded by almost insuperable difficulties. How can we always distinguish, with certainty, a small encysted mass of an encephaloid nature, which is mobile, unadherent, and which rolls below the fingers, from an incipient hypertrophic tumour? Who has not met with such cases? But let us, on the other hand, suppose the case of an enormous ulcerated tumour, with everted and fungous edges, with profuse fetid suppuration, the hemorrhages from which have undermined the general health,—and all this occurring simultaneously with general cachexia. M. Velpeau has said that he would declare such a tumour to be cancerous, suppose all the microscopes in the world were to testify against him. I deprecate this singular mode of demonstration. Less confident or more inquisitive than M. Velpeau, I appealed to the microscope in a case similar to this which I met in 1849, and it showed me that this horrible tumour was composed only of the hypertrophied mammary tissues. I operated, and the patient has ever since enjoyed perfect health. How precious to us then is a method by which we may rectify our first impressions! No one has a higher estimate of the sagacity of our eminent colleague than myself, but after clinical examination of a morbid tumour with the naked eye, he is often, like ourselves, left in doubt as to its true nature. This I can prove. At the last meeting of the Academy, the members were deeply interested by M. Velpeau exhibiting several tumours which he had removed, and the nature of which he had diagnosed before operating. In these cases examinations with the naked eye and by means of the microscope, testified once more to the ability of our colleague. But one of the specimens puzzled M. Velpeau; and the diagnosis of it then, as well as before the operation, was very obscure. The tumour resembled neither encephaloma, nor schirrus, nor colloid cancer,—in spite of the gelatinous aspect of its tissues,—nor had it much likeness to an adenoid tumour. M. Velpeau re-

garded this doubtful tumour as of benign nature, but added that he would never be surprised to hear of the ultimate appearance of a similar tumour elsewhere. Thus, out of six specimens, taken at random, he is unable to determine the nature of one, by ordinary means. In his embarrassment he consults two microscopists, one of whom finds cancer cells, and the other discovers none; and M. Velpeau, as much in doubt as ever, doubts the efficacy of the microscope. Had I been present on that occasion, I confess that my convictions would have been strongly shaken. But I have since then been fortunate enough to obtain accurate and precise information respecting this tumour, from the most competent authorities.

(Here M. Robert read notes, which he had received from MM. Robin, Verneuil, and Houel, which showed that these three able microscopists, after having severally demonstrated identically the same structures in this tumour, had unanimously concluded that it contained no cancerous elements, and that it was merely a particular variety of mammary hypertrophy.)

Since receiving this information, said M. Robert, I confess that I feel much more convinced of the value of the microscope.

The question before the Academy as to the *curability of cancer*, lacks a proper basis. For, to know whether or not a disease is curable, we must know exactly what this malady is; we must carefully distinguish it from those affections which are liable to be confounded with it; in a word, we *must define it*. Hence, on beginning this discussion, we ought to have set out with a clear definition of what cancer really is,—which task all the orators have evaded. Owing to the difficulty which has hitherto existed in assigning to this malady precise and pathognomonic characters, we have merely recorded its most striking and constant phenomena; that is to say, as a substitute for a dangerour definition, we have given a summary description of the evils which attend the evolution of cancer. This hiatus is unfortunate, inasmuch as it has led, and must always lead, to the greatest confusion. And hence it happens that the word, *cancer*, instead of designating a pathological condition, and its simple varieties, is the qualifying attribute of an immense class of very different maladies. This family is, by common consent, composed of elements so diverse, that in looking to their clinical characters, and keeping their structure altogether in the back-ground, we are obliged to admit (as has been done by MM. Leblanc, Velpeau, and Cloquet), the existence of a great many genera, which are themselves sub-divisible into species—thus rendering the classification of limits quite indeterminate and arbitrary.

But, gentlemen, our venerable instructor, Bichat, long ago laid down the following law, which has remained immutable amid all the revolutions of science:—When two diseases differ in their symptoms, their course, and their termination, they also differ in their seat, and in the nature of their accompanying lesions; and reciprocally, when the anatomical lesions vary, and when the structure of the pathological products is dissimilar, then the functional disorders which cause their evolution, are distinct in their nature.

Taking this immutable principle, then, for our groundwork in the study of cancerous disorders, if we find that clinical observation does not afford us the elements of satisfactory distinctions, and that their physiological characters are not sufficiently general,—admitting of too many exceptions, and containing too many varieties,—then it behoves us to study their structure. But are the means afforded us by pathological anatomy, in the present day, sufficient for the prosecution of this study? I unhesitatingly say, No. Persevering and laborious investigations of tumours have doubtless produced very useful documents, but they yielded very insignificant results. To these researches we owe that useless, but copious, phraseology which at present encumbers pathology; —such words as colloid, tuberous, ligneous, napiform, chondroid, pustulous, encephaloid, schirrous, ladaceous, medullary, fungus haematodes, cancerous and phagadenic ulcers, noli me tangere, etc. What signifies to us the hardness or softness of a tumour, or the sound it gives when cut, if not decisive?

There can be no doubt that we still need to be possessed of more delicate characteristics of cancer. But whence are we to get them? Chemical analysis has been unable to give them. It was once hoped that we might gain our end by scraping the surface off a section of a tumour, so as to demonstrate the presence or absence of a particular creamy or lactescent juice. M. Barth attached great importance to this juice, and considered it almost pathognomonic of cancer. But an analogous fluid is found in some tumours which are not at all cancerous in their nature, and these exceptions prove that this test is not by any means conclusive.

Such was the state of science when the microscope came upon the field. It proposed a precise and brief definition for cancer, and declared it to be a malady constituted by the presence of a special anatomical element, which had no analogue in the economy, and the characters of which were easily recognizable by the practised eye. The microscope informs us that *that* only is a cancer, which contains the cancerous element; that this element combines with the various simple and composite tissues of the economy, and that it is from the occurrence of this mixture in different proportions that the innumerable varieties of cancer result. It further declares that there are other morbid tissues which, at first sight, are apt to be confounded with cancer, and that their development in the economy often indicates great danger, and may even occasion death. But instead of being constituted, like cancer, by the presence of an anatomical element, without any analogue in the economy, *i.e.*, heteromorphous in its nature,—these tissues are merely the result of an exaggerated yet circumscribed growth of one or more of the elementary tissues which enter into the normal constitution of the organism.

Finally, I would have you remember that the laborious microscopical investigators have made their clinical observations keep pace with their histological researches. Their practised eyes having discovered, in these morbid tissues, structural peculiarities which had escaped the earlier observers, they proposed an entire revision of this part of our nosology, inasmuch as they found that those diseases structurally resembled others totally distinct from them in their seat, origin, prognosis, etc.

What ought the partisans of the clinical school to do in the case of so notable a deviation from the great precept of Bichat which we have enunciated? We ought either to expose firmly the errors of the new doctrine, if such exist, or to confess frankly and honestly the progress which the microscope has achieved.

I shall now proceed to state to you what I have learned concerning this question, both from my personal observations, and from the attentive perusal of modern works; and if I succeed in demonstrating to you that cancerous, epithelial, and fibro-plastic tumours are not identical affections, I think I will have sufficiently proved the utility of the microscope, which alone can reveal to us precise and minute distinctions.

You are aware that, according to the microscopists, the cancer-cell is the specific characteristic of cancer. I will not pause to discuss, as M. Velpeau has done, the relative value of the cells, the nuclei, and the nucleoli; for, although I have certain convictions of my own on the subject, I think it is better to leave questions like this to those men who have studied them *specially*, and not to meddle with topics about which we know little practically. You will perhaps say that I am adopting the definition of cancer given by the microscopists, totally forgetting that my opponent refuses to accept their opinions. M. Velpeau has said that the cancer-cell cannot be the specific anatomical character of cancer, because it has been found in non-cancerous tumours. I would ask my learned colleague by what authority he declares the tumours which contained the cells in question, not to have been cancerous? Because, he will reply, they did not return after extirpation. From this it would follow that every tumour which does not recur after removal, although it contains myriads of so-called cancer-cells, is not cancer; and from this I would

also be correct in concluding that the so-called cases recorded in M. Velpeau's work, at p. 585, were not cancers at all. And, by carrying this mode of reasoning still further, I could prove that my colleague has not, any more than Munro and Macfarlan, cured a single case of cancer.

The physiological are not less marked than the anatomical characters of cancer. The disease has a constant tendency to become generalised, and manifested elsewhere, either in the neighbouring lymphatics, in some other organ, or in the viscera themselves, especially in the lungs and the liver. Before cancerous tumours ulcerate, and injure the constitution by profuse suppuration and hemorrhages, we often see the signs of general cachexia become manifested. The malady constantly returns after extirpation, and the progress of cancer becomes much more rapidly fatal in exact proportion to the frequency of its recurrences, and the operations for its removal. I have often noticed this fact, but was never more struck by it than in the case of a woman, aged 38, who, after four successive ablations of a small tumour of the mamma, had three recurrences of the malady in less than two years, and who returned to the hospital three months after the fourth operation, where she died from the recurrence of a multitude of similar tumours within the chest.

Fibro-plastic tumours are always circumscribed, and have not the irregular ramified (*rameuse*) form which characterises certain cancers, especially schirrus. On their first appearance they possess a firm, slightly elastic consistence; they are homogenous in structure, and contain no juice. Their tissue is termed by Lebert, *fibro-plastic*, and it is composed of cells, and of fibres which exactly resemble the elements of cellular tissue during their development. These tumours advance but slowly, and they are, for a long time, quite compatible with all the appearances of good health. Nevertheless they may become softened, ulcerated, and even generalised, and they may occasion death by being developed in the viscera. Of these facts M. Velpeau has given us several examples. But even among those rare cases in which the malignity of the fibro-plastic tissue rivals that of true cancer, we find consoling facts, which M. Velpeau ought not to have passed over in silence. Of these facts, which are now very numerous, I shall mention a few.

Paget (in his *Lectures on Tumours*, 1853) mentions a case quoted from Maclagan, of a girl, who, in 1832, was operated on for a firm, elastic, fibrous-looking tumour, in the left lumbar region. It returned after a year, and was again removed in 1834. A second recurrence happened after a period of eighteen months, and the third operation was completely successful; for in 1850—fifteen years after it—she was, and had continued until then, in perfect health.

In 1839 I removed from the right axilla of a young man, a large tumour, the size of both fists, which was circumscribed, lobulated, but firm and elastic, and appeared to be composed of fibrous tissue intermixed with fat. In five years thereafter it returned, and I operated again in October 1844, removing a tumour, the size of my fist, possessing the very same appearance as the first. Again it returned after the lapse of other four years, and for the third time it was extirpated, in January 1849. M. Lebert examined the tumour, and pronounced it to be fibro-plastic tissue mixed with fat. Since this time the patient has enjoyed perfect health.

Again, Mr Syme, in 1839, removed a tumour, situated over the first rib, from a man aged 48. A second operation was necessary in 1843, a third tumour was removed in 1847, and a fourth in 1849, and a fifth in 1851. Professor Bennett examined these tumours microscopically, and found them to be composed of fibro-plastic tissue. Shortly after this fifth operation another tumour, of a considerable size, was formed under the clavicle. During all this time the patient's general health was never impaired. Ultimately this tumour ulcerated; hemorrhage took place; and, worn out by it, the patient died in July 1852. *There was not a trace of the malady to be found in any of the internal organs.*

In September 1849, M. Chassaignac removed a tumour from beneath the left clavicle. In a few months it reappeared, and a second operation was performed in December 1850, a third in July 1851, a fourth in January 1852, a fifth in July the same year, and, finally, a sixth in October 1853. Since that date, M. Chassaignac has not seen the patient. During the course of these operations the general health continued excellent, and there was no implication of any of the lymphatics. The tumours were examined each time by M. Lebert, who pronounced their structure to be purely fibro-plastic, mixed with a little gelatinous matter.

When we reflect on the course of all these fibro-plastic tumours, on the obstinacy of their recurrences, and on the fatal termination of many of them, we certainly cannot regard them as of a benign nature. But, on the other hand, are we correct if we class them with cancerous tumours? Has an encephaloid or a schirrous tumour ever been extirpated for four, five, or six recurrences, without the disease becoming generalised, and the constitution cachectic? And can the instances of fibro-plastic recurrence in the viscera be compared, for frequency of manifestation, to the more constant return of cancer in the glands, and within the cavities of the chest and abdomen?

I would next speak of *epithelial affections*, which are composed anatomically of epithelium cells or of the epidermis which normally covers the skin and mucous membranes. I cannot conceive how M. Velpeau can say that the structure of such tumours (if we are to believe the microscope) renders them akin to benign growths, such as warts, corns, etc. What does it signify, that two tumours have in common a normal anatomical element, if this element be not disposed and combined in the same manner in each? In warts and corns, the epidermis, though more abundant, stands in exactly the same relations to the other parts of the skin as it does in the normal condition of the parts. We will find on examination that this is not the case in an epithelial tumour. M. Velpeau's assimilation then, is more specious than real.

Epithelial tumours appear principally on tegumentary surfaces, and ought to be studied separately according as they are situated on the skin or the mucous membranes. On the skin they begin by small crusts (*boutons croûteux*), warts, or fissures, and they form ulcers called *cancroid*, on account of their differing in appearance from cancer, progressing more slowly, and having less tendency to infect the system. The utmost evil that they do is to affect the neighbouring glands. When treated by energetic appliances, calculated rapidly to destroy the indurated parts, they often become healed definitely and radically. The epithelial tumours of the mucous surfaces (as types of which I shall specify those of the tongue and cervix uteri) are different in their progress; their course is more rapid; they reappear almost invariably after extirpation, and they terminate by causing death. Their tendency to generalization, however, is very far inferior to that of true cancer. If we examine the bodies of those women who have died of these deep ulcers of the cervix, we may often find the uterus partly destroyed; and the bladder, peritoneum, and great blood-vessels may be perforated by the disease. But, in such cases, we never find epithelial masses deposited in the viscera; the utmost traces of the disease which we can discover are the slightly enlarged glands in the neighbourhood of the uterus. It is the same with epithelial ulcers of the tongue; the glands of the neck alone become affected; and when the patients die, it is rather from being exhausted by hemorrhages, and from the long continued deglutition of putrid matters, than from any generalization of the disease having occurred.

At the orifices of the body, where the skin and mucous membrane become blended together, epithelial tumours possess, what I call, *mixed* characters, which they communicate to the complexed structure of the tissues affected. But there, as on the skin, the tumours continue for a long while in the condition of local maladies; and although it may be proved that their extirpation is less frequently curative than of those on the skin, it can also be demonstrated that the radical cures effected by the operation are very highly encouraging.

I am quite aware that epithelial tumours *may* become propagated or reproduced in the lymphatic glands, and even in the viscera ; but such instances of generalization are very rare.

These, then, are the differences, both of the minute structure and the mode of progress, of the three lesions which are generally confounded together under the term—cancer. We cannot consider epithelial and fibro-plastic tumours as benign in their nature, and we certainly cannot, in point of malignity, rank them with cancerous maladies properly so-called ; M. Velpeau does not admit these distinctions, for to him all these lesions are cancers, and he does not recognize the speciality of any of their elements. If the patient *must* sink, "what does it matter," he asks, "in *what manner* he dies,—whether by the disorganization of the diseased parts or from the results of general infection ?" I would answer, in the first place, that all these maladies are not alike in their progress and severity ; but even supposing that they *were* all equally fatal, is it not important to study them, in order to establish the differences between them, and to endeavour, sooner or later, to apply the knowledge thus gained to their scientific treatment ? I feel convinced that, at present, the only question concerning cancer that is at all settled, is that of prognosis. Is it nothing for us to know that although the removal of a cancer leaves the patient exposed to the danger of an almost certain recurrence, sooner or later followed by death,—that the extirpation of an epithelial or fibro-plastic tumour is much more hopeful and satisfactory in its results ? If, in the latter case, a return of the disease should take place, the surgeon may operate again with greater hope and courage, knowing well that a radical cure is not impossible.

Let us, then, study these diseases more and more, and let us endeavour to distinguish them from those maladies with which they are often confounded. Let us strive to learn still more accurately their clinical differences ; and, in doubtful cases, let us always avail ourselves of the aid afforded us by the microscope.

M. Leblanc said that he did not consider, with the microscropists, that cancer did not exist unless the heteromorphous elements called cancer cells were found in the morbid growth. He gave a more extended signification to the term, and included under it all those grave lesions which never healed spontaneously. which became generalised, and which possessed physical characters peculiar to each species of tissue. To determine these characters with certainty, the microscope should, in all cases, be used. Among the lesions which he considered cancerous, were a large number which consisted of the substitution of a homeomorphous, but abnormally placed element, for some other homeomorphous element which was absent at the diseased spot. This was what occurred in epithelial and fibro-plastic growths. They were anomalous accumulations of normal elements occurring, in different proportions, in regions where they ought not to be found in a state of health. The union or simultaneous occurrence of fibro-plastic, epithelial and melanotic tumours, was by no means uncommon. He cited an interesting case, which proved that fibro-plastic tumours may return, either at the same or at a different place ; that these tumours may co-exist in the same individual, with other growths demonstrated by the microscope to be true cancers ; and that each of these varieties possesses a different degree of importance. In every case it was futile to attempt to form a correct diagnosis or prognosis by clinical observation alone, without also appealing to the microscope.

Veterinary pathology presented many good cases illustrating the curability of cancer ; and he could advance many instances in which a radical cure was obtained by operating *early* on animals, while the disease was as yet local and not fully developed. It was the same in human as in veterinary medicine,—the chances of recurrence were materially lessened by an early operation.

M. Barth exhibited to the Academy a tumour which had been removed, by M. Amussat, jun., from the testis of a boy, æt. 11, several of whose relatives had

died of cancerous affections. The tumour had all the appearances of encephaloid cancer, save that no juice could be obtained from it by scraping or compression. One of the most distinguished microscopists had been unable to find any cancer cells in it ; while another gentlemen, equally expert in histological investigations, had demonstrated their presence. M. Barth himself had seen none ; and he therefore regarded the growth as a fibro-plastic tumour of the worst species. It seemed to him probable that the hereditary cancerous taint had, in this boy's case, given a malignant character to what was, naturally, a simple and benign tumour. M. Barth discussed the relative value of the microscopical characteristics of cancer, and remarked, in conclusion, that although the microscope had not revealed cancer cells in this tumour, he did not the less consider it to be a malignant growth. He was quite convinced of the utility of the microscope, but thought we should guard against over-enthusiasm for it, which would lead us to exaggerate its value, and be deceived by its applications.

M. VELPEAU exhibited several tumours which he had recently removed, the nature of which he had diagnosed before their extirpation. "I only exhibit these specimens," said he, "to show that clinical diagnosis is not uncertain, and that when I affirm having seen a tumour, I wish my word to be accepted."

M. GERDY—(with great warmth).—I cannot allow this. I protest strongly against such a pretension (*une semblable prétention*).—(Great excitement).

31*st October*.—M. LARREY did not intend to have taken part in the discussion, but he was surprised to hear so much said, both for and against the microscope, without the cause of the instrument being defended by any of those who had made its use a special study. The microscopists had hitherto abstained from speaking, and it had required all M. Robert's talents to defend the instrument from the imputations cast upon it by M. Velpeau. He possessed no claim to constitute him an efficient partizan of the microscope, but he thought we might as well deny the utility of pathological anatomy as of microscopical observation.

If the cancer cell appeared, to microscopists, the distinctive cancerous element, we ought not to deny that it was so, in spite of certain exceptional cases, and those differences of opinion which were more apparent than real. One objection urged against the use of the microscope was that its employment required an amount of patient investigation, which was incompatible with the exigencies of the profession, and with the acquirements of most medical practitioners. But it was a pity that we should neglect a thing which was really useful, merely because all of us were unable to use it properly. He himself, in doubtful cases, always referred the matter to his microscopical confières. It had been said by others that we had presumed too much upon the value of microscopical appearances. The discrepancies of opinion among microscopists were to be explained in two ways : 1st, They certainly were not infallible in their dicta ; and 2d, the mistakes of the inexperienced histologists had brought discredit on the more able observers. Another reason of their diversities of opinion was, that observers were seldom furnished with *entire* tumours for examination, but merely with fragments of them. Now, if these tumours were of a mixed character, they might easily contain cancerous elements in certain parts, and none in others ; a circumstance which would occasion contradictory results, if they were examined piecemeal. Moreover, it was not fair for surgeons to try to puzzle microscopists by submitting to them morbid structures for examination, without previously informing them anything about their history, course, diagnosis, or other elucidatory circumstances.

As to the curability of cancer, the microscope—although it was not an infallible test—had not been consulted in the majority of the so-called cases of true cancer, said to be radically cured ; hence he entertained strong doubts as to whether cancer was ever curable. One of the most renowned practitioners they had ever known, Marjolin, did not believe in its absolute curability ; he

looked upon every truly cancerous affection as sooner or later fatal by relapse, and believed a radical cure only to be possible in those tumours which were benign, or had been erroneously diagnosed. Could not this distinction, which Marjolin established from practical observations only, be decided with habitual precision by the aid of the microscope? The real value of this instrument consisted in the aid it afforded us in elucidating cases of doubtful, and rectifying those of erroneous diagnosis. M. Larrey cited the details of several interesting cases, in which benign fibrous tumours had been mistaken for cancerous, and showed the advantages of microscopical examination in such cases.

In conclusion, he considered that the academy was not as yet in a position to decide definitely either for or against the utility of the microscope in forming a diagnosis and prognosis of cancer. He proposed that the Academy should hear the cause of the instrument defended before it, by one or more of the microscropists.

M. Malgaigne considered that the microscope had been too much depreciated by M. Velpeau, and too much praised by M. Robert. He thought that the microscope had been of little real service to surgery. He went over, in a rambling manner, the clinical views of cancer which had been held by the older surgeons, as Boyer, Scarpa, Bayle, Laënnec, Astley Cooper and Abernethy. He said that the microscope had made a complete revolution in surgery; he did not admire the instrument, and still less those who used it. If the microscopists had not been kept in check by surgeons, they would have destroyed science altogether.

He thought that there were three kinds of cancer; namely, that containing the characteristic cells; epidermic cancer, and fibro-plastic tumours; and all of these were malignant. M. Velpeau had been very severe on the microscope, yet but for it his work on *Diseases of the Breast* could never have been written, for we there find tumours divided into malignant and benign,—true cancers and fibro-plastic growths. M. Velpeau had said that he could dispense with the microscope. *He* might be able to do so, but M. Malgaigne affirmed that, for the bulk of practitioners, *a scientific diagnosis was impossible without the employment of the microscope.* Those who obstinately refused to employ the microscope appeared to him to resemble a person with imperfect sight, who would not use spectacles, affirming that he could see just as well without them. The question of the curability of cancer was entirely one of correct diagnosis, which could never be made without the microscope.

(*This discussion not having yet terminated in the Academy, its conclusion is necessarily postponed.*)

JOBERT ON THE RADICAL CURE OF HERNIA BY INJECTIONS OF IODINE.

M. Jobert lately presented to the Academy of Medicine three cases of inguinal hernia in which this new mode of treatment had been productive of the best results.

The first case was that of a young man, 18 years old, with complete inguinal hernia of the left side, which, though it did not interfere with his ordinary avocations, rendered him ineligible for the military service. The tumour did not descend to the bottom of the scrotum, and it was separated from the testis by a constricting band. It was easily reducible by taxis, but reappeared upon the patient coughing or assuming the erect posture.

At the patient's request M. Jobert performed the following operation on the 12th May. An incision, 2 centim. in length, having been made over the inguinal canal, a fine trocar was introduced into the sac, through which 5 grammes of pure tincture of iodine were injected. The wound was then closed by means of twisted sutures, and dressed with simple ointment. The patient suffered almost no pain. In the evening there was slight tumefaction and pain of the inguinal region, but no fever or constitutional disturbance.

May 14.—The swelling and redness were considerable. The sutures were removed, and the wound was found imperfectly healed.

In a few days the external wound cicatrised, and the swelling and redness disappeared. The left testicle was not atrophied. On 5th June the patient was able to go about, and to cough without the least inconvenience or reappearance of the tumour. As a precautionary measure he continued to wear a suspensory bandage for some time.

The second case was that of a patient, æt. 34, who entered the Hôtel Dieu on the 18th Nov. 1853, having not only a congenital inguinal hernia, but also a hydrocele of the same side. These were separated from each other by an annular constriction of the tunica vaginalis, which contained the protruded bowel above, and the testicle and sermo fluid below. M. Jobert resolved to obliterate the tunica vaginalis, for the purpose of curing both the hydrocele and the hernia at the same time. He accordingly passed a trocar obliquely into the lower portion of the constricted sac, and evacuated the serosity therein contained ; and then, having previously made pressure with the finger over the inguinal canal to prevent the possibility of there being any communication between the tunica vaginalis and the peritoneum, he injected some tincture of iodine into the tunica vaginalis and the hernial sac. The external wound was finally closed by adhesive plaster. Next day the scrotum was reddened, somewhat tender to touch, and distended with fluid effused from the tunica vaginalis ; but there was no pain or constitutional disturbance. On the 5th Dec. the swelling which had been gradually subsiding, had nearly disappeared ; and on the 12th the scrotum had regained its normal size. The scrotum and the spermatic cord on the affected side were firmer than before ; and a cylindrical cord extended along the whole of the inguinal canal, as well as along the spermatic vessels down to the testicle. The patient was completely cured, and he left the hospital able to walk easily and without fatigue, and without any reappearance of the hernial tumour.

The third case occurred in a young man, aged 27, who had had a large oblique inguinal hernia of the left side, for eight years, for which he had worn all sorts of trusses without benefit. At the desire of the patient, and after a consultation with his colleagues, MM. Rayer, Begin, and Cloquet, M. Jobert punctured the hernial sac, and injected the tincture of iodine into the tunica vaginalis. The patient was laid in the horizontal posture after the operation ; his legs were flexed on his thighs, and maintained in this position by means of a bolster. The hernial sac swelled immediately after the injection, and in fifteen hours it was as large as the hernial tumour, and had a fluctuating, half-solid, feeling. The parts continued thus for eight days, and then the swelling speedily diminished. On the twenty-eighth day after the operation the cure was complete ; the track of the spermatic cord was occupied by a hard and compact cylindrical substance, which completely prevented any return of the hernial protrusion, and allowed the patient to go about without any recurrence of his malady. Four months had elapsed at the time when M. Jobert related his case, and the cure was still as complete as at first ; the inguinal canal was filled with a hard, solid cord ; and there was no atrophy of the testicle.

Remarks.—When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous—M. Jobert recommends puncturing the sac at once with a trocar, without previously making an incision. The trocar is to be inserted perpendicularly at first, then obliquely upwards, and, when it has reached a certain depth, it is to be directed cautiously backwards so as to puncture the sac—the accomplishment of which process is known by the operator no longer meeting with resistance, and by his feeling the canula move about without obstruction in the empty sac. Having entered the sac, the pure tincture of iodine should be injected into it by means of a glass syringe—whereby we may be able to know how much fluid has been used. M. Jobert ordinarily injects one or two spoonfuls (*cuillerées*) of the tincture, but often much less. The quantity injected should always be in proportion to the size of the serous pouch. The injection should be suspended whenever we feel an obstacle to the entrance of the liquid.

Instead of allowing the fluid to escape through the canula, M. Jobert withdraws it by means of the suction of the syringe.

When the sac is thin, moveable, and easily displaced, Jobert exposes it by dividing the integuments with a bistoury, and *thereafter* punctures it with the trocar—*never with the knife*. In such cases he unites the lips of the external wound by means of twisted sutures.

What occurs after the operation? Immediately after the injected fluid is withdrawn, a new liquid begins to be thrown out within the hernial sac. After twenty-four or forty-eight hours have elapsed, the sac is filled with this fluid; it continues in this state for eight days, and after the first week it begins to diminish gradually and visibly in bulk. In proportion as the tumour diminishes it begins to acquire solidity, and becomes as firm as wood; nay, it eventually becomes as hard as bone or ivory.

M. Jobert affirms that this method by injection is superior to all the other operative procedures which have yet been devised for the radical cure of hernia, inasmuch as it is at once simple and harmless. M. Jobert, in conclusion, acknowledges that this operation is not invented, but merely revived, by him; inasmuch as M. Velpeau, in his *Annales de Chirurgie*, published ten years ago, first suggested the likelihood of injections of iodine proving beneficial for the radical cure of reducible hernia.—*Gazette des Hôpitaux*, August 10, 1854.

M. JOBERT'S CASE OF VARICOSE ANEURISM CURED BY INJECTION OF THE PERCHLORIDE OF IRON.

A young man, named Leopold Goëb, æt. 18, consulted M. Jobert on the 20th February, about a small tumour, situated at the bend of the elbow, which presented all the characters of a varicose aneurism. The tumour disappeared wholly on pressure, but only partially upon compression of the brachial artery. It seemed to be formed by the passage of the blood from the brachial artery into the median basilic vein. Its pulsations were synchronous with those of the heart. It was occasioned by venesection.

Having previously compressed the brachial artery, M. Jobert introduced a trocar into the tumour. Venous blood escaped through the canula. Six drops of a solution of perchloride of iron were injected into the sac through the canula, and the external wound was closed by means of adhesive plaster. Next day a coagulum seemed to have formed in the tumour, the pulsations were less vigorous, and the aneurismal *bruit* less distinctly audible. In a day or two, however, the apparent solidification had disappeared, and the pulsations and *bruit* had returned. On the 24th February, M. Jobert repeated the injection; and on withdrawing the compression of the artery, a jet of red blood, mixed with the perchloride, was thrown out. Jobert therefore injected the fluid while the artery was compressed. The tumour solidified, and its pulsations ceased; but violent pain was felt in the forearm, and a sensation of heat extended along the course of the radial and ulnar arteries. The muscles of the forearm were spasmodically contracted, and the patient was feverish and sleepless. These, however, disagreeable symptoms finally abated, and the tumour continued to become harder; and three days after the operation it was quite solid, resistant, and non-pulsating. The brachial artery had become obliterated for about an inch above and below the tumour, and was converted into a solid fibrous cord.

M. Jobert attributes the failure of the first attempt to the perchloride not having been mixed with a sufficient quantity of blood. The coagulum formed should completely obstruct the artery. He believes the constitutional symptoms that occurred to have resulted from arteritis; hence he thinks this method of cure should only be tried in cases where the aneurismal sacs are of recent formation, and are free from inflammation and disease.—*Gazette Méd. de Paris*, 29th July 1854.

CHRONIC HYPERTROPHY AND EXCISION OF TONSILS. M. GUERSANT. (*Hôpital des Enfants Trouvés*).

According to M. Guersant, hypertrophy of the tonsils occurs most frequently

among pale, lymphatic children, who have a delicate skin, and are very susceptible of atmospheric influences. They are subject to frequent attacks of sore throat, and the hypertrophy occurs as the result of these. The swelling often disappears after puberty, hence, when excision is not demanded by the urgency of the symptoms, we should always (except in the case of very young children) wait until this period is passed to see what is effected by nature. Excision is generally indicated when the tonsillar enlargement interferes with important functions, as respiration or deglutition; but it may also be advisable in cases where the affection impedes the child's articulation, or renders it unable to proceed with its education on account of deafness.

M. Guersant condemns the use of the bistoury for the excision of the tonsils, as being both difficult and dangerous; and he disapproves of the employment of scissors, as rendering the operation tedious and laborious. He prefers, for this purpose, the *tonsillotome* of Fahnestoch, or the simpler modification of it recently made by M. Charrière of Paris. In cases where we do not wish to remove the whole of the tonsil, Guersant advises the ablation of a considerable portion of it, to prevent the necessity of a repetition of the operation.

As regards hemorrhage after excision, Guersant says that although in adults it is often abundant or even alarming, yet *in young children he has never seen it occur to any extent.* Probably the vessels are smaller and more retractile in children than in adults. This fact, whatever be its explanation, is one of considerable importance as regards prognosis and subsequent care; for while little children may safely be sent home immediately after the operation, persons beyond puberty should be always kept under surveillance for three or four days. When hemorrhage from the tonsil is about to stop, the patients, in place of pure blood, expectorate bloody mucus. Sometimes sufficient blood is swallowed to provoke vomiting, and it is then ejected at once and *en masse*, to the great terror of the patient.

Treatment of Hemorrhage from Tonsils.—In simple cases the use of cold acid gargles, rest in bed for five or six days, the maintenance of strict silence, and the use of aliments easily swallowed, are quite sufficient to stop the bleeding. If the hemorrhage continues pieces of ice may be held to the bleeding surfaces by means of a pair of dressing forceps, as has been proposed by Chassaignac. Or cold may be applied externally, by means of pounded ice applied to the sides of the neck, and the angles of the jaws. M. Guersant has invented an india-rubber *mentonnière* for this purpose. If these means are insufficient we may apply the actual cautery to the wound, or lay pieces of lint upon it saturated with the solution of the perchloride of iron. When cauterization is done with due precaution there is no danger to be apprehended; a white cicatrix is left, which becomes detached on the fifth or sixth day.—*Gaz. des Hôpitaux*, 15th July 1854.

ACUTE TONSILLITIS. M. CHASSAIGNAC. (*Hôpital Lariboisière*).

M. Cassaignac prefers excision, either complete or partial, of the tonsils to all other methods of treatment in cases of acute cynanche tonsillaris, and affirms that it is always followed by immediate and satisfactory cessation of all the distressing symptoms. The inflammatory condition of the tonsils, instead of counter-indicating the operation, is in his opinion favourable to its success. —*Ibid.* 24th August 1854.

ORCHITIS. M. VELPEAU. (*La Charité*).

(The readers of the *Monthly Journal* are probably aware that it has recently been proposed to apply collodion to the scrotum for the cure of orchitis. M. Bonnafont applied it in 56 cases, and found its employment productive of very great benefit (*vide Bull. Gén. de Thér.* June). M. Velpeau recently alluded to the subject in one of his clinical lectures, and we doubt not that the opinion of this eminent surgeon will be interesting to our readers.—*Trans.*)

During the last twelve months 35 cases of orchitis were treated in M.

Velpeau's *service;* all of which, with the exception of three—were due to blenorrhagia. The right testicle was affected in seventeeen, and the left in fourteen cases. In three cases it is not stated on what side the inflammation occurred. This does not confirm M. Ricard's views as to the greater frequency of the affection in the left testis.

In twenty-one cases there was engorgement of the cord, accompanied by effusion into the tunica vaginalis. These cases were treated by means of small punctures made with the point of the lancet, to relieve the tension of the skin and evacuate the serosity. The patients were also made to rest in bed, and apply water-dressing to the parts. In such cases a cure was generally effected in eighteen days.

In six cases there was no effusion, although the epididymis was considerably enlarged. In these cases rest in bed and purely expectant treatment effected a cure, in a period which averaged, in general, seventeen days.

The remaining eight cases were treated by the external application of *elastic collodion* [1] which was preferred for this purpose to simple collodion on account of its remaining flexible, and not coming off in scales. In all the cases, the patients experienced a slight, but transient pain, on the application of the collodion. The duration of this mode of treatment varied from thirteen to thirty days; the average period was eighteen days—precisely the same as that of those cases which had been treated by means of punctures, rest, and cataplasms. The latter applications were only used in the more severe cases.

M. Velpeau sums up his lecture with the following conclusions as to the collodion treatment: "In the simpler cases, rest, water-dressing and cataplasms, and in the more severe, the addition of punctures, are always sufficient to effect the cure of blenorrhagic orchitis. The collodion treatment is quite harmless and painless, but *it has no appreciable influence on the resolution of blenorrhagia orchitis.*"—*Union Médicale*, August 3d 1854.

M. MAISONNEUVE'S CASE OF ENORMOUS FIBROUS TUMOUR OF NECK, EXTIRPATED BY THE METHOD OF "MORCELLEMENT;" RECOVERY.

On the 8th August, M. Maisonneuve presented to the *Académie de Médicine*, a woman, æt. 35, from whom he had removed a very large fibrous tumour of the neck, which other eminent surgeons had pronounced beyond the reach of art. He extirpated it by the method he terms *morcellement*, which consists in dividing it into pieces. He had first employed this method in the case of large fibrous tumours of the uterus, which were not extricable on account of their enormous size.

The tumour was of two years standing, and had latterly increased in bulk so much as to render suffocation imminent. It occupied the whole of the left side of the neck, and extended vertically from the mastoid process to below the clavicle, and transversely from the spinous processes of the vertebræ to behind the larynx and trachea, which were considerably displaced by it to the right side. The carotid artery and internal jugular vein were visible on its external surface. It was slightly nodulated on its surface, and it had the firm resistant feeling of fibrous texture. The skin glided with ease over its surface; but the tumour could not be moved over the deep-seated textures. It was not painful to touch. The arm was neither painful nor swelled, but retained perfect freedom of motion. M. Maisonneuve diagnosed the existence of a fibrous tumour adherent to the transverse processes of the vertebræ, and he considered that the important vessels and nerves of the neck were not involved in its substance. for two reasons:—1st, The unimpaired condition of the sensory and motory functions of the head and arm; and, 2d, The fibrous character of the growth, and its intimate attachment to the vertebræ. His anatomico-pathological researches had long satisfied him that *tumours which have a periosteal origin always remain isolated and distinct from the soft parts in the midst of which they are developed.* He therefore resolved to attempt its removal, and the poor patient, on learning his decision, wept tears of joy.

Chloroform having been administered, a vertical incision was carried from the mastoid process to the clavicle, and another transversely from the larynx to the spinous processes of the vertebræ. The flaps of this crucial incision were laid down, and the tumour was extirpated by a long, laborious, and most delicate dissection, in the course of which, the carotid, the internal jugular vein, the pneumogastric nerve, and the cervical and brachial plexuses were exposed. On account of the firm adherence of the tumour to the adjacent parts, it was found impossible to extract it *en masse;* M. Maisonneuve therefore resolved to divide it into two equal portions by a transverse incision. This greatly facilitated his operations; and saved the important structures from injury. Portions of the scaleni muscles were unavoidably removed in the dissection. Maisonneuve had to subdivide one of the halves of the tumour, but he succeeded in removing the whole of it, and completing the operation in three-quarters of an hour. There was very little loss of blood, owing to the extreme care taken to avoid wounding large vessels; and the patient was under the influence of chloroform the whole time.

The wound left by the operation was a deep and ghastly excavation, at the bottom of which lay the lower six cervical vertebræ, the first rib, the cervical and brachial plexuses, the larynx, trachea, pharynx, and æsophagus—all exposed to view! M. Maisonneuve, anxious to avoid suppuration and obtain union by the first intention, brought the tissues together by means of adhesive plaster, *serres-fines*, and properly regulated pressure. In three days, four-fifths of the wound had cicatrised, and the remaining portion was filled with healthy granulations. In a month the patient was quite well; the parts around preserved their normal site; the arm had no impairment of its sensibility or motion; the voice was clear, and deglutition was easily performed.

The tumour was purely fibrous, and of a dense hard texture. It weighed 475 grammes (*i.e.*, nearly 8 lbs).—*L'Union Médicale*, 10th August 1854.

ON THE QUANTITY OF SULPHATE OF ATROPINE REQUISITE FOR DILATATION OF THE PUPIL.

AMONGST the narcotics which induce dilatation of the pupil, belladonna occupies the first place. Its best preparation is the sulphate of atropine, which in weak solution has no irritant effect, and is free from that mechanical action which may be objected to the extract of belladonna, while through its uniform composition, it can be applied in precisely regulated strength.

The English were the first to introduce this preparation into practice. In London it is generally used in the proportion of 4 grs. of sulphas atropine, to an ounce of distilled water. A single drop of this, retained in contact with the cornea and conjunctiva for only a few instants, produces, in twenty to twenty-five minutes, a *complete dilatation, with immovability of the pupil.*

Such a dilatation is desirable and even necessary to obviate synechia, synizezis, prolapsus, iridis, etc., and also as preparatory to the operation for cataract, in which the pupil has so great a tendency to contract. It would also be highly advantageous, when it is wished to dilate the pupil, in order to examine the deeper seated parts, the lens, the vitreous humour, the retina, and the choroidea, with the aid of the ophthalmoscope; but there is here an important counter-indication, in the marked disturbance of vision which is temporarily induced by it. Besides the intolerance of light, which annoys some, the seeing of small objects, as in reading, is rendered almost impossible for from four to eight days, in cases where this could be accomplished readily in ordinary states of the pupil, so that most persons complain of it bitterly. In cases of amblyopia also the patient becomes usually less able to distinguish objects during several days; and shows unnecessary alarm lest the instillation should have injured the sight permanently, notwithstanding the forewarning, which I have never neglected, that the effect was merely of a temporary nature.

The objection, thus occasioned, led me to the inquiry, whether it was not possible to fulfil our purpose, without exposing the patient to the inconvenience

of which he thus justly complains. One obvious course was, to employ weaker solutions; and yet I continued for long, like others (it may be said in excuse), to pursue the old routine, and to use in all cases the solution of gr. iv. to the ounce of water. Dr De Ruiter (*Nederlandsch Lancet*, 1854, p. 464) had already stated, that a drop of a solution, in which was contained not more than $\frac{1}{129,600}$ of sulphate of atropine, when kept sometime in contact with the eye of a dog, sufficed to produce a dilatation lasting for twenty hours, Farther experiments on dogs, have shown him, that a solution with a proportion of $\frac{1}{3600}$ of sulphate of atropine, induces powerful dilatation in ten to fifteen minutes, which disappears only at the end of four days; that a solution with $\frac{1}{21,600}$, five to ten minutes in contact with the eye, causes also strong dilatation, and even sometimes immovability: that a solution with $\frac{1}{129,600}$, kept five minutes in contact, gave a good dilatation at the end of an hour, which lasted eighteen hours; that with a threefold dilution, and the same time of application, a perceptible dilatation still followed, and that it was only upon a sixfold dilution, and therefore with $\frac{1}{772,600}$ that the effect became doubtful. The sensitiveness of the eye to atropine, indeed, excites astonishment, when we consider that of the single drop of the attenuated solution, which suffices to produce dilatation, probably not a fiftieth part is absorbed.

At my request, Dr De Ruiter has also investigated the sensibility of the human eye to atropine. It seems to be somewhat smaller than in the dog; yet is so strong that, where it is desired to examine the internal parts of the eye, a much weaker solution than that ordinarily employed is sufficient to produce a good result. I consider it superfluous to communicate the various trials made upon man. It is enough that they have led me to adopt the following solutions.

1st. Of gr. iv. of sulph. atropini to an ounce of distilled water, as preparatory to operations, to prevent threatening synechia, synézizis, or prolapsus iridis, and to increase the capacity of sight in central cataract, or in central opacity of the cornea,etc.

2d. One part of this solution, diluted with fifteen parts of water, in order to induce full dilatation, with transient immovability of the pupil, with a view to a full examination of the internal parts, in all directions. The dilatation ensues after thirty to forty-five minutes, and ordinarily in twenty-four hours, ceases to disturb the vision.

3d. The same solution, diluted with eighty parts of water, that is one part of sulph. atropini with 9600 parts of water; of this I make use in far the largest proportion of cases. One or two drops of this solution, held for a few seconds between the eyelids, causes, in thirty to sixty minutes, a dilatation sufficient for the examination of the greater number of eyes. The dilatation, however, is not so strong as perceptibly to injure vision, and in eight to thirty-six hours it has wholly passed away. I esteem it a great advantage in common cases, to make use of this dilute solution.—*F. C. Donders, in Nederlandsch Lancet*, Maart, 1854, p. 533.

M. RICORD'S OPINION OF THE COLLODION TREATMENT.

M. Ricord tried the collodion treatment, as proposed by Bonnafont (see p. 558), in thirty-eight cases of blenorrhagic epididymitis; and the following are the conclusions at which he has arrived:—

1*st*, Elastic collodion causes less suffering than ordinary collodion, but more than the other modes of treatment.

2*d*, It is not so efficacious a therapeutical agent as was imagined.

3*d*, It does not allay the pain, nor effect a cure more promptly than other modes of treatment.

4*th*, Its action is principally manifested on engorgement of the subscrotal cellular tissue, and on inflammation of the scrotum itself.

5*th*, It is only a feeble mode of exerting compression.

6*th*, If collodion only acts through the medium of the cold resulting from the evaporation of the ether; the application of simple ether, or of cold to the scrotum, would answer equally well.

7*th*, It is not rational to believe that collodion can cure orchitis and epididymitis, simply by protecting the diseased parts from the action of the air.—*L'Union Médicale*, 14th September 1854.

CATHETERISM OF CERVIX IN NEURALGIA OF UTERINE APPENDAGES. M. MALGAIGNE.

M. Malgaigne has given three cases illustrative of the beneficial effects of introducing the uterine sound into the cavity of the cervix, in cases of neuralgia of the uterus and its appendages. We give a brief *resumé* of one of them :—

Leonie G——, æt. 28, of nervous temperament, came under Malgaigne's treatment on the 23d May 1854. First menstruated at ten and a half years. Became pregnant at eighteen : had a premature labour in consequence of an accident, and was confined to bed for fifteen days. Had pains in loins and hypogastrium for three months thereafter. In end of 1853 these pains returned and were worst on left side : she had constant menorrhagia and some leucorrheal discharge at the same time. She recovered somewhat, but in April last the pains recurred with increased severity, and she entered M. Malgaigne's ward.

On admission, she complained of severe pain in the hypogastrium, and in both groins, especially the left, extending down the anterior and inner part of the thighs. The cervix was small, smooth, very painful near the orifice, especially at the left side. The uterus was anteverted, and painful to touch on the fundus. The uterine appendages were painful when pressed upon, either externally or per vaginum. There was slight leucorrhœa.

Malgaigne diagnosed anteversion with slight metritis, and neuralgia of cervix; and, above all, double neuralgia of the uterine appendages, which constituted the prominent feature of the malady.

M. LIMAUGE'S NEW METHOD OF DIAGNOSIS OF FISTULA IN ANO.

In cases of fistula ani, where the existence of an internal opening cannot be ascertained by the ordinary means, M. Limauge suggests the injection of tincture of iodine into the external orifice of the fistula, the operator having previously introduced his finger into the anus. On withdrawing the finger, an indelible stain of iodine is found upon it if the fistula has an internal opening, and by comparing the situation of the stain with the depth to which the finger has been inserted into the bowel, a tolerably accurate calculation of the height of the opening above the anus may be formed. The tincture of iodine is preferable to all other coloured liquids for this purpose, because however small may be the internal opening, a sufficient quantity of fluid will pass into the bowel to stain the finger; and moreover, the discolouration thus produced is permanent.—*Archives Belges de Med. Milit.* 1854.

ENEMATA OF SULPH. CUPRI IN HEMORRHOIDAL HÆMORRHAGE. M. TROUSSEAU.

A woman, æt. 38, pale, anæmic, and very weak from loss of blood from the rectum, entered M. Trousseau's wards. On examination the hemorrhage was found to depend upon piles. She was ordered injections of nitras argenti of various degrees of strength—from 20 to 40 centigrammes of the salt without any benefit. Enemata of ratanhy were next tried, but also unsuccessfully. M. Trousseau therefore ordered an injection of 50 centigrammes of sulph. cupri, dissolved in a glassful of water, to be administered daily. The hemorrhage gradually became less, and in eight days had quite disappeared. The patient was ordered chalybeates internally.—*Gaz. des Hôpitaux*, 5th August 1854.

HASBACH ON THE USE OF CREOSOTE IN NOMA.

In several severe cases of noma, Hasbach has effected a complete cure by the application of creosote with a hair-pencil. Immediately after its application he found a line of demarcation formed; the gangrenous portions were found

to separate, and the healing process to go on satisfactorily. He regards creosote as more efficacious in putrefactive conditions (*sepsis*) of the system, than in purely ulcerative affections.—*Organ. für d. Zesammt. Heilk. Hft.* 3, 1853.

ANGENSTEIN'S CASE OF VAGITUS UTERINUS.

The author had applied the first blade of the forceps to deliver a primiparous woman, and was in the act of introducing the second, when the child was distinctly heard to cry twice; and when the head alone was born, before the body was expelled, the child began to shout lustily.—*Org. für d. ges. Heilk. II.* 1. 1853.

[The occurrence of respiration and crying when the head only of a child is born is not very uncommon. Vagitus uterinus, although rare, has certainly been repeatedly heard; and it may be remarked that the use of the forceps must be favourable to its occurrence, from the blades separating the walls of the vagina, and facilitating the access of the cold air to the child's face. A long labour, too, requiring artificial aid, will, by its disturbing influence upon the placental circulation have, perhaps, produced to some extent the *besoin de respirer* in the child.]

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

Session xxxiv., 1854–55.

November 15, 1854. J. Y. Simpson, M.D., President, in the Chair.

Tuberculous Disease of the Bladder and Kidney.—*Dr Haldane*, Pathologist to the Royal Infirmary, exhibited the pathological appearances illustrative of this case, which, besides the rarity of the locality of the disease, was interesting, inasmuch as the sexual organs were found free from diseased action, a fact which was at variance with the law laid down by Rokitansky, viz., that in cases of tuberculous disease of the urinary apparatus, there has always been pre-existent affection of the sexual organs. The lungs were found tuberculous.

Dr W. T. Gairdner had seen the case during life, and stated that the symptoms had been very marked, and the sufferings of the patient very acute. He (Dr G.) had seen several similar cases without any primary disease of the sexual system, and he had observed, with other authors, that as the renal symptoms in these cases became developed, the pulmonic symptoms declined in severity, and the pathological changes underwent a corresponding remission. Similar remissions had been noticed as regards the testis and the serous membranes.

Case of Amputation of the Foot without Removal of the Astragalus. By John Traill, Esq., Arbroath.—The operation had been performed on account of caries of the os calcis, which, together with ulceration of the toes, had followed a severe injury which the limb had received several years before from machinery, and which had apparently done serious damage to the femoral vessels and nerves. The caries was believed to be limited to the os calcis, and on dissection afterwards, was found to be so. From the ulcerated condition of the integuments of the heel, lateral flaps were had recourse to; slight sloughing resulted, but recovery was complete in six weeks. A cast of the stump was handed round.

A short conversation ensued as to the relative merits of this operation and that of Mr Syme's, and the general feeling of the Society seemed to be, that

this new operation, notwithstanding the slightly increased length of limb which it afforded, offered no advantage over the older operation at the ankle.

NOTE ON THE THERAPEUTIC ACTION OF THE SALTS OF CERIUM. BY J. Y. SIMPSON, M.D.—The salts of few metals were at present in our pharmacopœia, and Dr S. saw no reason why many more might not be added to their number. He had recently drawn the attention of the profession to the salts of nickel, which, as far as his observation went, presented much similiarity in action to those of iron and quinine, and seemed to be of use in cases of sick headache. He now proposed to read to the Society some very imperfect observations on the therapeutic action of some other metals: and first, as to cerium, which, given in the form of nitrate, and in one grain doses twice or thrice a-day, appeared to act as a sedative tonic of considerable value, strongly resembling bismuth, and the salts of silver. He had employed it in the first instance in cases of general chronic intestinal eruption—a peculiar and intractable form of disease, for which arsenic and nitrate of silver were generally prescribed, and where these remedies had failed, cerium had been tried with marked advantage. In irritable dyspepsia, with gastrodynia and pyrosis, and in chronic vomiting, its exhibition was attended with satisfactory results; and in the vomiting which occurs during pregnancy prompt relief was afforded. It was a good tonic, and a useful substitute for the salts of silver, bismuth, and hydrocyanic acid. Dr S. had not employed it much in convulsive diseases, as chorea and epilepsy, in which nitrate of silver was used, but the exhibition of the salts of cerium was certainly attended with this advantage, that it could be persevered in without any fear of discolouration of the skin. As far as his experiments with cadmium went, it bore much resemblance to the preparations of antimony, and excited diaphoresis and vomiting. Tellurium, besides its expense, was precluded from being used in practice by its disagreeable effects. Dr S. mentioned the particulars of a case where a dose had been inadvertently given to a student of divinity, and had been followed by the evolution of such a persistent odour, that for the remainder of the session the patient had to sit apart from his fellow-students.

Dr Gairdner (who occupied the chair during the reading of Dr Simpson's communications), in the name of the Society thanked Dr S. for his very interesting paper. He (Dr G.), however, felt doubtful how far a medical man was justified in experimenting with new remedies, before their effects had been ascertained on some of the lower animals. This query he threw out merely in the way of suggestion, in the hope of hearing the views of Dr Simpson and the other members of the Society on this interesting subject.

Dr Sellar said, that it was now a long time since the pharmacopœia had received any addition from the metals. Bismuth, he (Dr S.) believed, was the latest, and he thought that the profession was indebted to Dr Simpson for having opened up such a new and promising field of investigation. The question as to the propriety of instituting these experiments now ceased to exist, as the experiments had been successfully made, whether on inferior animals or not. He (Dr S.) had little faith in bismuth or nickel, and as to tellurium, the less it was given the better.

Dr Simpson thought, he had been justified in carrying out these experiments, as, with the exception of arsenic, none of the metals were poisonous in smaller doses than half a grain, and, in the present instance, he had previously tried the remedies in similar doses on his own person. He might mention that the cost of cerium was one penny a grain, and that in London, nitrate of silver was charged at the same price.

CASE OF CÆSAREAN SECTION. BY J. Y. SIMPSON, M.D.—The previous history of this case is to be found in this Journal for 1847. At that time, though the conjugate diameter measured 3 inches and the tranverse under 1 inch, delivery was effected without any instrumental interference. The fœtus had died and become macerated in utero, and was a mere bag of tough skin

with the disarticulated bones showing here and there as hard points through it. Labour at the supposed full time again set in on the evening of Wednesday, June 21st 1854. Dr Simpson was only summoned late on the Friday evening, and did not reach Cupar till the morning of Saturday. The woman had now been 60 or 70 hours in labour, and was moribund. The child's heart could still be heard beating, and Cæsarean section was determined on, and performed without difficulty. The child, after extraction, sighed once or twice, but unfortunately gradually sunk; it weighed 2 lbs. 3 oz. The mother died in two hours. On dissection, a small quantity of blood was found in the peritoneum and cavity of the uterus. The body was much deformed. The pelvis measured at the brim $3\frac{3}{4}$ inches in the conjugate and transverse diameters, while at the outlet, the conjugate diameter was 3 inches, and the transverse at the narrowest point, half an inch. Dr S. recapitulated the history of several very deformed pelves from the collections of Hamilton, Kellie of Leith, and Dr Nimmo of Dundee, and which he showed to the Society. He had little doubt but that the child's life would have been saved in the present case had the operation been performed 12 hours earlier.

Dr M. Duncan desired to know, why neither abortion, premature labour, nor craniotomy had been performed in this well known case.

Dr Keiller asked Dr Simpson if the pelvis had undergone any change in size since the date of the previous confinement.

Dr Simpson said, that at the commencement of labour, a midwife had been in attendance, and that Dr Wiseman had not been called in till the labour had advanced. As to craniotomy, he (Dr S.) felt confident that the child could not have been pulled through such a contracted pelvis. Its size appeared to be the same as at the former period.

An interesting conversation ensued as to the prevalence of malacosteon in Scotland and England, in which several of the members joined.

Dr Mackenzie from Russia stated, that he had frequently seen the disease in Russia, but he had never heard of the performance of Cæsarean section for the disease in that part of the world.

Dr Storer, Jun., from Boston had known but of one case of Cæsarean section in his native town, but malacosteon was frequently met with in the Western States.

Injection of Iodine in Ovarian Dropsy.—*Dr Simpson* mentioned that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present, however, after the injection of a hydrocele. He (Dr S.) had now injected some 10 or 12 cases of ovarian dropsy; the disease had recurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only (if present) in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr Martin, in his paper on the cure of hydrocele by injection.

Cholera in Edinburgh.—*Dr W. T. Begbie* stated, that at present there were only four or five convalescents in the Cholera Hospital, and that during the last ten days, but one case had been admitted, and that was in the way of recovery.

THE LATE PROFESSOR EDWARD FORBES.

By this time the death of Professor Edward Forbes is known to the scientific world. The loss which the Medical Faculty of the University of Edinburgh has thereby sustained is, we fear, irreparable. The suddenness, and recent nature of the event, however, has prevented the possibility of our noticing it in

a manner commensurate with its importance, and with the extended reputation of our departed friend. This omission we shall endeavour to rectify in our next number. We think it now only important to say, that at a meeting of his friends, it was unanimously resolved to procure subscriptions, which are limited to L.1, 1s., for the purpose of having a marble bust of him executed, for which ample materials are in existence. Intending subscribers are requested to communicate with the treasurer, Professor Piazzi Smythe of the Edinburgh University, or with Messrs Blackwood and Black, publishers, by whom subscriptions will be received.

VARIETIES.

The Cockpit of a Man-of-War no Place of Safety.—In the recent naval attack upon Sebastopol, a shell entered the after cockpit of H. M. S. Albion. It lodged in the first lieutenant's cabin, burst there, destroying everything in that cabin and in the surgeon's cabin on one side, and another officer's on the other. Mr Mason, the surgeon, was himself so much bruised in the arm by one of the splinters, that he was disabled, and could not operate. A similar splinter from another shell struck the paymaster, who was acting as assistant to the medical officers, and bruised him severely.—*Dr Mackay in Medical Times.*

More English Law.—A London general practitioner, a Mr Sweetnam, was summoned late at night to the family of a Mr Read, at Islington. He was consulted by Mr Read, but also asked to prescribe for an infant three weeks old. As it was late when he left, he went to a neighbouring druggist, wrote two prescriptions, one for the father and another for the infant, and paid for the medicine. It was proved that the druggist by mistake inclosed a powder intended for the father with the lotion to be used by the child. Whereupon the mother gave the powder to her infant, of the effects of which it died. The judge stated that no blame could attach itself to Mr Sweetnam, but that as he employed the druggist, he was in the eye of English law accountable for his agent's acts. A verdict was therefore pronounced against Mr Sweetnam, damages L.6, 14s. with costs. Did such law as this prevail in Scotland, almost every town practitioner would have to wait in the druggist's shop and see his prescriptions made up, in order to protect himself from legal prosecution in cases of accident.

Clinical Lectures at length required by the London College of Surgeons.—It is now some years since the London College of Surgeons recognised the importance of anatomy and physiology being taught by two professors, although this has been done in Edinburgh ever since the foundation of its medical school. It has now adopted another of the excellencies of the Scottish metropolitan school, and required from its candidates attendance on clinical lectures, twice a week for nine months, partly medical and partly surgical.

A Blundering Translation.—Sir John Pringle, in his "Observations on the Diseases of the Army," mentions having cured a soldier of a violent scurvy, by prescribing two quarts of the *dog* and *duck* water, to be drunk every morning before dinner. In a French translation of this work, the remedy is specified to be two quarts of broth, made of a duck and a dog.—*Dublin University Magazine.*

Conclusion of the Directory War.—The suit in reference to the "London and Provincial Medical Directory," and the "British Medical Directory," has at length, through the intervention of mutual friends, been brought to an amicable termination in a manner satisfactory to both parties. In pursuance of this arrangement, the "British Medical Directory" will be merged in the

"London and Provincial Medical Directory," which by this combination will, we trust, be rendered well worthy the patronage of the profession, to whom we shall more readily recommend it, inasmuch as it will continue to be conducted on the principles so long advocated by us, of excluding the titles and honorary distinctions of homœopaths, and others of that stamp.—*Lancet.*

Want of Medical Officers for the Army and Navy.—Numerous letters from the seat of war continue to speak of the insufficiency of medical attendance, both at Sebastopol, and on board transport ships laden with wounded for the hospital at Scutari. To meet the emergency the London College of Surgeons has intimated its readiness to examine, in December, all those young men who have reached the age of 21, but whose studies would not otherwise have terminated until March. The inferior grade of Army apothecaries has also been had recourse to by Government. We deprecate both these proceedings, as injurious to the profession, and as tending to subject our soldiers and sailors to the inexperience of imperfectly educated men. It would be more becoming in our public bodies, instead of relaxing their regulations at this juncture, to impress upon Government the propriety and justice of raising the remuneration, and, above all, of honourably treating the medical men of both services. There would then be no difficulty in procuring, among such an overcrowded profession as that of medicine, abundant additions to the staffs both of the army and navy.

PUBLICATIONS RECEIVED.

A System of Instruction in Quantitative Chemical Analysis. By Dr C. Remigius Fresenius. Second edition. Edited by J. Lloyd Bullock, F.C.S. London. 1854. 8vo. Pp. 624.

The Use of the Blowpipe in the Qualitative and Quantitative Examination of Minerals, Ores, Furnace Products, and other Metallic Combinations. By Professor Plattner and Dr Sheridan Muspratt, F.R.S.E., etc. Third edition. London. 1854. 8vo. Pp. 405.

An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science, etc. By R. G. Mayne, M.D., etc. London. 1854. 8vo. Parts II. and III.

On the Topical Medication of the Larynx in Certain Diseases of the Respiratory and Vocal Organs. By Eben. Watson, A.M., M.D., etc. London. 1854. 8vo. Pp. 183.

Pathological and Surgical Observations, etc. By Henry Lee, F.R.C.S., etc. London. 1854. 8vo. Pp. 232.

A Disquisition on Certain Parts and Properties of the Blood. By David Tod, M.R.C.S., etc. London. 1854. 8vo. Pp. 263.

What to observe at the Bedside and after Death in Medical Cases. Second edition. 1854. 12mo.

A Manual of Pathological Anatomy. By C. Handfield Jones, M.B., F.R.S., etc., and Edward H. Sieveking, M.D. London. 1854. 12mo. Pp. 788.

The Anatomist's Instructor and Complete Guide to Anatomical Manipulation. By Frederick John Knox, Surgeon, etc. Edinburgh. 1854. 12mo. Pp. 152.

The Edinburgh Student's Guide to the University, Royal College of Surgeons, etc., etc. Fourth edition. Edinburgh. 1854. 12mo. Pp. 72.

Result of an Inquiry into the Invariable Existence of a Premonitory Diarrhœa in Cholera, etc. By David Macloughlin, M.D., etc. London. 1854. 8vo. Pp. 61.

The Scottish University System suited to the People. An Introductory Lecture. By the Rev. Philip Kelland, M.A., F.R.S.S., L. and E., etc. Edinburgh. 1854. 8vo. Pp. 15.

A Letter to the President of the Board of Managers of the Pennsylvania Institution for the Instruction of the Blind. By Robley Dunglison, M.D. Philadelphia. 1854. 8vo. Pp. 24.

The Micrographic Dictionary. By J. W. Griffith, M.D., and Arthur Henfrey, F.R.S., etc. London. 1854. 8vo. Part V.

Recollections of Europe in 1854. An Introductory Lecture. By Professor Robley Dunglison, M.D. Philadelphia. 1854. 8vo. Pp. 28.

The papers of Dr Martin Barry in our next. The Memoir of Mr Battye has been received.

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